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CONSILIENCE AND NATURAL KIND REASONING

(*in Newton's Argument for Universal Gravitation*)

1. STRENGTH *vs* SECURITY

In his ongoing debate with Clark Glymour and other scientific realists, Bas van Fraassen (e.g. 1983 pp. 165—168, 1985 p. 247 pp. 280—281 pp. 294—295) has often appealed to a widely accepted doctrine that *strength* and *security* are conflicting virtues which must be traded off one against the other. In limiting his commitment to only the empirical adequacy of a theory van Fraassen claims to be simply more cautious than his realist opponents. Certainly, as he delights in pointing out, a theory T cannot be more probable than its empirical consequences E. For Probability is monotone with respect to entailment, so $P(T) \leqslant P(E)$ if T entails E. If security is measured by any function which, like probability, is monotone with entailment, then it would seem that the kind of trade-off between strength and security van Fraassen appeals to is unavoidable.[1]

Upon hearing some of these debates it occurred to me that some celebrated cases from the history of science which involve unification under a theory provide *prima facie* counterexamples to the opposition between strength and security. Examples such as the unification of Galileo's terrestrial mechanics and Kepler's laws of planetary motion by Newton's theory of universal gravitation seem to be ones where accepting the stronger unified theory provides more security than would be obtained by accepting only the weaker hypotheses alone.

Imagine an acceptance context wherein the theoretical commitments include the approximate truth of Kepler's Laws of Planetary Motion and the approximate truth of Galileo's terrestrial mechanics. If we choose the approximations in a reasonable way then these commitments will be entailed by Newton's account of the system of the world in *Principia* Book III. Now consider a rival planetary hypothesis. Let it be a version of Tycho Brahe's system which has just the relation to Kepler's system that Tycho's original proposal had to Copernicus' system. According to such an hypothesis the six known primary planets

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all orbit the sun (just as they do for Kepler), but the sun (in its annual motion) orbits the earth. We now know that in so far as Kepler's system gives a correct account of the relative (annual) motions of the solar system from a reference frame on the sun (that is not rotating with respect to the fixed stars), so too will this Brahean system be a correct account of the relative (annual) motions of the solar system from a reference frame fixed at the centre of the earth (and not rotating with respect to the fixed stars). Now consider some data — the absence of stellar parallax. This data rightly counts against Kepler and for the Brahean account, since on the Keplerian account we have the diameter of the earth's orbit (two AU's) as a base for generating parallax.[2]

Now imagine an acceptance context in which the theoretical commitments include those of Newton's account in *Principia*. This is a context in which the theoretical commitments are stronger — the original commitments are entailed by this much richer account. Nevertheless, it seems clear that these commitments may reasonably have been regarded as more secure in the sense of more immune to revision. Once these richer commitments are accepted, Newton's dynamical argument at Proposition XII (using a centre of mass calculation to find an approximately inertial frame) can be used to support the Keplerian hypothesis against its Brahean rival (Stein 1967, pp. 177—184).[3] Given Newton's dynamical argument the proper response to the absence of stellar parallax data was to reason that the fixed stars are very much farther away than they had been thought to be.

Sometimes it is rational to hang on to one's theory in the face of putative counter-evidence. Our little thought experiment suggests that security in the sense of such immunity of our theoretical commitments, to revision in the face of putative counter-evidence can sometimes be legitimately increased by moving to accept a stronger theory. What it shows is that if only the weaker theoretical commitments are accepted the data about absence of stellar parallax might reasonably lead to rejection of Kepler (in favour of Brahe); but if the stronger Newtonian theory is accepted it would be legitimate to protect the theory by putting the blame on initial conditions (the fixed stars are much farther away than they had been thought to be).

This idea of immunity to revision in the face of hypothetical (or actual) evidence is one of a number of closely related conceptions of security that have begun to attract attention as philosophers of science have become aware that high probability is not the only conception of security we need to do justice to real life scientific reasoning.[4] I shall argue that a closer look at Newton's unification of terrestrial and celestial motion phenomena in *Principia* supports the suggestion that some such alternative conception of security does play a critical role in the evolution of science. I shall also argue that Newton's reasoning in support of universal gravitation is a vivid illustration of non-empiricist aspects of scientific practice that are quite widespread, reasonable, and perhaps even essential to science as we know it. As far as I know, no contemporary work by philosophers of science deals as adequately with some of these important but neglected aspects of the practice of science as the methodological work by William Whewell to which Robert Butts has for so long been calling our attention.

2. THE COLLIGATION OF FACTS INTO GENERAL PROPOSITIONS

In his analysis of the process of induction (Chapter V) of Book III of his *Novum Organon Renovatum* Whewell tells us,

> "... the colligation of ascertained Facts into general Propositions may be considered as containing three steps, which I shall term *the selection of the idea, the construction of the conception, and the determination of the magnitudes* (Butts 1968, p. 211).

He also tells us

> "*These three steps correspond to the determination of the* Independent Variable, *the* Formula, *and the coefficients, in mathematical investigations,* or to *the* Argument, *the* Law *and the* Numerical Data *in a Table of an Astronomical or other* Inequality (Aphorism XXXV, Butts 1968, pp. 210—211).

Malcolm Forster (1988) has recently interpreted Whewell's notion of induction as colligation of facts by explicating how these three steps work in a number of examples. One of these is the colligation of tidal data into the phenomenon that heights of highest tides vary sinusoidally with the angle from solar conjunction of the moon, so that spring tides (highest high tides) correspond to the two syzygies (0° = 360° and 180°) while neap tides (lowest high tides) correspond to the quadratures (90° and 270°). The selection of the idea is the selection of the angle of the moon from solar conjunction as the independent variable with respect to which to represent the approximately fortnightly cycle of spring and neap tides. The construction of the conception is the selection of a certain curve (a cosine curve) as that of the best fitting forumula representing the variation of heights of high tides with respect to this independent variable. Finally, the determination of the magnitudes is the determination of values of such coefficients of this curve as its amplitude and (of particular interest to Newton as we shall see) the numerical ratio between its high and low values.

It should be emphasized that what we are talking about here is as Whewell put it in our first quotation, "the colligation of ascertained facts into *general* propositions". The output of the three-step procedure is a generalization — the empirical law of the variation of neap and spring tides with the moon's angle from solar conjunction. It is this general proposition that counts as the primary tidal *phenomenon* which Newton (*Principia*, Book III, Proposition XXIV) explained by the joint action of the moon's and sun's gravity on our seas.

According to Whewell we extract such a general proposition from the mass of particular facts by imposing our idea and conception on the data.

> Thus in each inference made by induction there is introduced some General Conception, which is given, not by the phenomena, but by the mind (Butts 1968, p. 141).

He points out that the task of finding an illuminating idea (independent variable) and an appropriate conception (formula) requires sagacity and often also great effort and even luck. Anyone who doubts this should consider the difficulties faced, and the problems overcome, in Kepler's great struggle to master the motion of Mars.[5] He called this his war on Mars. It ended triumphantly with his successful extraction of the elliptical orbit law and the law of areas for Mars from the mass of data made available by Tycho Brahe's extensive and careful line of sight observations. Kepler's extraction of these two laws for Mars, his extension of them to the other planets and finally his extraction of his harmonic law some years later are all examples cited by Whewell (Butts 1968, pp. 130, 141, 142) as colligations of facts into general propositions.

It is important to note that once the idea is selected and an appropriate formula constructed the magnitudes of the coefficients are fixed (up to appropriate limits for error) by the data. Having settled upon an elliptical orbit with the sun at one focus the data fix the magnitudes of the greatest and least semi-axes and the period of the orbit, therefore they settle such interesting magnitudes as the average centripetal acceleration of the planet.

The idea or independent variable depends on what we choose to consider. Some choices are more illuminating than others and we shall defer discussing what Whewell has to say about evaluating such choices. For now I want to say something about Whewell's account of special methods of induction available in quantitative investigations to constrain the construction of an appropriate formula once the independent variable has been chosen. Whewell offers a very interesting account of how methods such as curve fitting, and the methods of means and least squares facilitate finding generalizations that might otherwise lie hidden among the errors and secondary effects which infect the data (Chapter VI, Book III of *Novum Organon Renovatum* (Butts 1968, pp. 223–237). Forster (1988) suggests that a proper appreciation of how such methods can average out errors (especially in large data sets such as the 13 000 observations extending over 19 years Whewell cites [Butts 1968, p. 233] for Lubbock's first investigation of the tides at London) will lead us to interprete Whewell's controversial claim that the result of a colligation can be 'more true than the individual facts themselves' (Butts 1968, p. 227) in a way that makes it both interesting and plausible.

Forster (1988) also explains how Whewell's method of residues can be used to separate out effects of component forces. On Forster's account Kepler's laws are true descriptions of that part of the motions of the planets due to the gravitation of the sun. Forster argues quite plausibly that when one considers them as generalizations revealed by averaging over a great mass of data the Keplerian effects are there to be found, even though mutual perturbations among the planets are also there to be found in the data.[6] Perturbations such as those resulting from the mutual action of Jupiter and Saturn on each other near conjunction can be revealed when the method of residues is applied to colligate laws about the various deviations from the Keplerian average values.[7]

I may have left the impression that data do not put any very strong constraint on our choice of formulae. That this impression is wrong should be clear from Forster's example of the tides. Once the independent variable is fixed any appropriate formula has to fit the general trend correlating spring tides with syzygies and neap tides with quadratures. Our choice of formula may be a conception we impose to go beyond the data, but it is a conception we construct to fit the data. Our scope for free invention in constructing this conception is significantly constrained by the data even if the data do not uniquely determine the correct formula.

3. WHEWELL's TESTS OF HYPOTHESES

According to Whewell, our choice of a formula in a colligation is not *just* a construction made to fit some given data. It is also an hypothesis subject to empirical tests. Whewell devoted a section of his chapter on scientific induction (Section III, Chapter V, Book II, NOR) to tests of hypotheses. Here are the subsection headings for this section from his table of contents (NOR, pp. XV—XVI):

Section III *Tests of Hypotheses*
Art. 10 True Hypotheses Foretell Phenomena
Art. 11 Even of Different Kinds — Consilience of Inductions
Art. 12 True Theories Tend to Simplicity
Art. 13 Connexion of the Last Tests

These headings suggest that Whewell distinguishes three tests of hypotheses, but that he sees a special connection between the last two of them.

3.1. *True Hypotheses Foretell Phenomena*

After setting the stage with a discussion of hypotheses which may be useful even though they turned out to be false (in article 9), Whewell opens article 10 with the following introduction to his first test.[8]

> 10. Thus the hypotheses which we accept ought to explain phenomena which we have observed. But they ought to do more than this: our hypotheses ought to *foretell* phenomena which have not yet been observed; at least all phenomena of the same kind as those which the hypothesis was invented to explain. For our assent to the hypothesis implies that it is held to be true of all particular instances. That these cases belong to past or to future times, that they have or have not already occurred, makes no difference in the applicability of the rule to them (Butts 1968, p. 151).

Whewell also devoted one of his aphorisms on induction to this first test

> It is a test of true theories not only to account for, but to predict phenomena (Aphorism XII, Chapt V, Butts 1968, p. 138).

He illustrated the application of this test by noting a number of examples of useful theories which eventually failed to meet it. These include what he called the "Epicyclical Theory of the Heavens", Newton's hypothesis of Fits of Easy Transmission and Easy Reflection and the doctrine of Phlogiston (Butts 1968, pp. 151—152).

I want to consider how this test applies to Kepler's so-called 'vicarious theory' of the orbit of Mars. This theory was Kepler's intitial attempt.[9] It is a classic example of an hypothesis that was useful, but discovered to be false. Kepler has left us a detailed account of the episode (in the second part of *Astronomia Nova*). We know exactly what data were used to construct it, what data verified it and what it could be relied upon to predict. We also know how Kepler refuted it.

For our purposes it may be sufficient to point out that the theory specified an eccentric (off-centre) circular orbit with an equant point (point about which equal angles are swept out in equal times) on the aphelion-line (line from the sun through the centre. It meets the circle on the far side at the aphelion, the point most distant from the sun. On the near side it meets the circle at the perihelion, the point closest to the sun). The equant point was to be on the side of the centre opposite the sun so that the planet would move fastest when it was closest to the sun. These specifications can be regarded as the conception in a Whewellian colligation. Kepler included two key geometrical parameters among the magnitudes to be fixed by the data. These were the distance between the sun and the centre as a fraction of the radius (the eccentricity) and the distance between the sun and the equant point as a fraction of the radius. Two more parameters, the direction from the sun of the Aphelion and the mean anomaly (time from aphelion as a proportion of the planet's period) of any one observation of the direction of Mars from the sun would complete this theory's specifications of the variation of Mars' direction from the sun with time (Stephenson 1987, p. 42).

The angular distance of the projection onto the plane of the ecliptic of the line from the sun through Mars from the point on the zodiac marking the vernal equinox is called its heliocentric longitude. About every 780 days Mars and the sun are in opposition (exactly on opposite sides of the earth). At such a time the observed location of Mars against the fixed stars in the zodiac fixes its heliocentric longitude. Kepler had twelve such observations to work with (ten from Tycho and two more he made himself). He was able to use four of them to fix the geometrical parameters (see Stephenson 1987, pp. 42—43). He had mean-anomalies for all so he was able to complete the theory when he managed to specify the heliocentric longitude of Mars' aphelion (Small, pp. 185—187). He then used the eight additional opposition observations to test the theory. Kepler concluded from these tests (the observed heliocentric longitudes were well scattered about the zodiac) that the theory could be relied upon to predict heliocentric longitudes to within about two minutes of arc, which was about the accuracy of the observations.

He went on to give two different refutations (Small, pp. 189—196, Stephenson, pp. 45—49) of the theory each of which used two observations, one near perihelion and one near aphelion, to constrain the ratio of the sun-aphelion distance to the sun-perihelion distance (Wilson, p. 92, diagram on p. 102). This contradicted the value of the eccentricity parameter (ratio of sun centre distance to radius) that had been fixed by the original four longitudes. Kepler was able to show that if the eccentricity were fixed to be consistent with the sun-Mars distances then any way at all of fixing the equant point would make for errors of up to eight minutes in some longitudes. This was sufficient for Kepler to reject this type of theory. The original version, however, continued to be a useful hypothesis for predicting longitudes. Indeed, Kepler continued to use this "vicarious" theory to make such predictions long after he had given it up as a candidate for the true account of Mars' orbit.

When we apply Whewell's first test to this episode we can surely count the four heliocentric longitudes used to fix its parameters as phenomena it was invented to explain. Perhaps, the eight heliocentric longitude observations used to verify it can count as successful predictions. They established its usefulness as a predictor of heliocentric longitudes. What about the solar-Mars distances that refuted the theory? If they are also phenomena of the same kind as those it was invented to explain then we can say that (after some initial success) the vicarious theory failed Whewell's first test. Suppose, however, we consider only the four longitudes used to construct the theory as the phenomena it was *invented* to explain and count only other heliocentric longitudes of Mars as phenomena of the same kind. On this way of settling what are to count as phenomena of the same kind as those it was invented to explain the vicarious theory did not fail Whewell's first test. It was a reliable predictor of heliocentric longitudes. In addition to the observations used to explicitly verify it, we can count later successful predictions when Kepler continued to rely on it to calculate longitudes.

We see here that Whewell's prediction test depends on how we specify which data are to count as phenomena of the same kind. Whewell's other examples show a similar relativity of the application of this first test to how data are sorted into kinds of phenomena. What he says about the Epicyclical Theory has an especially close parallel with the episode of Kepler's "vicarious" theory.

For example, the *Epicyclical Theory* of the heavens was confirmed by its *predicting* truly eclipses of the sun and moon, configurations of the planets, and other celestial phenomena; and by its leading to the construction of Tables by which the places of the heavenly bodies were given at every moment of time. The truth and accuracy of these predictions were a proof that the hypothesis was valuable, and, at least to a great extent, true; although as was afterwards found, it involved a false representation of the structure of the heavens (Butts 1968, p. 151).

In both cases the successful predictions are of line of sight observations, and in both cases the problem is a false representation of the structure of the heavens, specifically wrong relative distances.

In all of these examples Whewell cites as cases where an initially useful hypothesis failed this first test, the original regularities the hypotheses were actually constructed to fit continue to hold, at least more or less, even today. Whewell never provides a case where such regularities just stopped holding, as would be the case, for example, if the general trend correlating spring tides with syzygies and neap tides with quadratures were to begin to go radically wrong. Nor does he show us cases where we have discovered that we were radically mistaken from the beginning about such putative empirical regularities.

If we stick with a narrow interpretation of what count as phenomena of the same kind, then each of these examples is a case where the rejection of the hypothesis resulted from a failure to predict phenomena of a kind different from those it was invented to explain. This would make the rejection a failure of Whewell's second or third tests rather than this first test. Some ambiguity about this on Whewell's part may be shown by the fact that he discusses exactly these same cases as examples where it is the third test which fails to be met — what this third test demands is convergence to simplicity and unity as the hypothesis is generalized to explain phenomena of kinds different than those it was constructed to fit (Butts 1968, pp. 156—157).

We can see that Kepler's elliptical orbit unifies both heliocentric longitudes and sun-Mars relative distances so that it is superior to the vicarious theory. This may suggest that the vicarious theory fell because it was displaced by this superior rival, but such a suggestion is incorrect. Kepler rejected the vicarious theory before he had developed any hypothesis that could successfully account for both heliocentric longitudes and relative distances. Kepler was committed to finding a theory that could give a true account of the motions of the heavenly bodies among themselves. He intended the vicarious theory to account for relative distance phenomena as well as the heliocentric longitudes that were used to construct it.

A tradition in astronomy going back, at least, to Ptolemy treated astronomical theories as geometrical devices for predicting eclipses, eclipse magnitudes, and geocentric line of sight locations of heavenly bodies with respect to the fixed stars. These are the phenomena that Whewell (quoted above) pointed out that the epicycle theory could correctly predict. On this mathematical tradition the only constraints on an astronomical theory were that it *save these phenomena* and provide convenient methods of predicting them. Kepler can be regarded as a pioneer in a new physical-astronomy in which the aim is to give a true account of the motions of the heavenly bodies and not just to predict the geocentric line of sight data.[10]

Whewell offers some interesting remarks about the role played by advances in measurement in making new phenomena count as kinds that an adequate theory must account for (Butts 1968, pp. 156, 157). Tycho Brahe's observations made it possible to give much more accurate measurements of relative distances by triangulation than had been available when the line of sight angles were less closely fixed by the data. The use of the telescope and especially of the micrometer to give, by Newton's day, really precise data about relative visual angles occluded by bodies made an even more impressive increase in the availability of accurate relative distance data (van Helden 1985, pp. 118—156). Similarly, Whewell points out (Butts 1968, p. 157), it was the use of the balance in chemistry that made available the data about weights that led to the rejection of the phlogistan doctrine (though here, it appears, the rejection came only after the superior rival oxygen theory was developed).

Most of Whewell's discussion of his first test was devoted to its use to falsify hypotheses; however, in the last paragraph before moving on to discuss the much greater force of the evidence in favour of an hypothesis provided by its predicting phenomena of a different kind from those it was designed to fit, Whewell does acknowledge that successful predictions do count as positive evidence in favour of an hypothesis. This suggests that he would regard the accumulating mass of data points which the tidal correlation continues to fit as a continuing increase in its evidential support.

3.2 (*Predicting*) *even Phenomena of Different Kinds — the Consilience of Inductions*

Whewell introduces his second test of hypotheses with the following passage, which opens article 11, Chapter V:

> 11. We have here spoken of the prediction of facts *of the same kind* as those from which our rule was collected. But the evidence in favor of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from *that* being the point where truth resides.
>
> Accordingly the cases in which inductions from classes of facts altogether different have thus *jumped together*, belong only to the best established theories which the history of science contains. And as I shall have occasion to refer to this peculiar feature in their evidence, I will take the liberty of describing it by a particular phrase; and will term it the *Consilience of Inductions.*

This test also has one of Whewell's aphorisms on induction devoted to it.

> The Consilience of inductions *takes place when an Induction, obtained from one class of facts, coincides with an Induction, obtained from another different class. This consilience is a test of the truth of the Theory in which it occurs.*
> (Aphorism XIV, Chapter V NOR, Butts 1968, pp. 138—139)

Let us put aside, for the present, Whewell's claims about the forcible character of the evidence consilience provides, and look to these passages to find out what consilience is.

The first part of this passage suggests that an induction has the virtue we are trying to explicate

> when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis.

The main emphasis, however, appears to be on a coming together of two or more different inductions and the term that actually gets introduced in the definition at the end is "consilience of inductions". The Aphorism, similarly, characterizes the coming together of different *inductions*. Here is how Malcolm Forster explicates what these passages say about the sort of coming together that counts as a consilience of inductions:

> Here we must recall Whewell's insistence that every colligation of facts adds a new *conception* to the facts. Now, if we consider two separate domains of inquiry, then the colligation of facts within one domain will introduce conceptions unconnected (or not thought to be connected) with the conceptions introduced in the second domain. When these different inductions, i.e. colligations of facts, "jump together", Whewell means that the magnitudes independently measured within separate domains agree with one another, or are connected by some law-like regularity (i.e. connected by some formula). (Forster 1988, p. 74)

On this explication the consilience of inductions takes place when a higher level induction colligates the results of two or more lower level inductions.

Forster's claim that each case of the consilience of inductions is a higher order induction is, surely, a correct explication of Whewell's view, as the following passage indicates:

> For when the theory, either by the concurrence of two indications, or by extension without complication, has included a new range of phenomena, we have, in fact, a new induction of a more general kind, to which the inductions formerly obtained are subordinated, as particular cases to a general proposition (Butts 1968, pp. 159—160).

The phrase "concurrence of two indications" refers to consilience, while the phrase "by extension without complication" refers to the tendency toward unity and simplicity which is the virtue Whewell discusses in his third test of hypotheses. Here we see that each of these two virtues is realized by a higher order induction to which the lower level inductions it brings together are subordinated.

Forster's contention that by "consilience" of two or more inductions Whewell means just that their magnitudes have been colligated by a higher order induction — that, in consequence, any such higher order colligation counts as a consilience of inductions — is perhaps more controversial. Whewell tells us that the cases colligated have to be of different kinds. According to Forster, the absence of the higher-order colligation shows that they count as different kinds at least in the respects which are relevant to the case. This line is attractive even if it means admitting as examples of consilience many higher order colligations which do not have some of the impressive characteristics of Whewell's favorite examples.

Kepler's third law is a functional relationship among the magnitudes measured from the several applications of his first law to planets orbiting the same primary. According to this Law the ratio R^3/T^2 is constant, where R is the major semi-axis of the elliptical orbit and T is its period.[11] On Forster's account this shows a consilience of the different inductions colligating each of these planets' motions into its own elliptical orbit.

Whether or not this is something Whewell would count as a consilience of inductions depends upon whether or not we can find an appropriate respect in which these separate elliptical orbits can be regarded as phenomena of different kinds. I think we can reasonably follow Forster's suggestion here and take the absence of knowledge of any such functional relation among them as is specified by the third law as an exactly appropriate respect in which they were regarded as of different kinds before and as of the same kind afterward. If this is viable, then, on Forster's interpretation, consilience exhibits exactly what Larry Laudan suggested is its real virtue.

> The real strength of such an hypothesis is usually that it *shows that events previously thought to be of different kinds are, as a matter of fact the 'same' kind of event.* (1971, p. 374)

This historical relativity of what count as appropriately different kinds of phenomena is in line with Whewell's grand historical view of induction on which inductions are represented as stages in hierarchically organized tables representing the progress of science toward the unification of different kinds of phenomena. It suggests that the sorting of phenomena into kinds relevant for any stage in the development of science is determined by the colligations which have been achieved by that stage.

3.3. *True Theories Tend to Simplicity*

Here is how Whewell introduced his third test of true hypotheses:

> 12. In the preceding Article I have spoken of the hypothesis with which we compare our facts as being framed *all at once*, each of its parts being included in the

original scheme. In reality, however, it often happens that the various suppositions which our system contains are *added* upon occasions of different researches.

This being the mode in which theories are often framed, we have to notice a distinction which is found to prevail in the progress of true and false theories. In the former class all the additional suppositions *tend to simplicity* and harmony; the new suppositions resolve themselves into the old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our system. Different members of the theory run together, and we have thus a constant convergence to unity. In false theories, the contrary is the case. The new suppositions are something altogether additional; — not suggested by the original scheme; perhaps difficult to reconcile with it. Every such addition adds to the complexity of the hypothetical system; which at last becomes unmanageable, and is compelled to surrender its place to some simpler explanation (Butts 1968, p. 155).

This third test is one which applies to the process of theory construction. Whewell tells us that as the various suppositions are added in the course of its development a true theory tends toward greater simplicity and unification, while a false theory degenerates into complications until it becomes so unmanageable that it is compelled to surrender its place to some simpler explanation.

Robert Butts has long argued that one ought to take seriously Whewell's contention that his account of induction is to be construed as an account of discoverer's induction (1968, p. 20; 1973, pp. 56—57, 66—70). Whewell's discussion of this third test suggests that Butts has been on the right track in looking to the context of discovery for illumination of these views. In his important paper on Whewell's logic of induction (Butts 1973, p. 68) he called attention to an interesting passage at the beginning of section 13 where Whewell discussed the close connection between consilience and the explanatory virtues of convergence toward simplicity and unity exhibited in successful theory building.

13. The last two sections of this chapter direct our attention to the two circumstances, which tend to prove, in a manner which we may term irresistible, the truth of the theories which they characterize: — the *Consilience of Inductions* from different and separate classes of facts; — and the progressive *Simplification of the Theory* as it is extended to new cases. These two characters are, in fact, hardly different; they are exemplified by the same cases. For if these Inductions, collected from one class of facts, supply an unexpected explanation of a new class, which is the case first spoken of, there will be no need for new machinery in the hypothesis to apply it to the newly-comtemplated facts; and thus, we have a case in which the system does not become

more complex when its application is extended to a wider field, which was the character of true theory in its second aspect. The consiliences of our Inductions give rise to a constant convergence of our Theory towards Simplicity and Unity.

Butts argues that this close connection Whewell claims between consilience and the theory-building virtues of simplicity and successive generalization shows that consilience is more a pointer toward explanatory excellence than a measure of either entailment content or corroboration (1973, p. 69).

Butts also suggests (1973, p. 68) that for Whewell the concepts of consilience, simplicity, and successive generalization mean the same thing. Whewell does tell us that Consilience and Simplicity are hardly different, and that they are exemplified in the same cases; but he does not tell us that they mean the same thing. I think Forster (1988) has shown that Butts' main point — the importance of the context of theory building as the arena where consilience operates — can be even better served by distinguishing consilience from these explicitly explanatory virtues and then explicating the last sentence of the passage — where Whewell tells us that the consiliences of our inductions give rise to a constant Convergence of our theory towards Simplicity and Unity.

According to Forster the bottom-up character of Whewell's philosophy of scientific discovery is the key to understanding how it is that the consilience of inductions gives rise to convergence toward simplicity and unity.

However, Whewell's emphasis is on the philosophy of scientific discovery, whereas the top-down emphasis of hypothetico-deductivism obscures the most important component of this process — the *discovery* that two, or more, coefficients invented to explain diverse phenomena actually represent a common "cause". This feature is given its proper emphasis in the "bottom-up" approach to the philosophy of science.

Whewell has provided roughly the following picture of science. At the phenomenological level a colligation of facts will generally introduce the means of measuring formula coefficients, which may be instances of variable quantities when viewed more globally. These 'theoretical' variables may themselves enter into higher-level colligations, which may then introduce further coefficients and so on. This hierarchical procedure will eventually end when the coefficients introduced are no longer variable — as in the case of fundamental physical constants — or when the coefficients introduced are easily interpreted as conversion factors between different scales of measurement. (Forster 1988, pp. 76—77)

On this view the bottom-up character of Whewell's philosophy of discovery is shown by the step by step progression through the history of science toward increasing simplicity and unification. These are what

Butts (1973, p. 68) calls the steps of successive generalization exhibited in Whewell's inductive tables. Each step is guided from below by correlations colligated among the magnitudes measured in lower level colligations.

Forster's remarks continue with a simple illustration from laboratory physics of the way consilience can facilitate convergence toward simplicity and unity.

> Examples are everywhere; e.g. consider the magnitude of 'mass' as measured from balance phenomena or from spring phenomena. In cases like this, the observed correlation between independent measurements is *explained* in terms of an *identity* relation: The independently determined values are approximately the same (after being converted to a common scale) *because* they are measurements of the *same* thing, e.g. of 'mass', rather than measurements of the different properties of 'spring mass' and 'balance-mass'. The importance of these explanations is that they give rise to a simplification and unification of our previous theoretical committments (e.g. by replacing 'spring-mass' and 'balance-mass' with 'mass') (1988, p. 77).

In later remarks Forster expands upon this theme in a specific comment on the passage from Whewell we have been considering.

> Consilience is not the same as simplicity; the former merely gives rise to a *convergence towards* the latter. The goal — of simplicity and ontological unification — is only *achieved* once the observed consilience is deemed to be non-accidental and is subsequently *explained* in terms of an identity. Consilience is the evidence for accepting the identity. After that the theory can be further tested by its deductive consequences, but it is the *discovery* of the identities and unity in nature that is the admirable part of the process (Forster 1988, p. 79).

On this view the consilience of inductions gives rise to the convergence toward simplicity and unity by providing the evidence for introducing an identity. The agreement between the separate values is explained by a common cause — they are measurements of the same magnitude.

We have already pointed out that, on Forster's view, Kepler's third law is, itself, an example of consilience even if it doesn't directly illustrate the level of the explanatory virtues of convergence to simplicity and unity associated with Whewell's favorite examples where consilience is exhibited in grand unifying theories. The functional relationship colligated in Kepler's third law tells us that the ratio R^3/T^2 is constant for all of the planets orbiting the sun. On Forster's view this gives rise to convergence toward simplicity and unification when these approximately equal values can be explained by introducing an identity so that each can be regarded as a separate measurement of the same magnitude. This is just what Newton did. The equal ratios R^3/T^2 (suitably adjusted for scale) calculated from these various orbits can be understood as separate measurements of what Newton (Definition VI, Book I) called the absolute quantity of a common inverse square centripetal force holding all these planets in their orbits. He went on to establish that these could be understood as measurements of the mass of the sun. This magnitude can be regarded as the common cause of the ratio's characterizing the orbits of all the planets.

4. NEWTON'S ARGUMENT FOR UNIVERSAL GRAVITATION

4.1. *Phenomena*

Newton begins by citing six phenomena. The first specifies that Jupiter's moons satisfy Kepler's Law of Areas (by radii to the centre of the primary they describe areas proportional to the times of description) and Kepler's Harmonic Law. The second specifies that the moons of Saturn also satisfy these two laws. The last, Phenomenon VI, specifies that Kepler's Law of Areas is satisfied by our moon as well. The third, fourth, and fifth are of special interest to us.

Phenomenon III

That the five primary planets, Mercury, Venus, Mars, Jupiter, and Saturn, with their several orbits, encompass the sun.

Phenomenon IV

That the fixed stars being at rest, the periodic times of the five primary planets, and (whether of the sun about the earth, or) of the earth about the sun, are as the 3/2th power of their mean distances from the sun.

Phenomenon V

Then the primary planets, by radii drawn to the earth, describe areas in no wise proportional to the times; but the areas which they describe by radii drawn to the sun are proportional to the times of description.

Two points, rather immediately, strike an attentive reader. First, these phenomena do not specify that any of these orbits are ellipses. Secondly, they leave open our Brahean alternative to Kepler's account. Whether the earth orbits the sun, as Copernicus and Kepler specify, or the sun orbits the earth, as in the Brahean hypothesis, is not decided by what Newton takes as phenomena.

We saw that Whewellian colligations are general propositions colligated from the data by imposing a conception on them. Each of these phenomena cited by Newton is such a general proposition giving what purports to be an empirical law of the relative motions of a satellite (or satellites) with respect to its (their) primary. Another feature we noted about Whewellian colligations is that they are constrained to fit the data they colligate, even if they go beyond what these data logically imply. Newton discusses the evidence for each phenomenon. I think these discussions make it clear that the idealizations introduced by the conceptions which generalize on the data are fairly well within what would count for Whewell (or even contemporary statisticians) as reasonable estimates of fit.[12]

Not just any colligations count as phenomena. I think we can say, roughly, that only those colligations that can be reasonably regarded as presumed common-knowledge among appropriate experts are to count. Moreover, this presumption is defeasible in that corrections are allowed for. Professor I. B. Cohen (conversation, November 1987) suggested to me that G. E. L. Owen's (1961) interesting discussion of Aristotle's *Tithenai Phainomena* can illuminate the role played by Newton's phenomena. According to Owen (pp. 170—173) Aristotle's *Tithenai Phainomena* are the common conceptions on the subject accepted by all, or most, or by the wise. They are not rock hard data, but starting points that can be corrected as the investigation proceeds.

4.2. *Deductions from Phenomena*

In Proposition I, Book III, Newton infers that the moons of Jupiter (and Saturn)

(a) are held in their orbits by forces directed to the centre of their primary

and

(b) that these centripetal forces vary inversely with the square of the distance from their centre,

from the phenomena that they satisfy (a′) Kepler's Law of Areas and (b′) Kepler's Harmonic Law. In Proposition II he infers by similar arguments that the five primary planets are also held in their orbits by centripetal forces that vary inversely with the square of the distance from the centre of their primary. These are the paradigmatic examples of Newton's practice of inferring forces from phenomena.

(a) *Equivalence Theorems and Bootstrap Confirmations.* In each case the inference is backed up by theorems establishing equivalences between values of a phenomenal magnitude (say the behaviour of the rate at which areas are being swept out by radii to a centre as increasing, constant, or decreasing) and corresponding values of a theoretical magnitude of interest (say the direction of a deflecting force as off centre in the direction of tangential motion, centripetal, or off centre in opposition to the tangential motion). The assumptions used in the proofs allow for a range of possible values for the phenomenal magnitude, and equivalences with corresponding values of the theoretical magnitude are proved to hold over the whole range.

These equivalence theorems show that Clark Glymour (1980, pp. 203—225) was correct when he argued that Newton's deductions of forces from phenomena satisfy the requirements for bootstrap confirmations. A bootstrap confirmation is an inference where an instance of a theoretical claim is deduced from data together with theoretical background assumptions. The basic requirement is that the background assumptions be compatible with alternative data which would have led to a contradiction of the instance of the theoretical claim that was deduced from the actual data. Newton's deductions of forces from phenomena, however, are especially interesting special cases of bootstrap confirmations. Not only are the background assumptions compatible with alternative values of the phenomenal magnitude which would have contradicted the theoretical claim that was actually inferred, as bootstrap confirmation requires, but these background assumptions also entail the theorems which give the equivalences between the various values of the phenomenal magnitude and corresponding values of the theoretical magnitude over a whole range of alternative values which they leave open. This suggests that (given the background assumptions) the values of the phenomenal magnitudes can be regarded as *measuring* the corresponding values of the theoretical magnitude just as readings on a thermometer measure temperatures. Given Newton's theorems the motions of orbiting bodies can be regarded as measurements of directions of deflecting forces, and for cases where these are centripetal forces the motions can measure the variation of those forces with power of distance.

(b) *The Dynamical Significance of the Law of Areas.* Theorems I and II of Book I give the two directions of the equivalence between centripetal direction of a force deflecting a body from uniform rectilinear motion and constant rate for sweeping out areas by radii drawn to that centre, while Corollary I of Theorem II, Book I gives the relevant equivalences for increasing and decreasing rates. These theorems show that motions satisfying Kepler's Law of Areas with respect to a centre moving inertially measure the centripetal direction of a force deflecting the body from uniform rectilinear motion with respect to that centre. Theorem III extends this result to centres, howsoever moved, so that motions of a body satisfying Kepler's Law of Areas with respect to the centre of another body measure a force compounded of a centripetal force together with all the accelerative force by which that other body is impelled.

This shows that the dynamical significance Newton finds for Kepler's Law of Areas is not limited to idealizations where the centre counts as in inertial motion and the only force operating is the centripetal force deflecting the body. Rather, as Forster points out, Newton shows how relative motions satisfying Kepler's Law of Areas let us infer centripetal component forces even in the presence of other unknown accelerating forces.

This virtue of the theorems backing up Newton's deductions of centripetal forces from phenomena suggests that Forster's emphasis on the use of residues in large masses of data to make room for decomposing effects into components from which to infer component causes may not always be necessary. Such deductions of component centripetal forces from appropriate relative motions will work even when there are no multiple data points corresponding to repetitions of the motion. This is important for Newton's applications to motions of comets where there might be just a few observations on a single fly past of the sun. Newton's equivalence theorems make it possible to infer a component centripetal force from *even a single case* of a motion where equal areas are swept out in equal times by radii to a centre.

4.3. *Newton's Mathematical Theory of Forces and Motions*

Howard Stein (conversation) once called my attention to the fact that Newton places the laws of motion together with the definitions in a separate section before the actual beginning of Book One. Newton actually tells us (Scholium to the Laws translation [1987] by I. B. Cohen and Anne Whitman) that these principles are "accepted by mathematicians and confirmed by many kinds of experiments." They are assumptions we readers are expected to agree to before we are to even begin working our way through his rich investigation of the motions of bodies. These laws together with Euclidean geometry, and the lemmas on first and last ratios (which do for Newton what we would do with the calculus) are the general background assumptions which make possible Newton's deductions of forces from motion phenomena.

We saw that the deductions from phenomena we considered were special bootstrap confirmations because they were backed up by equivalence theorems that let us measure the forces from the phenomena. Here we see that they are special because the background theory is special — the very general assumptions of Newton's mathematical theory of forces and motions. In "On the nature of the truth of the Laws of Motion" (Butts 1968, pp. 79—100) Whewell held that the laws of motion were necessary truths even though they also had empirical elements. Butts (1965, especially pp. 169—173) has offered an interesting account of the interplay of empirical and *a priori* elements in Whewell's account of how we come to have a clear intuition of the necessity of the laws of motion to the application of the idea of cause construed as force.

Consider the situation with kinematics. The vector composition of motions according to Galiliean transformations were explicitly taken by Kant and Hertz as *a priori*. Moreover, I doubt that many scientists or philosophers from Newton's day through most of the 19th century would have regarded them as empirically refutable. Nevertheless, when Poincare developed an alternative conception that could take over their role in making intelligible the composition of motion and forces and Einstein used it to formulate special relativity theory, few had any difficulty understanding certain experiments as empirical refutation of Galilean kinematics. Putnam (1983, p. 95) coined the nice term "contextually *a priori*" for the special status I claim was enjoyed by kinematics. In the earlier context, when no alternative conception was available to make intelligible the compositions of motions, the Galilean transformations were treated as immune to any empirical refutation. When an alternative conception was developed enough to count as a serious rival the context changed and the earlier conception became open to empirical test.

4.4. *Maintaining Simplicity in the Face of Hard Tests: Proposition 3 and the Lunar Precession Problem*

In Proposition 3 Newton argued that the moon is held in its orbit by an inverse square centripetal force. That the force is centripetal is deduced from Phenomenon VI by the Areas Law theorem. The argument to establish the inverse square variation was more problematic. What made it a problem was the well established precession of the lunar apsides of about 3° forward every revolution.

Newton appealed to Corollary 1 of Proposition XLV, Book I, which establishes equivalences between precession of aphelion for a body orbiting under a centripetal force and the variation of that force with power of distance. A stable elliptical orbit corresponds exactly to an inverse square variation. Precession forward measures variation that falls off faster than the inverse square, while precession backward measures variation that falls off slower than the inverse square.

The precession of our moon is about 3° forward every revolution which would measure variation inversely as the 2 and 4/243 power rather than the inverse square variation. Newton argued that this lunar precession can be neglected. For one thing the variation is approximately equal to the inverse square. More importantly, the precession is to be neglected because it is due to the action of the sun (as he promised to show later). Finally, to the puzzlement of more than a few, he explicitly appealed to the moon test agreement with Huygens' value that results from the inverse square variation as a reason for neglecting the lunar precession.

The precession theorem is a one-body idealization. It only takes into account the centripetal force, but Newton used it as his main tool to solve for the gravitational effect of a third body on an orbit. His method was to calculate the centripetal (say earth-moon axis) component of the action of the third body, then compose this force with the basic inverse square centripetal force to give a new total centripetal force that no longer would vary inversely as the square. In Corollary 2 of Proposition XLV he developed a formula for calculating precession from the centripetal component of a foreign force as a fraction of the main central force.

When Newton applied this method with the correct value for the ratio of the centripetal component of the sun to the action of the earth on the moon he could only account for about one-half of the observed precession. When Clairault, d'Alambert and Euler all worked on the problem in the 1740's with no more success than Newton had, Clairault and Euler were led to propose modifying the basic inverse square law (Waff 1976). When Clairault used a calculation that took into account not just the centripetal but the transverse component of the action of the sun he was probably trying to conclusively falsify Newton's theory, much as Kepler falsified his vicarious theory (Waff 1976, p. 175). To his great surprise, he found that this accounted for the lunar precession.

4.5. *Unification, the Moon Test and the Tides*

(a) *The Moon Test.* Newton's famous moon test showed that the inverse square force on the moon (calculated from the centripetal acceleration of its orbit) would, at the surface of the earth, equal the force of terrestrial gravity (measured by Huygen's seconds pendulum). In Proposition IV, he appealed to his first two Rules of Reasoning,

Rule 1 No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena.
Rule 2 Therefore, the causes assigned to natural effects of the same kind must be, so far as possible, the same (Cohen I. B. and Whitman A. 1987).

to argue from this agreement in measured value to the claim that the centripetal force on the moon is identical with terrestrial gravity.

Newton's calculation of the centripetal acceleration of the moon used the simplifying geometrical assumptions that the moon orbits the centre of the earth (rather than the common centre of gravity of the earth-moon system) and that the earth was a perfect sphere (rather than oblate with a larger semidiameter at the equator). Yet he put great weight on the resulting close agreement with Huygens' value in the moon test. He made the key inference to identify the centripetal force on the moon with gravity, and he even appealed to the agreement with Huygens' as an additional excuse for neglecting the lunar precession in his inference to the inverse square variation of the force on the moon in Proposition 3.

Forster sees the role of Newton's first two rules here as that of endorsing a Whewellian policy to identify magnitudes when this is appropriately supported from below by a consilience showing agreement in their *independently* measured values. On this view Newton should have kept his evidence for colligating the inverse square variation of the force on the moon completely independent of the resulting agreement with Huygens. Moreover, he should have made sure that the simplifying assumptions were not giving an accidental agreement before regarding the consilience as independently established. Forster (1988, pp. 86—87) explicitly worrries that the moon's orbit cannot provide an appropriately independent measurement of its centripetal acceleration from celestial phenomena until it can be backed up with an independent measurement of the Moon's mass so that its contribution to the joint force can be factored out by using the common centre of gravity.

Newton's use of the rules to put so much weight on the agreement, even when its warrant from below had not been established, shows that his practice of theory construction is more free-wheeling than the bottom up procedure Forster admires.[13] I think it is clear that, once the initial agreement has been shown, the task of showing that it will hold up as simplifying assumptions are removed is not one of attempting to generate enough force of evidence to carry the inference to the identification through, rather it is simply making sure that the new unified theory can account for the data, so that the unification will not be undone by a failure to satisfy the first test. For Newton the Rules provide the weight for the identification. The consilience is needed only to license permission to give the unification a chance to get off the ground.

(b) *The Tides.* Newton did provide a way to measure the mass of the moon so as to show that the agreement in measured values in the moon test would hold up when the simplifying assumption that the moon's orbit is centred on the centre of the earth is replaced by the more correct assumption that they orbit a common centre of gravity. Newton's way of measuring the mass of the moon was to appeal to the tidal phenomenon which Forster gave as his first example of a Whewellian colligation. Newton used the ratio between high and low values calculated from the coefficients of the equation colligating the correlation of spring and neap tides with syzygies and quadratures to measure the ratio of the moon's mass to that of the sun from their relative contributions to this tidal phenomenon.

Consider the effect one body (say the moon alone) would have on the seas of a water-covered earth. On the side toward the moon the water is pulled away from the earth, because it is nearer to the moon than the solid part of the earth is. On the side away from the moon there is a similar increase in the depth of water, because the solid earth is closer to the moon and so is pulled away from the water on that side. The basic effect of the moon's gravity on a sea-covered earth is to cause nearly equal water bulges on the near and far sides of it.

Now consider the additional effect of the sun's gravitation. At conjunction and equally at opposition the sun's force adds maximally to that of the moon to increase the basic effect. This is why the spring tides (highest high tides) occur at both syzygies (at 0° and at 180°). At quadratures (90° and 270°) the action of the sun is maximally opposed to the stronger action of the moon (its basic effect here would be to take water away from the sides on which the moon is making it deeper). This is why the neap tides (lowest high tides) occur at quadratures.

Newton was not satisfied with this merely qualitative causal explanation. He wanted to be able to use the tidal ratio to calculate the ratio of the mass of the moon to the mass of the sun. Such a calculation can be given from the theory. The basic differential effect of the moon between the water at a point B and the centre of the earth C when the moon is at A directly over head is

$$L = GMm/(\overline{AB})^2 - GMm/(\overline{AC})^2$$

Similarly, for the sun

$$S = GMs/(\overline{A'B})^2 - GMs/(\overline{A'C})^2.$$

When it is directly over head at A'. If we had a perfectly unimpeded water flow, an untilted earth with the moon's orbit and the earth's equator in the plane of the ecliptic then we would have

$$\frac{(\text{Spring height})}{(\text{Neap height})} = \frac{L+S}{L-S}$$

so that we could start from the tide ratio and solve the equations for the ratio of Ms to Mm.

For cases where the earth is in equinoxes we can calculate the differential effect at a given latitude, taking into account the inclination of the lunar orbit by averaging over the various combinations of greatest and least angles from zenith of the sun and moon at syzygies and quadratures. Philip Catton (1988) was able to show that such a calculation for latitude 50° would give Newton's 9:5 ratio, when it was further corrected by three corrections Newton actually applied in his own calculation. Newton's tidal data were taken from equinoxial syzygies and quadratures (Cajori, p. 479) and were from about latitude 50°. One of the corrections is for tidal delays which could be established by local observations. The other two are corrections for true special features exhibited by the lunar orbit when the earth is near equinoxes. We can go from Newton's data back through the equations to recover the true ratio of the mass of the moon to that of the sun.

(c) *Explanation.* Newton established for the first time a new mathematical ideal for causal explanations. One should develop one's mathematical force model sufficiently so that when it is applied to explain a phenomenon one can establish from the model the sort of equivalence theorems that back up the deductions of forces from phenomena. This very strong sort of explanation requires that the relevant features of the magnitudes that explain the phenomena are also *measured* by the phenomena they explain. Here we see a realization with Newtonian component forces of Cartwright's (1983) ideal that causal explanations require that the cause be measured by the effect it explains.

4.6. *Generalization Using the Rules of Reasoning*

In Proposition V Newton extended the identification of orbital force with gravity to the moons of Jupiter and Saturn and to the Planets. His argument for this consisted in noting the inverse square centripetal nature of the accelerative measures of these forces (accelerations of satellites of the same primary depend only on their distances not on their respective sizes) and applying Rule 2. These inverse square centripetal forces are of the same kind as the force on the moon, therefore they should, as far as possible, be assigned the same cause – gravitation of the satellites to their respective primaries.

Whewell (Butts 1968, p. 332) argues that the problem of deciding when effects are of the same kind renders applications of Rule 2 vacuous.

> Are the motions of the planets of the same kind with the motion of a body moving freely in a curvilinear path, or do they not rather resemble the motion of a floating body swept round by a whirling current? The Newtonian and the Cartesian answered this question differently. How then can we apply this rule with any advantage? (p. 332).
>
> It is only when it appears that comets pass through this plenum in all directions with no impediment, and that no possible form and motion of its whirlpool can explain the forces and motions which are observed in the solar system, that he [the Cartesian] is compelled to allow the Newtonian classification of events of the same kind (p. 333).

Whewell suggests that Newton's claims to deduce inverse square centripetal forces from the phenomena of orbital motion (as well as his use of Rule 2 in Proposition V to identify these as gravitational forces) depend on his being able to refute the Cartesian alternative hypotheses of vortices. This would make Newton's internal criticism of Cartesian vortices in Book II an essential part of the argumentation supporting Newton's deductions from phenomena in Book III.

Rule 4 makes it clear (as does the famous remark about hypotheses in the general scholium) that Newton thought that mere hypotheses can be ignored because they are not deduced (or inferred by general induction) from phenomena.

> Rule 4 In experimental philosophy, Propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions (Cohen I. B. and Whitman A. 1987).

We have seen that Newton's deductions from phenomena depend upon background assumptions and upon idealizations about what phenomenon a certain effect is to count as. In the case of the centripetal force on the moon the very precession theorem (Corollary I, Proposition XLV, Book I) connecting variation of force with power of distance to apsidal motion would allow inverse variation with the 2 and 4/243 power of distance to be deduced from the observed apsidal motion, unless the assumption of no interfering forces can be undercut. Relative to that assumption, the theorem shows that the phenomenal magnitude would count as a measurement of this alternative to the inverse square variation. This is why Newton must make plausible the claim that the apsidal motion of the moon is due to an interfering force.

I assume that Newton thought that the Cartesian hypothesis was not backed up by any similarly detailed mathematical equivalences between vortical magnitudes and specifications of relative motions that could count as phenomena.[14] I see no reason why the reasonableness of this opinion should depend on his actually having carried out his internal criticisms of Cartesian theory in Book II. These criticisms may perhaps be viewed as an attempt to show that no plausible vortical magnitudes could have values equivalent to Keplerian orbital phenomena. Even if Newton had not provided these arguments, however, he would have been reasonable in regarding the burden of proof on the Cartesians to provide such equivalences if their proposals are to count as serious rivals rather than mere hypotheses that can be safely ignored.

In the first part of Corollary I, Newton extends the power of gravity to planets without moons.

There is, therefore, a power of gravity tending to all the planets, for, doubtless, Venus, Mercury, and the rest, are bodies of the same sort with Jupiter and Saturn.

Here there are no already established inverse square centripetal forces to go on, so there are no similar natural effects that can be used to apply Rule 2. Instead, the inference is carried by the assertion that these planets are bodies of the same sort as the others. In Corollary II, Newton infers that the gravitational forces to all planets are inverse square forces. Again, no rule and no additional facts are cited. The idea seems, again, to be that the gravitational forces toward the planets without moons are the same kind of forces as those towards planets with moons and therefore that they too should be inverse square.

Rule 4 tells us to regard as true those propositions inferred by a induction from phenomena, and Newton tells us

This rule we must follow, that the arguments of induction be not evaded by hypotheses (p. 400).

This makes it clear that it is even propositions like Corollary II and the first part of Corollary I, which are not deduced or inferred by Rule 2 but only inferred from phenomena by induction, that get protected from evasion by mere hypotheses.

We can use the fourth rule as a clue to illuminate some of the puzzles about the "vera causa" clause in Rule 1. There has been considerable discussion over what role, if any, the requirement that we admit only causes which are *true* should play in applications of Rule 1 (e.g. Whewell, Koyre). If we take Newton at his word, Rule 4 provides the answer. We are to regard as true those propostions inferred by general induction from phenomena (as well, of course, as those actually deduced or otherwise more directly inferred). This suggests that Forster's Whewellian interpretation of the "vera causa" condition in Rule 1

Whewell's reading of Newton's "true causes" (verae causae) as those "causes" that are found to be identical with the causes of other phenomena corresponds with our understanding of Newton's Rule 1 and reinforces the previous interpretation of what Whewell meant by consilience (Forster 1988, p. 80)

would be rejected by Newton. The extension of the power of gravity and its inverse square variation to planets without moons such as Venus and Mercury in Corollaries I and II of Proposition five is not backed up by any consiliences of independently measured forces.

In Proposition VI Newton argued that the motive quantity of the centripetal force of gravity of a planet on a body is proportional to the inertial mass of that body. In Proposition VII he argued that the absolute quantity of the gravity of any body is proportional to its inertial mass. This stage in his argument involved an appeal to rule three to extend gravity and its proportionality to inertial mass to all planets. It also involved the generalization to universal gravitation. These generalizations are even more radical departures from Forster's bottom-up ideal.

Forster's common cause realism may be less restrictive than the rather extreme bottom up interpretation I have put upon it. This is suggested by the warm remarks he makes toward Newton's third Rule of Reasoning (1988, p. 94).

> To complete this story, we should emphasize that, once discovered to hold within a certain domain, the law is automatically extended universally to apply in all future instances and *for all bodies* whatsoever. There is an inductive generalization as traditionally construed, but it is a generalization *constrained* by the results of experimentation. Newton's third rule of reasoning is as follows: "The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever." In particular, the *power* of gravity measured by the gravitational mass of a body is found to be invariant over all independent determinations for all applications and for all times so far examined, so this property should be extended to other bodies and to other times. So, we should extend the previous measured value for the mass of the cup to apply to future times, and we assume that all as yet undiscovered bodies obey the same laws (as was instrumental in the discovery of Neptune). However, the preconditions placed on this inductive inference — conditions demanding the consilience of the coefficients so far measured — protect this form of inference from the usual counterexample to enumerative induction (such as the chicken that is fed every day 99 times in a row and then killed on the 100th day). As Whewell says, it is the *intermediate* step of adding a new conception — in this case the 'mass' concept — that enables us to *verify* the truth of our law in terms of the consilience of inductions. It is this form of verification (Whewell's second and third tests of hypotheses) that the naive cases of enumerative induction lack (Forster 1988, p. 94).

Whewell (Butts 1968, pp. 333—334) did not at all share Forster's enthusiasm for Newton's third rule. As Butts (1970) has pointed out

It is central to Whewell's philosophy of science that we recognize that observation, even experimentally regularized observation, can confer on propositions about facts only a probable universality (Butts 1970, p. 144).

Forster has largely avoided talking about Whewell's views on necessity and conceptual change (except in so far as colligation involves imposing a conception). These are aspects of Whewell's philosophy of induction that Butts has drawn to our attention (Butts 1976). I think the issue of inferences to universality Forster confronts in this passage require him to deal with these more problematic aspects of Whewell's thought.

5. NATURAL KIND REASONING

I think Newton's generalizations by induction, and his applications of Rule 3, are examples of Natural Kind inferences such as the inference from a value of the specific gravity of one or a few samples of a given alloy of metal to the same specific gravity for others. Such inferences abound in science. We measure the charge of one or a very few electrons and are quite sure all other electrons have the same charge. They are cases where we generalize even when there are no significant masses of data points on which to use means, least squares and other statistical techniques to support the inference. I have argued (1986) that such super-inductive inferences are legitimate when made relative to a natural kind conception that specifies which features are essential to the kind in question. Thus, we can infer specific gravity but not shapes for samples of an alloy.

If we accept a conception into the role of specifying a feature ø which allows such super-inductive inferences for a kind x then we have to accept the enabling conditional,

If any x has ø then every x has ø,

as certain; or we will not be able to infer anything more than probable universality. So it is, I think, that when Newton tried to use Rule 3 to establish that gravity applied universally to all bodies and that it universally varied directly with inertial mass and inversely with square of distance, he could not avoid making these features essential to a natural kind conception for bodies, without giving up his inference to universality.

I also think that it is a part of our regular practice in science to regard these essential properties, on which we base such inferences to universality, as necessary. Kripke and Putnam are surely correct in their explication or how we reason about identities and natural kind terms. Therefore, if I am correct about the fact that we do make natural kind inferences and thus treat some conceptions as explicating natural kind essences, it follows that (contrary to the view of most empiricists) science does seriously involve modalities.

These scientific natural kind conceptions I have been talking about do not have some features Newton worried about when he resisted calling gravity essential to bodies. He was worried making gravity essential would preclude any further investigation into its causes. In particular he wanted to keep open the possibility of a mechanical explanation of gravity; nevertheless he was sure that gravity really existed and operated by the universal laws he had discovered. I think Newton's discussion in *De Gravatatione* (Hall, A. R. and Hall, H. B., 1962, pp. 89—156) where we can generate true and universal mathematical laws of motion phenomena without any metaphysical account of their ultimate nature can be regarded (in part) as an argument for introducing mathematical natural kind conceptions of just the scientific sort I am talking about. We don't stop making natural kind inferences with respect to charge on an electron when we explore deeper explanations of their nature. That their nature has such and such features essentially does not preclude further investigation in which we search for an explanation of these features.

Secondly, as I have indicated, scientific natural kind conceptions are subject to overthrow when an alternative conception becomes available which is developed enough to count as a serious rival. The status these natural kind conceptions enjoy when they are accepted is at most only contextually apriori. They are regarded as absolutely certain and, perhaps, even immune to any merely empirical refutation, but they are not incorrigible. Once a serious rival becomes available these conceptions lose their invulnerability and are open to refutation by being tested against the rival.[15]

There is a great deal of similarity here with Whewell's program. Not just any colligations count as natural kind conceptions, but those colligations Whewell characterizes as theories of causes do.

> Inductive truths are of two kinds, Laws of Phenomena, and Theories of Causes. It is necessary to begin every science with the Laws of Phenomena; but it is impossible that we should be satisfied to stop short of a Theory of Causes (Aphorism XXIV).

On Forster's common cause interpretation we begin to have a theory of causes when we can unify by identifying magnitudes so that several lower level colligations can be regarded as measurements of the same thing. On the view I am advocating these measurements do not count as causal explanations unless the equivalences grounding them are based on a natural kind conception. We do not treat them as really causal until they are based on conceptions we are prepared to universally generalize upon. This is why there is a point to Whewell's distinction between causal theories and laws of phenomena.

We have seen the close connection between Newton's use of his third rule and the respectable scientific practice of making natural kind inferences. Newton's other rules also make good sense in the context of natural kind reasoning (Harper 1983, 1986). The first two rules function to endorse a policy to introduce unifying conceptions wherever possible, while the 4th rule expresses the resilience of an accepted kind conception. It can be corrected by phenomena, but it will not be undercut by mere hypotheses. Until an alternative conception is developed enough to rival capacity of the original theory to illuminate the phenomena it colligates that alternative hypothesis should not be allowed to undercut the theory.[16]

What makes an alternative count as a serious rival, rather than as a mere hypothesis to be ignored? When we think of the role of a natural kind conception as that of making intelligible the phenomena in its range by providing causal explanations, the answer is obvious. A rival has to be a plausible candidate for developing into a theory that will provide alternative explanations for enough of these phenomena to count as a better account of the causal powers underlying them. This is why the Brahean hypothesis became a non-starter once Newton's dynamical explanation of Kepler's phenomena became available. Any hypothesis to restore it would have to come up with a rival treatment of Newton's dynamical explanation of motions including, for example, the force of the sun to move the tides.

What makes unification and simplicity contribute to security is that each identification of coefficients as measurements of a common cause adds a new range of phenomena that must be explained by any rival conception that would replace the conception grounding the causal explanation. Each supposition that is deduced from a phenomenon in the old theory must be maintained as true or approximately true, or the phenomenon must be challenged, or the background assumptions of the equivalence theorems must be undercut. Moreover, if the rival theory is to explain such a phenomenon it must provide for new equivalence theorems which are more defensible than the old ones and which allow the relevant coefficients of its alternative causal supposition to be measured by that phenomenon. Thus it is, that as the network of deductions from phenomena supporting a theory becomes more extensive and more closely knit the theory becomes more secure.

Forster's recommendation that each move to identify magnitudes as measurements of the same common cause be first backed up by consilience established by independent measurements may be a more restrictively bottom-up requirement than we want to impose on theory building. His moderate "common-cause" realism, which counts as real only those causes which have been measured by more than one effect, may also be less generous than we would like. Nevertheless, his positive view — that unifications which are supported by successful over-determination of a common cause by independent measurements provide powerful evidence in favor of a theory — accurately represents both common sense reasoning and scientific practice.

We can agree with Forster that this common sensical as well as scientific practice of attributing evidential force to independent measurements of a common cause is a reasonable one. We have seen that new (successful) independent measurements add new phenomena for which a rival must account. Indeed, we have seen that such new measurements can increase support, even though the theory being supported may be regarded as already certain. The increased support adds to security by legitimately stiffening resistance to revision.[17]

NOTES

[1] I say "it would seem to be unavoidable" rather than that "it would be unavoidable" because it is not clear that conceiving the new theory would not lead to a new prior probability function (perhaps by actually modifying the relevant algebra of propositions over which degrees of belief are defined) that would legitimately make the new theoretical commitments more secure (even more probable) than the old ones were. Some such proposal was suggested by L. J. Cohen in his insightful comment (1967) on Mary Hesse's (1967) interesting attempt to explicate Whewellian consilience within the framework of confirmation theory.

[2] Brahe actually used this point to argue against the Copernican hypothesis. His measurements were good enough to detect stellar parallax greater than 1′ of arc. This implied that if the Copernican hypothesis were correct the fixed stars would have to be much farther away than any of what were regarded as reasonable estimates of this distance (van Helden, 1985, p. 51).

There was also a problem about the implications for the sizes of the stars. It was generally agreed that the apparent diameter of a third magnitude star was about 1′ so that the Copernican hypothesis implied that even third magnitude stars would be at least as large as the earth's orbit with major stars comparably larger (van Helden 1985, p. 51). Galileo helped defend the Copernican hypothesis against this latter implication by pointing out that the stars' apparent diameters are much smaller when the sparkling rays surrounding them are removed by observation through a telescope or in twilight (van Helden 1985, pp. 66—67).

Stellar parallax was not actually observed until Bessell detected a shift of Alpha Centauri against more distant stars in 1838 (Hansen 1973, p. 20) long after the Newtonian revolution had settled the issue in favour of Kepler rather than Brahe.

[3] We can specify an approximation under which the Keplerian account follows from Newton's and the Brahean account is ruled out by Newton's. Newton applied an hypothesis that the centre of the system is at rest. Kepler's centre (the sun) is much more nearly unaccelerated than Brahe's centre (the earth).

[4] Other closely related ideas are Skyrms' (1980) concept of *resilience*, ideas about *relative importance of information* used by Gardenfors (1984, 1988) and by Spohn (1988), and concepts of *robustness* which have been appealed to by Rosenkrantz (1977), Wimsatt (1981) and Glymour (1988).

[5] Kepler's *Astronomia Nova: Tradita Commentariis de motibus Stellae Martis*, Heidelberg 1609 gives his own account of this struggle. N. R. Hanson (1973) *Constellations and Conjectures* (pp. 253—282) and C. R. Wilson "How Did Kepler Discover His First Two Laws?", *Scientific American* 226 (1972) pp. 92—106 are two quite accessible and illuminating accounts. The most impressive and helpful account I found was in B. Stephenson (1987, pp. 39—49).

[6] Howard Stein suggested to me that this is somewhat oversimplified since the so-called "secular inequities" don't average out, but rather accumulate.

[7] Forster makes this point a cornerstone of his cogent answer to skeptical arguments by Cartwright (1983) and Ellis (1965) about the existence of component forces.

[8] There is a discrepancy between Whewell's text and table of contents. In the text (NOR, pp. 83—85) he includes article 9 which is entitled. "Hypotheses may be useful though inaccurate" in Sect. III, while his table of contents (NOR, pp. XV) located this article in Sect. II on the use of Hypotheses.

[9] I am leaving aside Kepler's work on the inclination of Mars' orbit to the ecliptic (Wilson 1972, pp. 94—95; Small 1894, pp. 145—184; Stephenson 1987, pp. 21—39).

[10] Stephenson (1987, pp. 1—4 and especially pp. 21—49) makes a compelling case that Kepler was motivated throughout his work on Mars (and indeed in all his work) by an attempt to give an account that would fit with plausible reasoning about physical causes of the motions. His discussion makes it clear that there is much more going on in Kepler's move from the vicarious theory through various stages to eventually settle on the elliptical orbit than can be accounted for by simply noting the availability to him, in Brahe's data, of greatly improved information about relative distances.

[11] Herbert Simon's group have developed computer programs for discovery of quantitative empirical laws [The Bacon Programs (Langley *et al.* 1987, pp. 65—170)] that will generate Kepler's third law from the data. There is, perhaps, a significant difference between Kepler's war on mars, where causal reasoning led eventually to successful

colligation of the elliptical orbit (as well as the law of areas) and the more purely computational search for higher order symmetries that led to the third law.

[12] Here I am disagreeing with R. Laymon's (1983) suggestion that it was Newton's regular practice to count as phenomena idealizations that were beyond reasonable error limits on the measurements available. I think Laymon has failed to distinguished between those empirical generalizations Newton explicitly cited as phenomena and the special mathematical idealizations appealed to in proofs of theorems, in what I. B. Cohen (1980, pp. 52—153) has called "Newton's mathematical style".

My investigations suggest that Newton devoted some effort to, and on the whole did quite well at, keeping the idealizations he explicitly cited as phenomena within reasonable limits of error on the measurements available to him. For example, when we analyse the data in the table corresponding to the evidence for the Harmonic Law of Jupiter's moons, we find that the distances calculated from the periodic times differ from the means of the cited astronomical measurements by respectively 0.74, 1.1, 1.28, and 1.02 standard deviations. Newton goes on to give additional values for these distances calculated from Pound's measurements (Cajori, p. 402). If these are added in, the differences from the new means are respectively 0.324, 0.471, 0.525 and 0.372 standard deviations.

[13] Though I shall not take them up here, Mary Hesse's interesting discussions (e.g. 1967, 1974) of the role of analogical reasoning in consilience illuminate important aspects of this more free wheeling character of Newton's practice of theory construction.

[14] Howard Stein pointed out to me that Huygens and Leibniz (major defenders of vortical theories) accepted Newton's deductions of centripetal forces from phenomena, and did not take issue with Newton's argument for Propostition V. This suggests that Whewell's objection is even less cogent than I have allowed. Indeed, much work by vortical theorists following the publication of *Principia* can be regarded as an attempt to make vortical theory recover more or less similar versions of Newton's own deductions from phenomena.

[15] According to Quine's (1970) compelling picture, science is a piecemeal process of introducing and improving theories to replace prescientific natural kind conceptions based on innate ideas of similarity. Quine (pp. 21, 22) sees explicit scientific theories as rendering the modal associations of the old natural kind conceptions redundant. I see our scientific theories as taking on the modal features of prescientific natural kind conceptions as they take over their role in making ranges of phenomena intelligible. I think natural kind reasoning is unavoidable. We cannot get outside the process Quine describes. I do not think Cartesian doubt is an epistemic stance that any of us can actually take. What we can do is to try to improve our natural kind conceptions. This, I suggest, is just what science is doing for us.

[16] When we consider the failure of the vortical hypothesis to support equivalences that could seriously rival those Newton provided we can see that this is not as Van Fraassen (1985, p. 265—266) would have it, an excessively conservative principle. Nor is it, as he would also have it (van Fraassen 1985, pp. 265—266), limited in its descriptive application to the superceded scientific practices of an earlier age. As I see it, this rule and the others are reasonable policies for introducing and revising scientific theories

construed as natural kind conceptions. The history of science and its developing situation today reveal that these policies have been and continue to be a regular part of our scientific practice.

[17] The same point can be made probabilistically if we agree with Brian Skyrms (1979) that science aims at not just high probability but a *resiliently* high probability. The resiliency of an hypothesis h is defined relative to a set of conditions c_i, each of which is consistent both with h and with not-h.

$$\mathrm{Res}\,(h) = \mathrm{Min}\,\{P\,(h/c_i)\text{: all the } i\text{'s}\}.$$

Let the c_i's be various possible data to be added by future observations to those already colligated by two or more phenomena which have been successfully unified by an hypothesis h_u which takes each of the phenomena as independent measurements of the same common cause (say the mass of the Sun). Let h_j be an hypothesis about coeffiencts of one of these phenomena separately (say the ratio R^3/T^2 for Jupiter's orbit in some appropriate units). Let c_i be some bad data for the hypothesis h_j (say new observations which would put R^3/T^2 far away from the previously colligated value). If our conditional probabilities are sensitive to reasonable statistical estimates of fit in relevant data sets then

$$P_1(h_j/c_i) < P_2(h_u/c_i),$$

where P_1 is the original prior and P_2 is the new epistemic probability after the unification, since the relevant data base for h_u includes all the data supporting h_j together with all the data supporting the other phenomena that have been successfully unified as independent measurement of h_u. Moreover, it sould be clear that some such worst case for h_j will be worse for it than any worst case for h_u, just because the set of data supporting h_u is so much larger.

BIBLIOGRAPHY

Baigrie, B. S. (1987). "Kepler's Laws of Planetary Motion Before and After Newton's Principia: An Essay on the Transformation of Scientific Problems", *Studies in History and Philosophy of Science* **18.2**, 177—208.

Butts, R. E. (1965). "Necessary Truth in Whewell's Theory of Science", *American Philosophical Quarterly* **2.3**, 1965.

Butts, R. E. (1968). *William Whewell's Theory of Scientific Method*, Pittsburgh.

Butts, R. E. (1970). "Whewell on Newton's Rules of Philosophy", in Butts and Davis, pp. 132—49.

Butts, R. E. (1973). "Whewell's Logic of Induction", in Giere, R. N. and Westfall, R. S. (eds.), *Foundations of Scientific Method: The Nineteenth Century*, Bloomington, Indiana University Press.

Butts, R. E. (1977). "Consilience of Inductions and the Problem of Conceptual Change in Science", in Colodny, R. G. (ed.), *Logic, Laws and Life*, Pittsburgh, University of Pittsburgh Press, pp. 71—88.

Butts, R. E. and Davis, J. W. (1970). *The Methodological Heritage of Newton*

Cajori, F. (1962). *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and his System of the World*, Berkeley and Los Angeles, University of California Press.

Cartwright, N. (1983). *How the Laws of Physics Lie*, Oxford, Clarendon Press.
Catton, P. (1988). "Disputing 'Newton and the Fudge Factor' ", Manuscript University of Western Ontario.
Cohen, I. B. (1971). *Introduction to Newton's Principia*, Cambridge, Harvard University Press.
Cohen, I. B. (1980). *The Newtonian Revolution*, Cambridge, Cambridge University Press.
Cohen, I. B. and Koyré A. (1972). editors *Isaac Newton's Philosophiae Naturalis Principia Mathematica*,
Cohen, I. B. and Whitman A. Translators (1987). *Isaac Newton's Mathematical Principles of Natural Philosophy*, Forthcoming, Cambridge, Massachusetts: Harvard University Press, and Cambridge, England: Cambridge University Press.
Cohen, L. J. (1967). "An argument that confirmation factors for consilience are empirical hypotheses", in Lakatos, I. (ed.), *The Problem of Inductive Logic*, North Holland, pp. 247—250.
Earman, J. (ed.). (1983). *Testing Scientific Theories*, University of Minnesota Press.
Ellis, B. D. (1965). "The Origin and Nature of Newton's laws of Motion", in Colodny, R. G. (ed.), *Beyond the Edge of Certainty*, New York, Prentice Hall.
Forster, M. R. (1988). "Unification, Explanation, and the Composition of Causes in Newtonian Mechanics", *Studies in History and Philosophy of Science* **19.1**, 55—101.
Freidman, M. (1983). *Foundations of Space-Time Theories*, Princeton.
French, A. P. (1971). *Newtonian Mechanics*, MIT Press.
Gardenfors (1984). "Epistemic Importance and Minimal Changes of Belief", *Australasian Journal of Philosophy* **62**, 136—157.
Gardenfors (1988). "Causation and the Dynamics of Belief", in Harper, W. and Skyrms, B. (eds.), (1988).
Glymour, C. (1980). *Theory and Evidence*, Princeton.
Glymour, C. (1988). "AI for Statistical and Causal Modelling", in Harper, W. and Skyrms, B. (eds.).
Godin, G. (1972). *The Analysis of Tides*, Toronto.
Hanson, N. R. (Humphrey's W. C. ed.). (1973). *Constellations and Conjectures*, Reidel, Dordrecht.
Harper, W. (1986). "Kant on the *a priori* and Material Necessity", in Butts, R. E. (ed.), *Kant's Philosophy of Physical Science*, pp. 239—272.
Hesse, M. (1968). "Consilience of Inductions", in Lakatos (ed.), *The Problem of Inductive Logic*, North Holland.
Hesse, M. (1971). "Whewell's Consilience of Inductions and Predictions", *Monist* **55.3**, 520—524.
Hesse, M. (1974). *The Structure of Scientific Inference*, Macmillan.
Koyré. (1973). *The Astronomical Revolution*, Cornell University Press.
Langley *et al.* (1987). *Scientific Discovery*, MIT
Laudan, L. (1970). "William Whewell on the Consilience of Inductions", *Monist* **55.3**, 368—391.
Laudan, L. (1970). "Reply to Hesse", *Monist* **55.3**, 525.
Laymon, R. (1983). "Newton's Demonstration of Universal Gravitation and Philosophical Theories of Confirmation", in Earman (ed.), (1983). pp. 179—199.
Owen, G. E. L. (1961). "Tithenai ta Phainomena", in Moravcsik (ed.), *Aristotle*, New York, Doubleday Press, 1967. pp. 167—190.

Putnam, H. (1983). *Realism and Reason*, Cambridge, Cambridge University Press.

Quine, W. V. (1970). "Natural Kinds", in Rescher, N. (ed.), *Essays in Honor of Carl G. Hempel*, Reidel, Dordrecht. pp. 5—23.

Rosenkrantz, R. (1977). *Inference, Method and Decision*, Reidel, Dordrecht.

Russell, J. L. (1964). "Kepler's Laws of Planetary Motion 1609—1666", *British Journal for the History of Science* **2.5**.

Shea, W. R. (1980). Nature Mathematized, Reidel, Dordrecht.

Skyrms, B. (1980). *Causal Necessity*, Yale Press.

Small, R. (1804). *An Account of the Astronomical Discoveries of Kepler*, Madison, The University of Wisconsin Press.

Spohn. (1988). "Ordinal Conditional Functions: A Dynamic Theory of Epistemic States", in Harper, W. L. and Skyrms, B. (eds.), (1988). pp. 105—134.

Stephenson, B. (1987). *Kepler's Phsycial Astronomy*, Berlin, Springer Verlag.

van Fraassen, B. (1980). *The Scientific Image*, Oxford: Oxford University Press.

van Fraassen, B. (1981). "Theory Construction and Experiment: An Empiricist View" in *P.S.A. 1980*, Asquith P. D. and Giere, R. N. (eds.), vol. 2, pp. 663—78. East Lansing, Michigan: Philosophy of Science Association.

van Fraassen, B. (1983). "Evidence and Explanation", in Earman (ed.) pp. 165—177 (1983).

van Fraassen, B. (1985). "Empiricism in the Philosophy of Science", in Churchland, P. M. and Hooker, C. A. (eds.), *Images of Science*, University of Chicago Press. pp. 245—308.

van Helden, A. (1986). *Measuring the Universe*, University of Chicago Press.

Waff, C. B. (1976). *Universal Gravitation and the Motion of the Moon's Apogee: the Establishment and Reception of Newton's Inverse Square Law, 1687—1749*, PhD Dissertation, John Hopkins University, University Microfilms, Ann Arbor.

Whewell, W. (1842). *The Philosophy of the Inductive Sciences (Founded Upon Their History)*, London, John W. Parker and Son.

Whewell, W. (1858). *Novum Organon Renovatum*, London, John W. Parker and Son.

Wilson, C. A. (1972). "How did Kepler Discover his First two Laws?", *Scientific American* **226**, 92—106.

Wimsatt, W. (1981). "Robustness, Reliability and Overdetermination", in Brewer, M. and Collins, B. (eds.), *Scientific Inquiry and the Social Sciences*, San Francisco, Jossey-Boss, pp. 124—163.