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The Scope, Limits, and Distinctiveness of the Method of 'Deduction from the Phenomena': Some Lessons from Newton's 'Demonstrations' in Optics

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ABSTRACT

Having been neglected or maligned for most of this century, Newton's method of 'deduction from the phenomena' has recently attracted renewed attention and support. John Norton, for example, has argued that this method has been applied with notable success in a variety of cases in the history of physics and that this explains why the massive underdetermination of theory by evidence, seemingly entailed by hypothetico-deductive methods, is invisible to working physicists. This paper, through a detailed analysis of Newton's deduction of one particular 'proposition' in optics 'from the phenomena', gives a clearer account than hitherto of the method—highlighting the fact that it is really one of deduction from the phenomena *plus 'background knowledge'*. It argues that, although the method has certain heuristic virtues, examination of its putative accreditational strengths reveals a range of important problems that its defenders have yet adequately to address.

1 Introduction: Newton, hypotheses, and demonstration

Isaac Newton's professed methodology—his explicit theory of what counts as proper science ('experimental philosophy')—appears remarkably simple:

> Whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy, particular propositions are inferred from the phenomena, and afterwards rendered general by induction (*Principia*, Book III, 'General Scholium').

In this celebrated passage, Newton, in the space of two sentences:

(1) states his basic negative methodological thesis—that hypotheses have 'no place in experimental philosophy';
(2) defines the term 'hypothesis'—'whatever is not deduced from the phenomena is to be called an hypothesis'; and
(3) thereby states his basic *positive* methodological thesis: that in order to become part of 'experimental philosophy', and hence be non-hypothetical, a claim must have been inferred from the phenomena with the help of inductive generalization (an overall process which, as the passage indicates, Newton was happy to characterize as 'deduction from the phenomena').

For many twentieth-century commentators, however, the idea of deducing a genuine theory from the phenomena—even with the help of inductive generalization—seemed, to say the least, misguided. After all, theories standardly go beyond observation statements, not only 'horizontally' in universally generalizing on what is inevitably a finite basis, but also 'vertically' in introducing genuinely new, theoretical content referring (or allegedly referring) to entities lying 'beyond' or 'underneath' the phenomena. The basic principle governing deductively valid inferences, on the other hand, is the principle of non-content-increase: in a valid inference, the conclusion is already contained in the premises—though of course such inferences may still surprise us in that we may not have been aware, ahead of seeing the proof that establishes the validity of the inference, that the conclusion was in fact implicit in the premises.

This fundamental problem led some commentators to conclude that Newton was totally confused—providing classic confirmation for Einstein's dictum that those seeking to understand science should look at what scientists do and ignore what they say they do. Thus, Imre Lakatos, for example, wrote with characteristic self-confidence ([1978], p. 212):

> The schizophrenic combination of the mad Newtonian methodology, resting on the *credo quid absurdum* of 'experimental proof' and the wonderful Newtonian *method* strikes one now as a joke. But from the rout of the Cartesians until 1905 nobody laughed.

Others have suggested that Newton's favoured theories *must be* hypotheses really and hence that he must have had some distinction implicitly in mind between *good* hypotheses (such as his own hypothesis of universal gravitation) and *bad* hypotheses (such as more or less any hypothesis ventured by Descartes or Hooke). This line goes back at least to J. F. W. Herschel who wrote ([1830], p. 176):

> Hypotheses must of all things be framed—not loose and incapable of being tested by following them into consequences, like those which Newton proscribed in his celebrated 'hypotheses non fingo',—but such as can be tested [. . .]

And the same line was taken in this century (unsurprisingly) by Popper (who saw Newton's criticism as levelled *only* at untestable *ad hoc* hypotheses) and by N. R. Hanson who wrote ([1970], p. 31):

> Newton was a great natural philosopher and hence a great hypothesiser [. . .] While he may have *said* 'to the devil with hypotheses', we must be careful to read him aright. His meaning is surely 'to the devil with all this woolly, purple, capital-lettered, philosophical navel-contemplation'.

What else, Hanson went on to ask, was Newton's theory of universal gravitation but a (most gloriously confirmed) hypothesis?

But neither reaction to this interpretative problem seems attractive. The Hanson line is straightforwardly inconsistent with a series of Newtonian texts. Sir Isaac's message about hypotheses was quite general: '*Whatever* is not deduced from the phenomena is to be called an hypothesis; and hypotheses, *whether metaphysical or physical, whether of occult qualities or mechanical,* have no place in experimental philosophy'; and he was quite explicit: the theory of universal gravitation is *not* a 'good' hypothesis—he had deduced it from the phenomena and hence shown that it was not a hypothesis *at all*. Moreover, Newton's famous attitude towards the material emission ('corpuscular') theory of light would be irreducibly mysterious if this Hanson-style view were correct. As is well known, Newton many times and very heatedly insisted that this emission theory was a *mere* hypothesis because it could not be deduced from the phenomena; and yet the theory is clearly testable and indeed Newton constructed, in the 'Queries' section of the *Opticks*, a perfectly kosher hypothetico-deductive argument for it—to the effect that the theory explains phenomena, such as rectilinear propagation and prismatic dispersion, that rivals, such as the wave theory, cannot. As for the Lakatos line, it just seems risky, to say the least, to suggest that Newton was totally confused, not at any rate without trying a little harder to find a more generous alternative.

Hanson and others are obviously correct that Newton's theory of universal gravitation, for example, is a 'hypothesis' in the sense that it transcends the phenomena. Not only does that theory generalize the inevitably finite evidence, it takes us one level 'behind' the phenomena: we—at best—observe the motions of, for example, the planets; Newton's theory tells us that those motions *are a consequence of* the fact that every particle in the universe attracts every other with a force proportional to its mass and inversely proportional to the square of the distance between them.[1] But Newton explicitly and repeatedly claimed that the theory of universal gravitation was no hypothesis in his terms, since it had been deduced from the phenomena. Immediately after the description of the method, quoted at the very beginning of this essay from the *General Scholium*, Newton asserted: 'Thus it was that [. . .] the laws of motion and of gravitation [. . .] were discovered' (*Principia*, Book III). Indeed, it was precisely the fact that he had 'demonstrated' this theory from Kepler's phenomena that, he believed, distinguished him from Hooke, Huygens, Wren, Wallis, and others, all of whom had indeed conjectured—but *merely* conjectured—the inverse square law.

But whatever Newton may have *claimed* to do, the laws of logic apply even to him. And those laws dictate that no valid inference can have a genuine theory as conclusion and purely 'phenomenal' statements as premises (even if we allow generalisations of those premises). So, did Newton simply make a logical blunder? Or did he, instead, perhaps have some more complex and sophisticated notion of 'deduction from the phenomena' in mind?

Recent work in philosophy of science has started to describe in some detail exactly such a notion and hence to provide a possible 'rehabilitation' of Newton's method. Sometimes known under aliases such as (new, improved) 'eliminative induction' or 'demonstrative induction', the idea that theories can be, and have been, in an important sense deduced from the phenomena has been argued by several recent influential commentators, to be in fact an important corrective to the many inadequacies of 'hypothetico-deductivism'. This argument stems from some initially relatively neglected papers of Jon Dorling's in the 1970s and has been developed by John Earman and John Norton, among others (see Dorling [1973], [1974], and [1991]; Earman [1992]; and Norton [1993], [1994], and [1995]). Important work along these same lines on Newton's 'deductions from the phenomena' (and especially his deduction of the principle of universal gravity from the phenomena) has been done by Clark Glymour, Elie Zahar, and Bill Harper, again amongst others (see Glymour [1980]; Zahar [1989]; and Harper [1989], [1991], and [1993]).

It seems, therefore, that Newton the methodologist is currently undergoing an 'image makeover': transformed from the totally confused methodologist—

[1] And, indeed, Newton's problem seems to be made still more intractable by the now well-known fact that his theory of universal gravitation is logically *inconsistent* with Kepler's 'phenomena'. No valid inference can, of course, lead from consistent premises to a conclusion inconsistent with those premises. (Newton himself, *of course*, knew of the inconsistency; it was reintroduced into active consideration in philosophy of science by Duhem; Duhem's point was then repeated by Popper and taken up by Feyerabend and others.)

the classic illustration of how great scientists can make great mistakes about how they achieved their great science, which he appeared to be in the heyday of hypothetico-deductivism—into the insightful anticipator of the altogether superior method of 'demonstrative induction' (or whatever we decide to call it) in our post-hypothetico-deductive era. Since his predecessors had nothing like as clear an understanding of this method (or even perhaps any understanding at all), Newton seems to be in the process of being revealed to be as great a revolutionary in methodology as he was in science.

This paper aims to give this interpretative story still one further twist. I argue that this latest view of Newton's methodology, although an important step in the right direction, is only half—or perhaps three-quarters—correct. I believe it to be correct as an interpretation of Newton: he really did hold that to establish theories as parts of positive science, they must be deduced from the phenomena; he really had much the same idea of this method as its more recent defenders; and he really did believe he had successfully applied the method, not only in establishing the principle of universal gravitation, but also, as we shall see, in establishing certain fundamental propositions in optics. I shall go on to argue, however, that, while there is a *somewhat* distinctive method of deduction from the phenomena, both Newton himself and its more recent defenders may have inclined to exaggerate its power, to exaggerate its real distinctiveness from 'hypothetico-deductive' methods, and hence to underestimate the problems that this method also faces.

My argument proceeds by concentrating, not on the already heavily analysed case of universal gravitation, but instead on one of Newton's relatively little-considered 'deductions' of propositions in optics 'from the phenomena'. Precisely because this optical 'demonstration' is simpler and more immediate, it highlights the general methodological issues about Newton's method much more straightforwardly, I believe, than the otherwise altogether logically more challenging and more interesting case of universal gravitation.

Of course, an analysis of Newton's optical demonstrations will produce the same general results as an analysis of his 'demonstration' of the principle of universal gravitation only if his method in the two areas is essentially the same. And this has in fact been denied: some historians have argued that there are two quite different approaches to science to be found in the *Principia* and the *Opticks*, respectively.[2]

There are undoubtedly significant differences between Newton's work in optics and his work in gravitation and mechanics—differences that result from the relative degrees of maturity of the two fields. For example, work in optics in

[2] This is a view associated, for example, with I. B. Cohen (see especially his [1956]); it is also, as we shall shortly see, held—by implication—by Howard Stein ([1991]).

the late seventeenth and early eighteenth centuries was altogether more 'empirical'—less mathematical and more directly based on experiment—than work in mechanics and astronomy. It does not, however, follow that the fundamental method of accreditation of theories—the logic of evidence—differed in the two areas.

Newton, as we shall see, preferred the term 'proof by experiments' for the arguments for his positive doctrine in optics. Although this term and the term 'deduction from the phenomena' might appear to be synonymous, Howard Stein claims in his [1991] that in fact Newton meant quite different things by them (and something different again by the term 'demonstration'). According to Stein, a 'proof by experiments' meant for Newton nothing more than '[t]he subjection of a proposition to test by experiment or observation' ([1991], p. 219). Although there are many other insights in Stein's challenging article, this claim—which would entail that Newton was after all a hypothetico-deductivist in optics—seems to me quite mistaken. There is, of course, no denying that the term 'proof' has often been used in the history of thought as a synonym for 'test' (as in 'the proof of the pudding is in the eating').[3] Hence the mere fact that Newton used the phrase 'proof by experiments' is not telling in itself. But the structure of his arguments *is* telling and it tells decisively against Stein: aside from a difference in deductive complexity and depth, a 'proof by experiments' and a 'deduction from the phenomena' differ only in the fact that experimental results form the explicit premises of the former, observational results the explicit premises of the latter. Newton's argumentative style in the *Opticks*, no less than in the *Principia,* is explicitly one of inference *to*, not from, theories: the asserted propositions consists of THEOREMs, the phrase 'which was to be demonstrated' occurs often, and, as we shall in detail below, Newton uses liberal helpings of phrases like 'therefore' or 'and so' followed by the theoretical proposition he is endorsing.

Moreover, Stein's view, if correct, would once again make quite mysterious Newton's repeated insistence on the difference between his positive doctrine of light and the material emission hypothesis. There seems no doubt that Newton could construct an argument that showed that that hypothesis passes tests—for example, it explains the so-called rectilinear propagation of light, and the simple law of refraction—tests which moreover the only plausible rival hypotheses then around (one form or other of the view of light as a disturbance transmitted through a medium) failed. (The argument that the wave theory fails the rectilinear propagation test is the main content of Query 28.)

A central question to be raised in this paper is exactly that of whether, on analysis, a Newtonian proof or demonstration of a theory by experiments *really* gives the theory any higher epistemic status than it might get by passing

[3] See Lakatos ([1976]) for the evolution of the notion of proof in mathematics.

experimental tests. But I do not think it can seriously be denied that Newton believed (or at least claimed to believe) both that his inferential method was different from any hypothetico-deductive argument, and that he had applied this method in arguing to his 'positive doctrine' of light. Because of the relatively unmathematical nature of Newton's optical researches, the 'depth' of the inferences from the optical phenomena is altogether less impressive than in the case of gravitation—but this, as I already suggested, promises to be an advantage from the present point in view: the general methodological problems are raised more readily and more sharply by the optical examples.[4]

2 Newton's deduction from the phenomena of the 'different refrangibility' proposition in optics

Newton insisted in his optical work, just as in the *Principia*, on differentiating sharply between hypotheses and positive doctrine. A proposition becomes positive doctrine, part of 'experimental philosophy', when it has been derived from the phenomena. It was in the 1672 letter to Oldenbourg containing his 'New Theory about Light and Colours' that Newton forcefully asserted:

> For what I shall tell concerning them [the spectral colours] is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phenomena [. . .] but evinced by ye mediation of experiments concluding directly and without any suspicion of doubt (*Correspondence,* **I**, pp. 96–97).

And Book I of the *Opticks* begins with the following statement of intent (p. 1): 'My Design in this Book is not to explain the properties of Light by Hypotheses, but to propose and prove them by Reason and Experiments.' I take it here that Newton sees 'Reason' as governing the *process* of proof, while the *premises* are provided simply by the results of 'Experiments'. Certainly the specific THEOREMS of the early parts of the *Opticks* are generally accompanied by arguments labelled simply 'The Proof by Experiments'. What exactly does it take for a proposition to be 'proved by experiments'? Newton's general remarks are few and unilluminating. His position can only really be gleaned from examples of his method at work.

The first major result of the *Opticks* and the cornerstone of Newton's 'experimental philosophy' of light states that 'the light of the Sun consists of Rays differently Refrangible' or, as he also put it, that sunlight is a 'heterogeneous mixture' of 'Rays differently Refrangible'. As already indicated, Newton had first claimed to prove this assertion from phenomena in his

[4] Another, related, advantage concerns the point already made about the strict inconsistency of Newton's principle of universal gravitation with Kepler's 'phenomena'. This means that the inference in that case necessarily involves use of the 'correspondence principle'. (For an exceptionally clear treatment see Zahar ([1989]).) No such problem occurs in the optics case.

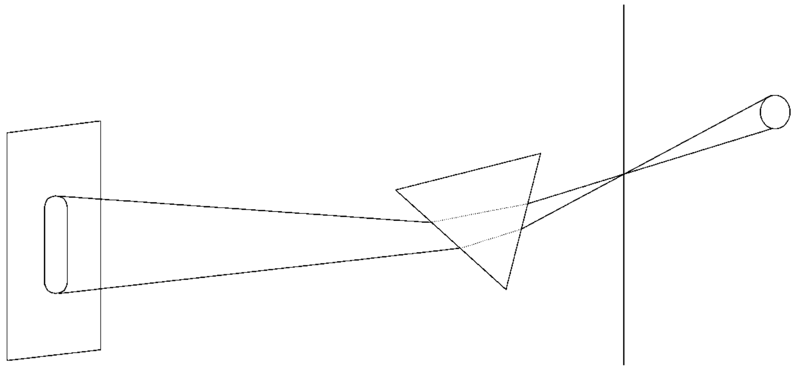

Fig. 1

celebrated 'First Paper on Light and Colours' of 1672. Although four experimental results are cited there, the proof seems to depend on just two.

In the first experiment a small beam of light was admitted into a darkened chamber through a hole in a window blind; and the beam was then passed through a single glass prism. The result is that the beam fans out in a direction orthogonal to the prism's axis. In fact the image of the sun cast on a wall some distance away from the prism was considerably longer than it was broad (see Figure 1).[5]

The second experiment involved in the 1672 proof is the celebrated *experimentum crucis*. This experiment is an elaboration of the first. Two screens with small apertures were placed in the light diverging from the first prism, and the light passing through both apertures refracted through a second prism (Figure 2). Both screens and the second prism were kept fixed throughout the experiment, but the first prism was set successively in different positions, so that different parts of the spectrum were in turn incident upon the second prism. Because of the two screens, these different parts of the initial spectrum all had the same angle of incidence on the second prism. But, despite that equality of the angle of incidence, the angle of refraction was different for the different parts of the initial spectrum. In particular, the light from the uppermost part of the spectrum created by passage through the first prism was refracted through a greater angle by the second prism than was the light from the lowermost part of the initial spectrum.

[5] Very careful reading of the paper reveals that Newton had observed this elongation of the image, even when the prism was held at the angle of 'minimum deviation'. This is highly significant since, in view of the finite angle subtended at the hole by the sun's disc, the theory of a single refractive index for sunlight predicts some elongation of the image for all positions of the prism, except that of minimum deviation. (Although very important, this feature of Newton's experiment was very easy to miss and his less acute critics duly missed it. For details see Sabra [1967].)

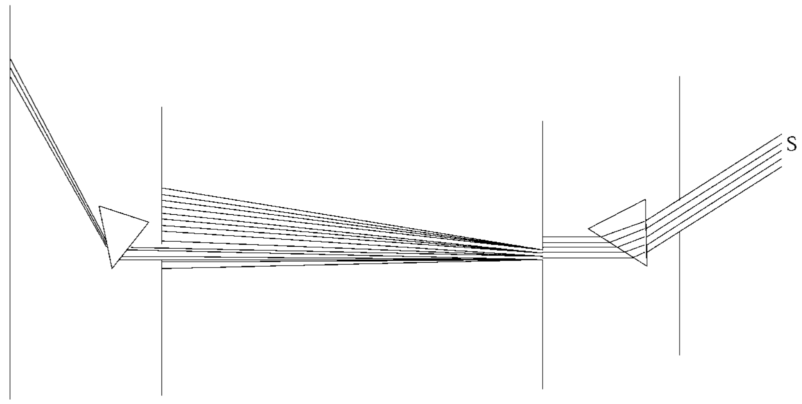

Fig. 2

Immediately after the statement of the result of this second, 'crucial' experiment, Newton drew the inference (and it *is*, notice, an inference):

> And so, the true cause of the length of that Image [that is, the one created by passage through the *first* prism] was detected to be no other than that Light [that is, the original sunlight] consists of Rays differently refrangible, which, without any regard to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards the divers parts of the wall (*Correspondence*, **I**, p. 95).

Newton was a persuasive arguer: if his inference appears obviously valid, notice that he here inferred a conclusion about the constitution of light *before* it enters the first prism from premisses concerning what happens to the light that emerges from that first prism when it crosses the second one; Newton's conclusion is that the *cause* of the beam's spreading out into a spectrum in the first prism is that it already consisted, on arrival at that first prism, of the differently refrangible Rays—Rays that are observationally identifiable (at best) only *after* emerging from the prism. (In fact, as we shall see below, 'Rays', in the sense in which Newton used the term, are genuinely theoretical, non-observational, entities—or, rather, *would be*, if they existed at all.)

The *Opticks* contains a somewhat different proof of this same proposition. Again there is no doubt about the claim to have produced a demonstration—the proposition is explicitly labelled 'Theorem' (it is Prop II, Theorem II of Book I, Part I) and is followed by a justification headed 'The PROOF by Experiments'. There is room for dispute about the exact structure of the *Opticks* proof—the results of no less than eight different experiments (some with their own variants) are cited. But Newton drew his conclusion several times before reaching the end of this list of experiments, and he seems to be offering several alternative proofs, the overall rhetorical effect of which was

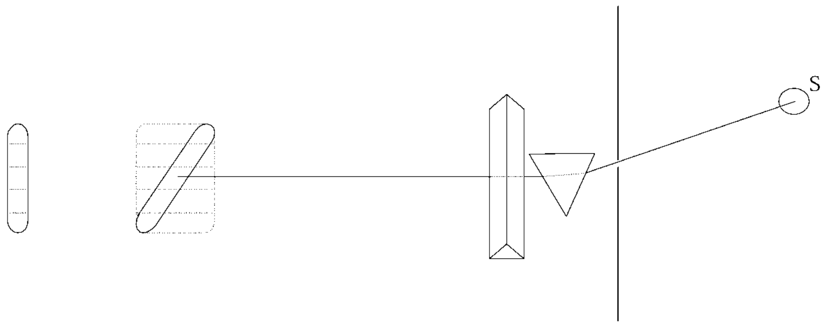

Fig. 3

intended to be overwhelming, but each of which alone would have been sufficient from the logical point of view.

The first of these proofs is in fact a revamped and rather more explicit version of the 1672 argument. Again two experiments were involved. The first of these was the single prism experiment—now reported in considerably more detail and with a lot more clarity, but with no essential change. The role of *experimentum crucis* was however recast. This new experiment consisted as before of re-refracting the light emerging from the first prism, but now the whole spectrum was re-refracted *at once* through a second prism placed *transversally* to the first (Figure 3). Hence, the plane of the second refraction in this 'crossed prisms' experiment was orthogonal to the plane of the first. Newton pointed out that if there were further dispersion of the elements of the first spectrum in the second prism then the initial spectral image would be spread out transversally—in particular if there were exactly as much dispersion of each of the elements of the once-refracted spectrum in the second prism as there had been of the initial sunlight in the first prism then the final image would be fully 'four square'. But, reported Newton, 'the Event is otherwise': no further dilatation of the image resulted from passage through the second prism. Instead, the final image retained the shape of the original, singly refracted image, but was shifted so as to be oblique to the original: the obliquity being the result of the upper (most refracted) part of the initial spectrum being displaced in the second refraction through a greater angle than is the lower (least refracted) part of the initial spectrum (again see Figure 3).

Newton argues from these experimental results to his proposition that sunlight consists of 'Rays differently refrangible'. The argument, as I see it, is essentially as follows:

(1) The single prism experiment produces 'parts of light' that are refracted by different amounts at the same angle of incidence—this is shown by the fact that, rather than a simple circular image of the source, an

elongated spectrum is projected onto the observation wall. The result is highly significant in itself but it leaves important issues open:

> But whence this inequality [of refraction] arises, whether it be that some of the incident Rays are refracted more, and others less, constantly, or by chance, or that one and the same [incident] Ray is by Refraction disturbed, shatter'd, dilated, and as it were split and spread into many diverging Rays, as *Grimaldo* supposes, does not yet appear by these Experiments (*Opticks*, p. 34).

(2) However, exactly this *does* appear 'from those [experiments] that follow'—and in particular from the crossed prisms result. The different 'parts' of light from the first spectrum are simply re-refracted in the same order as before by the second prism—there is no further dilatation or spreading of the individual rays. Newton takes it that this entails that the different 'parts' are refracted by different amounts 'constantly'—that is, that each of the 'parts' of the initial once-refracted spectrum has its own inherent 'degree of refrangibility'. (So the 'by chance' possibility has been eliminated—it would be too much of a coincidence if chance happened to produce the very same effects twice.)

(3) The next step is to argue from this sub-conclusion about the constitution of the light *emerging from* the first prism to the final conclusion about the constitution of the observationally undifferentiated sunlight as it *arrives at* the first prism. This step clearly needs a justification, since, as Newton explicitly pointed out, it is *possible*—'as *Grimaldo* supposes'—that, rather than the first prism's simply separating out Rays that already exist within the sunlight with their inherent degrees of refrangibility, those Rays are instead *created* by that first refraction.

Newton seems to make this step in the following way (I attempt merely to describe accurately here, reserving critical analysis for the next section). Concentrate on some particular 'part' of the initial spectrum, say the uppermost part. That part is simply re-refracted in the second prism through one particular angle (rather than being again spread out as was the initial beam of sunlight) and that angle is larger than the angle through which any other part of the initial spectrum is re-refracted:

> So then the Light which went towards the upper end of [the initial spectrum], was (at equal incidences) more refracted in the second Prism, than the Light which tended towards the lower end [of the initial spectrum] [. . .] and therefore was more refrangible (*op. cit.*, p. 37).

And similarly for all the other 'parts' of the initial spectrum: each is refracted through a single, characteristic angle. *Moreover*, now turning back to what happens at the first prism,

> The same light [that is, 'part of light'] was by the Refraction of the first

> prism translated farther from the place [. . .] to which it tended before Refraction; and therefore suffered as well in the first prism as in the second a greater refraction than the rest of light and by consequence was more refrangible than the rest, even before its incidence on the first prism (*op.cit.*, pp. 37–8).

3 Analysis of Newton's 'deduction'

Despite its apparently anachronistic nature,[6] the correct way to analyse Newton's inferences is surely as if they were attempted fully fledged deductions in the modern sense. One could certainly try—as some commentators in effect have—to articulate a distinctive Newtonian 'logic' or method of demonstration at work here. But, in the final analysis, Newton's deductions from the phenomena or proofs by experiment begin with certain explicit 'phenomenal' or observational premises and end with conclusions that are at least somewhat theoretical. This means that the arguments involve deductive gaps. Once these gaps have been precisely characterised, we can attempt to identify the *implicit* assumptions that might plausibly be treated as the necessary gap-filling *enthymemes* or 'hidden lemmas': assumptions that once articulated and added as explicit premises do indeed produce a deductively valid argument. As Frege would have pointed out, if we instead treat Newton as using an ampliative, non-deductive logic, then these enthymemes are nonetheless, but implicitly, involved—being then carried by the logic itself. By insisting on proper standards of proof, the enthymemes are forced into the open, so that their credentials can be inspected.

Just as these general considerations make inevitable, then, Newton's inference is no deduction in the modern sense. As we saw, he explicitly allowed for inductive generalisation in his method. And indeed, not only did he ('horizontally') generalize on the results of his experiments, he also made various 'vertical' generalizations as he went along. For example, step (2) of the inference above clearly involves a major inductive leap: starting from the fact that the parts of light that emerge from the first prism at the greatest angle of refraction are most refracted by the second, those parts emerging from the first prism at the least angle of refraction are least refracted by the second prism, and similarly for all the intermediate parts of the first spectrum, Newton inferred that each part of that spectrum has an *inherent* 'degree of refrangibility'. This is a strong claim that implies that for any two 'parts' of light P_1

[6] 'Anachronistic' only in the sense that Newton did not have available to him the fully spelled-out versions of classical deductive logic, together with its rich meta-theory, that we now enjoy. However, at least so far as issues about what does and what does not really deductively follow from given premises are concerned, it surely is—as my colleague David Ruben likes to put it—'all one big seminar in the sky'. The point I put in Frege's mouth later in the paragraph is universally, timelessly valid—not valid-relative-to-some-time-parameter.

and P_2, if P_1 is refracted more in one transparent medium than is P_2 (for of course the same angle of incidence) then P_1 is refracted more than P_2 in *any other* refraction, either in the same type of medium or in any other transparent medium whatsoever. Various experiments that confirm this general claim—involving three and four refractions and involving prisms, not only of crown glass but also of flint glass, and of glass cases filled with water—are mentioned almost in passing at various points in the *Opticks*, but Newton seems to have taken the general claim as established, at any rate to all intents and purposes, by the crossed prisms result.[7]

An inductive step of a different, and perhaps more interesting, kind is involved at step (3). This step is in fact, as I shall shortly argue, best reconstructed as an application of Newton's first Rule of Reasoning—sometimes known as the '*vera causa* principle'.

Most significantly, aside from such inductive steps, and underpinning the whole argument, are certain implicit 'background' assumptions. Newton's presentation of the argument swings backwards and forwards between empirical-*sounding* talk about the 'parts' of light or about the 'same light', on the one hand, and talk about 'Rays' of light, on the other. The final proposition is explicitly about 'Rays'. Ray is certainly not, in the sense Newton intended, an observational notion—even in a liberal sense of that term. It refers, instead, as we shall immediately see, to the *least parts* that Newton takes light to have; and obviously we do not directly observe such least parts. Any inference from phenomena to such a result must therefore rely—because of the non-content-increasing nature of valid deductive inference—on further implicit assumptions.

The assumptions Newton needed here are in fact introduced on the very first page of the *Opticks* by way of *definition*. 'DEFIN. I' there reads (p. 1):

> By the Rays of light I understand its least Parts, and those as well successive in the same Lines, as contemporary in several lines. For it is manifest that light consists of Parts, both successive and contemporary; because in the same place you may stop that which comes one moment, and let pass that which comes presently after; and in the same time you may stop it in any one place and let it pass in any other. For that part of Light which is stopp'd cannot be the same with that which is let pass. The least light or part of light, which may be stopp'd alone without the rest of light, or propagated alone, or do or suffer anything alone, which the rest of light doth not or suffers not, I call a Ray of Light.

Newton's Definition may seem to carry an operational air, but it is only air.

[7] I thank Teddy Seidenfeld who pointed out to me that Newton's step is even more bold than I initially gave it credit for—there is no consideration, for example, of whether the result might depend on further variables such as, perhaps, the temperature, either of the air or the glass, or on atmospheric conditions or on [. . .] In short, Newton draws the generalisation with no attempt to 'vary the conditions' (as Teddy put it 'his *ceteris paribus* clause was empty!').

The Definition in fact carries the substantive, contingent assumption that light comes in discrete elements, separable from one another both spatially and temporally; and the further assumption that these elements are, when left to themselves, transmitted along straight lines.[8] It may not seem obvious that the 'same Lines' in which Rays successively occur are to be understood as *straight* lines, but this does become clear from the context. Newton plugged the defined notion of 'Ray' into his various optical 'Axioms'. These axioms amount essentially to the principles of geometrical optics—the simple law of reflection and of refraction for example, as they would still be presented in optics textbooks, with, however, one important difference. Newton expresses those principles in terms of Rays that are taken to be discrete entities *travelling along* the rays (i.e. straight lines) of geometrical optics. For example, Newton's 'Axiom III' reads (p. 5): 'If the refracted Ray be returned directly back to [toward] the Point of Incidence, it shall be refracted into the Line before described by the incident Ray.'

This relates, then, to the standard geometrical optics result, applying of course to straightline rays, that if a ray travels from medium 1 to medium 2, and impinges on the separating surface at an angle i to the normal, then, if it is refracted at an angle r, then, a ray travelling 'backwards' from medium 2 into medium 1 and incident at the separating surface at angle r would be refracted into medium 2 at angle i. Clearly, then, in this context the unadorned 'line' meant for Newton *straight* line.

In sum, Newton's 'Definition' and 'Axioms' involve substantive assumptions and without them Newton's proof would not go through. It is only in virtue of these assumptions, for instance, that Newton could track the path of the 'same light' as it passed through various prisms.[9]

So far, then, a couple of straight inductive generalizations and some important background assumptions have been identified as at work in Newton's 'deduction'. Finally, let us return to the tricky inferential leap that took Newton from the sub-conclusion that light consists of differently refrangible Rays *after* it emerges from the first prism to his final theorem that the apparently-undifferentiated sunlight already consists of such Rays.

One interpretation would be that Newton simply cheated here with a blatant *petitio principii*. He simply assumed in making this step that the 'same light' that is refracted by a certain amount in the second prism already existed as a

[8] I assume here that Newton was implicitly committed to the assumption that 'Rays' as he defines them actually exist. As it stands, Newton's definition, like definitions in general does not *explicitly* carry existential commitment.

[9] It might be thought that that identification could be made—at any rate after the first refraction—courtesy of the *colour* of the ray. However, the whole argumentative strategy of the *Opticks* is clearly to establish first results about different refrangibility of Rays, *independently* of any consideration of colour; and only *later* use those demonstrated propositions, together with further results, to establish a rigid connection between degree of refrangibility and colour.

separate component in the observationally undifferentiated sunlight arriving at the first prism. This, however, was exactly what was to be shown. Of course, *if* the rays exist independently in the initial beam, then the two successive refractions show that those rays were refracted by the same relative amounts in each refraction and therefore in particular in the first. But Newton himself, citing Grimaldi, had raised at least one other possibility—that the different 'parts' were *created* by the first refraction. And that means that the antecedent of this conditional cannot be discharged without further ado.

A more generous interpretation reconstructs Newton's reasoning here as an application of his first rule of reasoning (or perhaps a joint application of the first and second rules—the second anyway being presented as a consequence of the first). These rules read (in the Cohen and Whitman translation quoted from Harper [1991]):

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 things of the same kind must be, so far as possible, the same.

Given these rules, Newton could argue as follows. Different refrangibility of different Rays is a *true cause*—once these Rays have been created in the first prismatic dispersion, their different refrangibilities can be established as the causes of the effects observed in second and subsequent refractions (or at any rate this is what the first part of Newton's argument is meant to establish). Different refrangibility of component Rays is also *sufficient* to explain the effects on the sunlight at the first prism: that is, *if* it were assumed that the Rays already exist unseen with their different refrangibilities in the incident sunlight, then the initial prismatic dispersion would be entirely explained (as consisting in fact of a *separation* of the already existing components by directing them along different refracted routes). Hence, although it is of course logically possible that some causal process occurs at the first prism that is entirely different from that occurring at the second, Rule 1 entitles us to infer that this is not the case, and hence that the initial sunlight is indeed a 'heterogeneous mixture' of those differently refrangible Rays.

4 What is the status of the implicit assumptions involved in Newton's proof?

To summarize so far: the non-content-increasing nature of deductive logic entails that 'proofs' of the kind Newton envisaged must rely not only on the explicit 'phenomenal' premises, but also on extra, hidden assumptions. This has—inevitably—been confirmed by the analysis of the particular 'proof from experiments' we considered. The straightforward answer to the—entirely reasonable—question of how genuine theories can be deduced from 'phenomenal' premises is that they cannot, except with the assistance of further assumptions (which must themselves be somewhat theoretical).

The cynic will be quick to point out here that *any* invalid inference can, of course, trivially be 'turned into' a valid one by the addition of further premises.[10] Certainly, so far as the distinctiveness and the power of the method of deduction from the phenomena is concerned, much hangs on the status of these implicit assumptions. Since Newton did not have the advantage of our explicit knowledge of the scope and limits of deductive logic, he did not directly face the question of their status. Nonetheless a plausible reconstruction of his position is, I believe, possible.

Return to the question of what it was that made Newton consistently insist on a sharp division between, on the one hand, his 'positive doctrine' of light, which is part of 'experimental philosophy', and, on the other, the material corpuscular theory, which is a mere hypothesis. Like many historians (see, for example, Sabra [1967] and Cantor [1983]), I have no doubt that Newton believed the material hypothesis to be true and no doubt that the assumption played an important heuristic role in his thinking. Moreover, given the evidence and theoretical understanding then available, he could have produced a strong hypothetico-deductive argument in favour of the material emission theory. In particular, his arguments in Queries 28 and 30 show that the only alternative that had ever been suggested (namely some version or other of the idea that light consists of disturbances transmitted through a medium) seemed impossible to reconcile with known experimental results—rectilinear propagation, dispersion and the polarizability of light (see Worrall [1976]). Nonetheless, he always insisted that while his 'Theorems' about light constituted real 'experimental philosophy', the claim that light is material remained merely hypothetical.

It can be argued (both Sabra and Cantor incline towards this view, for example) that, whatever Newton may have said, there is no real difference between Newton's 'positive doctrine' and the corpuscular theory—that Newton's Rays or 'parts' of light are to all intents and purposes material corpuscles. If this were true and Newton were aware of it, then he would stand condemned of covertly smuggling his own favoured materialist hypothesis (principally under the guise of 'Definition 1') into the alleged empirical demonstration of his 'Theorems'. How might Newton have responded to this charge?

There is at least one place where Newton did in effect recognize that

[10] Or, more correctly, for any given invalid inference from premises $P_1 \ldots P_n$ to conclusion C, a *different* inference can always be produced which (a) is valid, (b) has C as conclusion and (c) has a set of premises which contains the set $\{P_1 \ldots P_n\}$ as a proper subset. (The addition of the single extra premise $P_1 \ldots P_n \rightarrow C$, for example, trivially turns the trick.)

contingent assumptions underpinned inferential steps in his demonstrations. He appended justifications to his 'Rules of Reasoning': for example, he attached to Rule 1 ('No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena') the assertion that 'Nature does not affect the pomp of superfluous causes'. This claim *is* obviously contingent—it is clearly a logical possibility that causal superfluity is rampant. Newton made no attempt to defend this claim. But it seems plausible to suggest that he would, if pressed, have defended it as an assumption that all scientists always make. No scientist would dream of multiplying entities without necessity—that is, if two theories can each account (*properly*) for all the phenomena in some field and if T_2 invokes all the primitive notions that T_1 does and then some more (while T_1 invokes no notion not also invoked by T_2) then the scientist will always prefer T_1. Hence every such scientist is committed to the non-pompous view of nature. It may be a substantive, non-analytic claim about the world, but it nonetheless seems to be a fixed part of background knowledge in science.[11]

Does our imaginary Newton facing this modern, properly deductive analysis of the real Newton's 'demonstrations' have a similar defence concerning the assumptions underpinning his 'Definitions' (Definition 1 in particular) and his 'Axioms'? His comment about Rule 1 surely points us in the right direction: these other assumptions too are not sensibly classifiable as conjectures, but rather formed part of generally accepted background knowledge at the time. This defence—as simply a description of the contemporary state of belief—is, perhaps surprisingly, far from implausible.

The laws of geometrical optics—principally the law of reflection at a plane mirror, and the Snell–Descartes law of refraction—amount essentially to Newton's 'Axioms' and they were certainly accepted, as 'background knowledge' on all sides. The more difficult issue concerns 'Definition 1' which, as we saw, in effect carries the assumption that, whatever the true basic nature of light, it comes ultimately in discrete parts that are, when left to themselves,

[11] Of course, this principle cannot be interpreted as true, let alone as generally accepted, unless proper attention is paid to the requirement that the phenomena be explained *properly*. It would, for example, clearly be simpler so far as Darwinian theory is concerned if *every* characterisitic of every organism were an adaptation—yet Darwinians in fact presuppose multiplicity of causes—drift, for example, as well as adaptation. But this is because a sensible interpretation of the evidence *does not allow* the simpler account. Hence it is *necessary* to invoke this multiplicity of causes. Similarly the principle when correctly interpreted does not sanction any empiricist reduction of theories (Craig's theorem does not show that all theoretical assumptions are 'unnecessary' in the appropriate sense of necessity). As I interpret it, the principle bans only the suggestion that—unknown to us and beyond the reach of all evidence—multiple 'causes' operate (somehow conspiring in all cases to hide their separate effects) where one cause would equally—or in fact better—explain all phenomena. A classic example is the variety of causes that classical physics had to invoke which just happened to cancel out the effects of the motion of the earth through the ether to which classical physics was centrally committed. (Critical comments by Carl Hoefer—about the Darwin example, and by Jeff Ketland brought home to me the need to make this clarification.)

propagated rectilinearly. Given that Newton had at least two contemporaries—in the persons of his arch-enemy Hooke and the more formidable Huygens—who held versions of the mediumistic theory of light, it might seem extremely implausible that any such assumptions could sensibly be classified as 'accepted background knowledge'. However, Hooke in particular struggled to make his account of the nature of light consistent with these assumptions—making light consist, not of continuous uniform waves, but rather of irregular, discrete 'pulses' which follow essentially rectilinear paths. Of course, and as Newton was fully aware, it is not at all clear that Hooke's assumptions about his 'pulses' are consistent with the laws of mechanics, but Newton, not unreasonably, regarded this as Hooke's problem not his. Although Hooke believed that light consists of pulses transmitted through an all-pervading medium, while Newton believed that it probably consists of material particles, they both believed that light comes in 'parts' and that, at any rate most of the time, these parts follow rectilinear trajectories. (Huygens, too, perhaps surprisingly, shared at least the assumption of rigid rectilinear propagation[12] and, if he did not commit himself to a Hooke-style discrete 'pulse' view, he also did not hold the view of light as continuous periodic motion.)

Whatever the real Newton may have been up to, then, there is at least a plausible reconstruction of the debate which sees him as playing fair with Hooke: the question of whether or not light is material was a controversial one, not one settled by generally accepted background knowledge, hence he would never presuppose a positive answer to it (no wonder then that the real Newton was enraged by Hooke's suggestion that the material hypothesis amounted to his 'first supposition'); he could not provide an empirical demonstration of the claim that light consists of material particles and so, perfectly properly, branded it a hypothesis, even though all the signs are that he believed it to be true; on the other hand, all the presuppositions that are in fact invoked in his 'deduction from the phenomena'—whether of the form of the general metaphysical assumptions that underpin the rules of reasoning or of the more specific claim that light consists of discrete parts obeying the laws of geometrical optics—were arguably uncontroversial between himself and his opponents.

[12] The folklore of science talks of the Fresnel–Huygens secondary wave construction—a construction that yields rectilinear propagation as merely an approximation that happens to be empirically adequate in a range of situations. However, this construction—as it appears in presentday optics textbooks—is in fact entirely due to Fresnel. Huygens assumed that his secondary waves are 'infinitely feeble' except where they 'conspire' and they conspire only along their common tangent. This is *both* inconsistent with mechanics—although Huygens himself did not of course recognize this—*and* yields rectilinear propagation as a direct consequence. (See my [1976].)

5 So *is* Newton's method distinctive and, if so, what can it deliver?

I now come to the central philosophical issue of this paper—that of whether this analysis of Newton's 'demonstration' reveals a general method of accrediting theories that is both distinctive and powerful. In outline my—disturbingly Blairite, 'third way'—answer is that Newton's method is *somewhat* distinctive and *somewhat* powerful: both Newton himself and his current followers have perhaps tended to exaggerate the virtues and power of what is undoubtedly an important idea.

5.1 The distinctiveness of 'deduction from the phenomena': heuristics

Newton's method is clearly different from any hypothetico-deductive (H-D) approach *from the point of view of heuristics*. Assuming that H-D approaches are automatically committed to the position that there are no prior constraints on hypotheses, that there is no systematic way of constructing theories likely to be confirmed, that all a scientist can do is make a bold conjecture and hope that it withstands empirical testing, then Newton's account is irreducibly different. This is because the account insists that scientific theories can be derived from 'background' principles plus observational or experimental results. Of course, these background principles themselves no doubt count as hypotheses in the trivial sense that, no matter how firmly entrenched they may be, they transcend purely observational results (this problem will be taken up in Section **5.2**). But a scientist is, descriptively speaking and give or take a little vagueness, *presented with* accepted background knowledge—she cannot simply 'conjecture' a piece of it.

One way, then, in which Newton's method might be defended as a superior method for science is on methodological or heuristic grounds. The chances of arriving at a theory that survives severe empirical tests by simply lying on one's couch and making 'bold conjectures' are infinitesimal. Instead theories are worked towards systematically in ways of which their inventors are no doubt not always fully conscious, but which are always *reconstructible* as some sort of derivation. Indeed there is a growing consensus in recent philosophy of science against the idea that arriving at theories is a matter of genius unconstrained by, and unanalysable in the light of, logical principles.[13] Even

[13] Of course, as was long known to practitioners and as has been established by results in mathematical logic, finding a deduction of some theorem from given axioms or premises is itself a matter of intuition, often requiring 'genius' (there is no algorithmic decision procedure for theoremhood in first order systems, though there is for proof—that is, proof *checking*). Recognising that some proposition is indeed a theorem of some axiomatic system is clearly in some sense an outstandingly creative act (mathematicians regard intuiting that some proposition

Popper, who sometimes seemed to be the most forceful defender of the 'no logic of discovery' view also sometimes wrote that no scientist will ever arrive at a worthwhile theory unless 'fully immersed in the problem-background'. This is clearly true but would be quite mysterious if the path to the new theory were, as Popper insisted following Reichenbach, entirely immune to logical reconstruction (see in particular Worrall [1995] and [1999a]). Clearly there is an argument that a theory that can be deduced from background knowledge plus new empirical information is much more likely to be empirically adequate than some claim simply plucked out of the air as a 'bold conjecture'. For one thing, the deduced theory is at least bound to entail the new empirical information that supplies the premises for its derivation.

Newton did make disparaging remarks, along 'needle in a haystack' lines, about the 'method' of hypothesis. But he surely could not have believed that the superiority of his method is *merely* a matter of heuristics. After all, however incredible it might seem that plausible hypotheses are merely 'plucked out of the air' rather than arrived at systematically from prior information, the material emission hypothesis was as a matter of fact already around at the time of the *Opticks*—it had already been arrived at (however mysteriously) and it arguably passed certain tests (ones that, as Newton cogently argued in the Queries, the wave theory as then developed failed). Yet he was quite explicit that this theory was to be denied access to the realm of chaste experimental philosophy. Newton's view must have been that, not only is building new theories out of what is already known much more likely to produce good theories, it also gives that theory much stronger *accreditation* than it could receive simply from passing observational or experimental tests, no matter how numerous.

5.2 How distinctive is 'deduction from the phenomena'?: accreditation

5.2.1 The inductive steps

Newton's method certainly appears at first glance quite distinct from anything of an H-D character. Whereas H-D methods start with the theory and proceed by investigating the truth of observational consequences deduced from it, in Newton's method the theory is the *conclusion* of an argument that begins with observational premises. But once we begin to analyse the method more closely

is a theorem as the mark of genius, filling in the details of the proof, hack work—remember Gauss's celebrated remark 'find me the theorems, and I shall soon find the proofs!'). But what else can a great mathematician be doing when recognising that proposition P is a theorem, but somehow—and clearly in large part subconsciously—going through some mental process that amounts to the construction of a sketch-proof for P?

then that distinction blurs. First, of course, H-D methods usually involve some sort of (invariably vague) 'acceptance rule' that sanctions the 'acceptance' as part of accredited science of a theory that has passed sufficiently many tests. So even there we do—in a sense—end up *inferring to* the theory. It may appear that the conclusion of Newton's demonstrative method is stronger—the theory itself, rather than a meta-level (and again rather vague) conclusion about the theory's 'acceptability'. But once we take into consideration the (at least in principle) corrigibility of the conclusion—and Newton, as we shall see, *did* allow that his 'demonstrated' conclusions might later turn out at least to need some modification—any difference between the two accounts seems in danger of collapsing into a merely formal one.

A second way in which the distinction between the two sorts of method tends to blur once they are looked at more closely concerns the inductive steps involved in Newton's 'demonstrations'. John Norton—in the most recent and perhaps most challenging defence of the method (see his [1993], [1994], and [1995])—stresses the difference between demonstrative induction (Newton's method) and mere 'instance' or 'enumerative' induction, which he uncontroversially regards as dubious. But all these deductions or demonstrative inductions start from *generalized* phenomena. And it is difficult—at any rate in the case I analysed—to see anything other than enumerative induction at work in arriving at the required generalizations.

Newton in effect took it that because (i) the Rays (the 'red-making' ones) refracted least in one refraction were again refracted least in the second refraction, those most refracted in the first again most refracted in the second, and so on, and, (ii) this happened in prisms of flint glass and of water, as well as in the crown glass prism he used initially, it followed that this always holds for any transparent medium. But how does this differ from 'mere' enumerative induction? What if there were a transparent medium—call it magic glass—that Newton had not investigated and which reversed the order of refraction? For those of the robust 'no time for trips to philosophers' wonderlands' persuasion, I should add that, although I sympathize in general, the problem in this particular case is that magic glass turns out to exist—though it was only discovered much later. In fact it exists in several forms—one is the dye fuschine, another is iodine vapour. A prism of either material will deviate the red rays from sunlight more than the violet ones. (More of this later.)

One could easily dress up the argument from the instances (actual experimental results) to the generalized statement of the phenomena as itself a deductive inference invoking the sort of general assumption underlying Newton's 'rules of reasoning', but again the difference between such an inference and the corresponding H-D account of the acceptance of such a generalization on the basis of tests threatens to be purely formal.

A similar difficulty arises concerning the more interesting and challenging inductive step in Newton's proof that invokes the *vera causa* principle. William Whewell argued long ago in effect that this principle is closely linked to the requirement of *independent testability*—a requirement that is clearly a necessary part of any half-way adequate version of hypothetico-deductivism. Whewell's position, in somewhat more up-to-date terms, seems to be essentially this: the condition that some 'cause' introduced to account for some phenomenon be *sufficient* translates simply into the requirement that the theory postulating that cause entail the phenomenon (of course in conjunction with accepted auxiliaries); while the requirement that it be a *true* cause, although it might sound stronger, can surely, on analysis, amount to no more than the requirement that the causal theory also explain some independent phenomenon or phenomena. Newton sometimes gave the impression that causes can, at least in some circumstances, be directly observed (we know that gravity is a true cause because we 'feel' its pull). But this is not serious: all our causal knowledge—at any rate in physics—is mediated by theory, the requirement that a 'cause' (gravity, say) introduced to explain some phenomenon (the motion of the moon, say) is a *true* cause can only amount in the end to the requirement that the theory of gravity has passed tests in areas other than that of the moon's motion.

But, if this is true, then it seems difficult to view the following two modes of reasoning to Newton's different refrangibility proposition as much more than notational variants:[14]

1. T [Newton's different refrangibility proposition] states that causal mechanism M [separation of the components by dispersion] operates at prism 1 to produce effects E [the elongated spectrum].

Effects E occur and moreover causal mechanism M exists—it is a 'true cause' [it produces the effects E′ observed at prism 2].

If T were not true, then some other causal mechanism M′ would produce the effects E at the first prism.

No other empirical data require M′.

Nature does not affect the pomp of superfluous causes [that is, do not invoke M *and* M′ unless this is required by the data].

Therefore causal mechanism M operates at prism 1 to produce E, and so T.

2. Theory T entails that M operates at prism 1 to produce E.

Theory T also entails that the same causal mechanism M will operate at prism 2 to produce effects E′; and in fact E′ [i.e. T is independently tested and independently 'verified'].

[14] I thank Nancy Cartwright for criticisms of an earlier version that led to the following more careful articulations of the two modes of reasoning at issue.

T is moreover inconsistent with no other evidence and is the most parsimonious explanation of all the evidence.

Therefore, T should be 'accepted' in science; and therefore accepted science tells us that what happens at prism 1 is that causal mechanism M operates to produce E.

2 is, I take it, sophisticated hypothetico-deduction (with an 'acceptance rule'). **1**, I take it, is a fully spelt out version of this part of Newton's deduction invoking the *vera causa* principle. The difference seems to be purely presentational. **1** simply turns the independent evidence for T (it correctly predicts the results E' at the second prism) into the real existence of the true cause M (it is of course T that entails that M operates at the second prism); it turns the implicit premise underwriting the admittedly vague acceptance rule invoked in **2** into an explicit premise; and hence converts the H-D account into an inference. Again, *any* argument can of course be turned into a valid deduction by adding enough premises. It is not, however, always clear exactly how much is achieved by this.

5.2.2 The dependence on 'background knowledge'

Despite the foregoing, Newton's method *is* importantly distinct from anything hypothetico-deductive because of its insistence that theories be deducible from premises *all of which* (and not just the very general ones underwriting the various inductive steps) are parts of background knowledge. This insistence makes a difference (one that redounds to the credit of the Newtonian method) in terms of heuristics, as already argued. But, as we also saw earlier, Newton was clearly committed to a stronger view: that trying to deduce theories from the phenomena is not only a better discovery method, it also, if successful, gives such theories much stronger accreditation than they could achieve by passing any number of tests. How exactly?

The threat, of course, is that all that is achieved by deducing a theory from the phenomena—really deducing it from phenomena, *plus* background principles—is that the accreditational buck is simply passed to the background principles. The most obvious accreditational question facing Newton's method is, in other words, how the necessary 'background knowledge' assumptions are themselves accredited. Given the non-ampliative nature of deduction, then the answer—on pain of infinite regress—cannot be by a further deduction from the phenomena. John Norton, although a strong advocate of 'demonstrative induction', acknowledges that some ampliative inference is needed at this point:

> It must be stressed that the flight to demonstrative induction does not and cannot free us of the need to employ ampliative inference. Typically ampliative inference will be needed to justify the [background] principles of greater generality ([1994], p. 12).

But if it turned out that the ampliative inference involved is essentially inference to the best explanation—that is, H-D with a vague 'acceptance rule'—then deduction from the phenomena might, it seems, ultimately be better described as an adjunct to H-D rather than a replacement for it.

Consider the most straightforward case—that which might be called intra-paradigm or intra-research programme deductions. Suppose a scientist already accepts the general wave theory of light—the theory that light from any particular source consists of waves of some wavelength or other transmitted through the luminiferous aether. This general theory does not specify the wavelength of any particular kind of monochromatic light—say, light from a sodium arc. The scientist would like a more specific theory that specified that wavelength. Does she lay on her couch and make a Popperian bold conjecture? Such an attempt to find a needle in an infinite haystack would be entirely ill advised. Instead she would *deduce* the specific theory, involving the specific value of the wavelength, from the phenomena. She would look for some consequence, e, of her general theory T, where e specifies some observable magnitude (fringe distance in some particular experiment, say) as a one-to-one function of the wavelength. She would perform the experiment using light from a sodium arc, measure the magnitude at issue—here, the fringe distance (call the result of this measurement e')—and infer to a more specific theory T'.[15]

But, although in such a situation, e' will give a conclusive reason for holding T', *given the general theory T*, this definitely does no more than pass the accreditational buck back to that general theory. T' will, of course, be further tested by drawing a further now very specific consequence e'' from it—specifying the fringe distances, say, in some *different* diffraction experiment using the same light source. But, in either case—whether we describe this as an instance of a general theory T being tested by a conjunction of e' and e'' or an instance of a specific theory T' being deduced from e' and then tested (as 'representative of T') by e''—H-D testing seems ultimately to be involved.

Defenders of Newtonian method will (plausibly) counter that the general principles from 'background knowledge' involved in Newtonian deductions or 'demonstrative inductions' have a quite different status from even general theories—like 'the' wave theory of light. For example, the claim that light is

[15] So, for example, subject to a couple of idealisations, one can derive from the general wave theory that, in the case of the famous two-slit experiment, (observable) distance X from the fringe at the centre of the pattern to the first fringe on either side is related to (theoretical) wavelength λ, *via* the equation $x/(x^2 + D^2)^{1/2} = \lambda/d$ (where d is the distance between the two slits and D the distance from the two-slit screen to the observation screen—both of course observable quantities). It follows analytically of course, just by manipulating the equation, that $\lambda = dx/(x^2 + D^2)^{1/2}$. But *all* the terms on the right hand side of this last equation are measurable. Hence particular observed values will determine the wavelength and so the more specific theory T'. (It was presumably this sort of thing that van Fraassen had in mind in referring, in his [1980], to observation as 'theory construction by other means'.)

propagated rectilinearly cut across, and was common to, all sensible general theories in the mid- to late seventeenth century. Here I think it should be acknowledged that a Newtonian deduction, whatever its accreditational value from a logical point of view, may have great accreditational value *psychologically speaking*. The demonstration that a specific theory or theoretical claim, T, is deducible from observational statements plus background knowledge shows something more than simply that the specific theory passes certain tests. The deduction also shows that, contrary to what might seem to be case initially, T in fact *stands or falls with the background knowledge.*

The arguments in the *Principia* show that in order to challenge the seemingly 'bold' claim that every material particle in the universe is attracted to every other with a force inversely proportional to the square of the distance between them, and proportional to the product of their masses, one must—in view of Kepler's phenomena, and whether one is aware of it or not—be ready to challenge some *very* basic, apparently far from bold assumptions, either of the very general kind that 'nature does not affect the pomp of superfluous causes', or like Newton's second law of motion (or perhaps still deeper assumptions such as that of the conservation of momentum).[16] Similarly, the arguments in the *Opticks*—if we take the 'phenomenal' premises for granted—show that if quite specific claims about sunlight—for example that it consists of 'Rays differently refrangible'—are to be challenged, then so must some very general and fundamental assumptions about light. Basically, we must be ready to challenge the assumption that light comes in discrete 'parts' which if left to themselves are propagated in straight lines—an assumption that was arguably generally accepted and deeply entrenched at the time. The very specific claims—surprisingly (of course surprisingly for optical investigators *at the time)*—stand or fall with the very general ones.

5.3 The scope and power of deduction from the phenomena: Isaac Newton and John Norton on certainty

If we lay aside the issue of any possible fallibility of the phenomenal premises, a successful deduction from the phenomena simply transfers whatever epistemic status the general 'background knowledge' principles are considered to have to the specific theory that forms the deduction's conclusion. If the background principles are considered to be to all intents and purposes *certain* then so will be the 'demonstrated' theory. But the converse also holds: the claim of certainty for the demonstrated propositions will be justified *only* if the necessary premises taken from background knowledge are certain. Advocates of the method are committed implicity to some claim that these background

[16] For an especially clear treatment see Zahar ([1989]).

principles are indeed at any rate *more* likely to be true (or, speaking more pragmatically, more likely to be a fixed part of science, less likely to be corrected later) than any hypothesis—no matter how well tested, and hence that a 'demonstrated' theory is similarly more likely to be true (or fixed). Only some such claim of relative certainty could justify the asceticism of Newton's method: remember that Newton had a good H-D argument for the material emission theory and yet insisted that, unlike his demonstrated propositions, it remained hypothetical. Admission to the realm of chaste 'experimental philosophy' is restricted to demonstrated propositions, which must then differ from (even tested) hypotheses in terms of (degree of?) certainty or fixity.

Some remarks suggest that Newton was ready to go all the way and claim that his method produced—at any rate 'moral' or pragmatic—certainty. For example: 'Tis much better to do a little with certainty and leave the rest for others that come after you than to explain all things by conjecture without making sure of anything' (MSS. Add. 3970–3. f.480v; quoted from Westfall [1980]). Colin MacLaurin, one of the most insightful of his immediate successors, similarly asserted that the great virtue of Newton's distinction between demonstrated propositions and hypotheses and of his inclusion of only the former in his 'experimental philosophy' was that Newton thereby 'secured his [experimental] philosophy against any hazard of being disproved or weakened by future discoveries' (MacLaurin [1775], p. 10).

Throughout the nineteenth century this seems to have been the general view—propositions that were presently only hypotheses might of course be properly 'demonstrated' later once the base of phenomenal premises had been extended, but equally might well turn out later to be radically false; only a proposition that had been demonstrated by the method discovered by Newton was safe from all possibility of later rejection.[17]

Newton's method is more restrictive than any H-D view—because you cannot simply 'conjecture' a principle of background knowledge, it has to be there waiting for you in generally accepted science—and the pay-off for this asceticism is supposed to be (at least *greater*) certainty. Two main questions then arise: (i) the general philosophical question of whether or not this is a tenable view, and (ii) the historical question of how far, and how consistently, Newton himself held it. In the course of answering these two questions, I shall also need to address the further specific issue of whether it is plausible to claim that, whatever the general merits of the case for certainty, (iii) Newton's different refrangiblity proposition can still be regarded as certainly established—at any rate 'to all intents and purposes'.

In some interesting recent papers ([1993], [1994], and [1995]), John Norton

[17] For a detailed discussion of the nineteenth-century view of Newton's method—especially the view of the 'common-sense' philosopher Thomas Reid—see Worrall ([1999b]).

has advertised the merits of the methods of demonstrative induction or eliminative induction—both simply forms of the method of deduction from the phenomena.[18] Norton has, in particular, argued the following striking thesis:

> In common scientific practice, near certainty is accorded to the basic principles of a mature science and this certainty is said to be based on experimental evidence. I [. . .] show how two related forms of inference, demonstrative induction and eliminative induction can be used to support judgments of this type ([1994], p. 3).

Norton, surely correctly, claims that the massive underdetermination of theory by data much discussed by philosophers is—at least in many cases—diametrically opposed to psychological reality so far as scientists are concerned. As has been pointed out many times, physicists are often hard pressed to find even one theory that fits all the data, let alone indefinitely many; and, more significantly, Norton argues, there are occasions when scientists believe that the evidence picks out a *unique theory* T. Such a belief can be explained by showing that T is indeed deducible from the phenomena—meaning of course that T is deducible from the phenomena *plus 'background' material that those scientists regard as already established*.

As already indicated, I have no doubt that 'deduction from the phenomena' is *in fact* an important mode of inference in science; and it seems to me that Norton's account is correct as an *explanation* of the historical fact that scientists sometimes accord near certainty to theories—of why they sometimes regard such a theory as uniquely determined by the phenomena. That is, Norton is correct that scientists have this attitude towards a theory exactly when it can be 'deduced from the phenomena'. However, as philosophers, we must surely also ask whether or not scientists are *justified* in believing in the certainty of theories on this basis.

The above analysis of Newton's deduction from the phenomena shows unsurprisingly that a theory deduced from the phenomena is exactly as certain as the conjunction of the explicit phenomenal premises *and the implicitly presupposed background knowledge.* Hence taking, for the sake of argument, the phenomenal, experimental premises as certain, scientists are justified in believing in the certainty of the theories demonstrated in this way if and only if they are justified in believing in the certainty of the underlying background assumptions.

One robust response to this obvious point might be: *of course*, the inferences rely on other assumptions—just as the claim that we can effectively be certain that the meter-needle now points to 5 implicitly relies on the assumption that

[18] So for, example, an argument by eliminative induction works in the special case where 'background knowledge' entails that one of a finite disjunction of specific theories holds, the 'phenomena' then refute all but one of these and hence the remaining specific theory is 'demonstrated'.

we are not being systematically deceived by a Cartesian demon; but nonetheless the method produces certainty or essential certainty, so presumably these other assumptions *must* themselves have that character, whatever fancy fantasies the philosopher can dream up to challenge this. Unfortunately the robust response is untenable in the particular case we analysed (and *a fortiori* not generally tenable)—not on account of any general philosophical quibbles about certainty, but because of facts about the subsequent development of science.

As we saw, Norton's claim echoes some of Newton's own and some made on his behalf by later commentators. In the specific case of the positive doctrine about light, and in particular the proposition whose proof we examined that sunlight consists of 'Rays differently refrangible', several recent commentators have also claimed that Newton did indeed deliver certainty—at least to all scientific intents and purposes. The over-fussy logician may insist that Newton made assumptions that cannot be strictly verified but none the less the result that he arrived at, if not logically certain, at least, as Howard Stein puts it, 'stands uncorrected by the whole subsequent history of science'. This echoes the view of R. S. Westfall who wrote ([1962], p. 49):

> Newton successfully incorporated the heterogeneity of light into optics as a basis for a new theory of colours, and this aspect of his work has survived successive revolutions in optics substantially unchanged.

The view is directly contradicted, however, by another eminent historian, A. I. Sabra, who argued in his ([1967], Ch. 11) that Newton's doctrine of the 'original heterogeneity' of light is, properly understood, inconsistent with the classical wave theory and hence, far from being a fixed part of positive science that 'survived successive revolutions in optics substantially unchanged', was in fact rejected in the very first post-Newtonian 'revolution' in optics.

The view endorsed by Westfall and by Stein is difficult to sustain. Apart from the considerations of a conceptual, theoretical kind raised by Sabra, there are subsequent experimental discoveries that entail that Newton's proposition—on any reasonable interpretation—is empirically false (discoveries of which Westfall and Stein seem to have been unaware).

Newton's proposition that the constituents of sunlight have different inherent refrangibilities means that the amounts of refraction undergone by those constituents on entering *any* transparent medium, although of course differing in absolute amount depending on the refractive index of the material, are nevertheless always ordered in the same way. That is, the Rays (the 'red-making' ones) refracted least in a prism of, say, crown glass will also be refracted least in a prism of any other transparent medium; the Rays (the 'violet-making' ones) most refracted in the first prism will be most refracted by any second prism and similarly for all the intermediate Rays. Some inductive support for this claim can be found in the *Opticks* in the form of confirmations using not only prisms made of crown glass, but also ones made of flint glass and filled with water.

But, as already pointed out, to argue for the general claim on the basis of the instances is to argue by 'mere' enumerative induction. What if there were a medium—call it magic glass—that reversed the order of refraction? The existence of magic glass would mean that, contrary to Newton's conclusion, an inherent degree of refrangibility could not be attributed—as a monadic property—to given Rays, contrary to the clear implication of his proposition; the degree of refrangibility would instead be a *relational* affair between a type of ray and a type of transparent material. This a-inductive suggestion might appear a typical philosopher's fantasy: only philosophers have time for trips to the wonderland of magic glass, scientists have far more pressing demands on their time. Although I am in general sympathy with this sort of robust reaction, the problem in this case, as suggested earlier, is that magic glass was found later to exist—in fact in several different forms. One form is the dye fuchsine; another is iodine vapour. A prism filled with either material will deviate the red rays from sunlight more than the violet. Indeed, all transparent materials, including ordinary crown or flint glass are, in a sense, forms of 'magic glass'. Speaking in modern terms, *all* transparent materials selectively absorb certain wavelengths in the electromagnetic spectrum, and, in the neighbourhood of the absorption region, the normal dependency between wavelength and degree of refraction is reversed. Science discovered this only long after Newton simply because the absorption bands of relatively few materials happen to lie in the visible region.

Moreover, Newton's proposition clearly entails that each identifiable 'part' or Ray of light has its own inherent degree of refrangibility, not only in the ordinal sense just explained but also in the sense that the Ray retains that degree of refrangibility through any number of refractions or—as later results in the *Opticks* are taken to demonstrate—reflections. The 'parts' once separated by prismatic dispersion have *intrinsic* unmodifiable 'degrees of refrangiblity'. But in fact, as science later discovered, a ray's degree of refrangibility *can be* modified—for example, by the Doppler effect produced by reflecting it from a moving mirror.[19]

[19] I am indebted hereabouts to the helpful comments of an anonymous referee. (Another phenomenon that might be thought to refute Newton's claim is that of *fluorescence*. Certainly a monochromatic ray enters a prism of fluorescent material with one 'degree of refrangibility' and leaves it with quite another. However, in that case, since the emergent ray, rather than being the result simply of the properties of the field within the medium, is produced by the vibrations of the material's atoms induced by the incoming light, it is more accurate to say that the emergent and incoming light are not 'the same ray'—so that this is not a case of one and the same 'Ray' having its refrangibility altered by some interaction.)

Although Westfall does not mention these phenomena, it is conceivable that they formed the reason for his claiming only that Newton's proposition survived subsequent revolutions 'substantially' unchanged—Rays of light exhibit the same order of refrangibility and retain their degree of refrangibility in all but a few 'exotic' media, like fuchsine or iodine vapour or fluorene and in all but a few 'exotic' operations such as reflection from a moving mirror. I would certainly dispute even this weaker interpretation—although it is true, as pointed out, that very few transparent materials happen to exhibit 'anomalous dispersion' in the visible region, the phenomenon when properly understood shows that, quite contrary to Newton's proposition and *in general,* the order of refraction of different elements of the solar spectrum is not dependent simply on the element of light itself (on its wavelength in more modern terms) but also on the medium; the Doppler effect shows that, quite contrary to Newton's proposition, a Ray's degree of refrangibility is not intrinsic to it but can be changed by reflecting it from a moving surface. Moreover it can be argued that, aside from these empirical refutations, a fundamental revision of a *conceptual* kind was involved in the very next 'revolution' after Newton.

Newton clearly thought of the 'Rays of different refrangibility' (and therefore of course of the different colours) as separately existing with their own identities within the sunlight. He explicitly asserted that these separate Rays do not even interact with one another. Proposition 6 of his reply to Huygens, for example, states: 'The rays of light do not act on one another in passing through the same medium' (*Correspondence,* **I**, p. 293). Newton several times drew analogies between white light and a mixture of differently coloured powders—a mixture that might appear white at a distance but which, when examined more closely, is seen to consist of parts each with its proper colour.[20] And—most clear-cut of all—he provided the following summary of some of the main results of the *Opticks* as follows:

> It has been proved [...] that when several sorts of Rays are mixed, and in crossing pass through the same space, they do not act on one another so as to change each other's colorific qualities [...] It has been shewed also that as the sun's light is mix'd of all sorts of rays, so its whiteness is a mixture of the colours of all sorts of rays; these rays having *from the beginning* their several colorific qualities as well as their several Refrangibilities, and *retaining them perpetually unchanged* [...] (pp. 159–60; emphases added).[21]

[20] Not, of course, that he intended this analogy to be taken all the way; he did not believe that a microscopic analysis of 'white' solar light would reveal the different colours—only prismatic dispersion did this.

[21] *Cf. Opticks*, p. 244: 'Whence it follows, that the colorifick Dispositions of Rays are also connate with them, and immutable; and by consequence, that all the Productions and Appearances of Colours in the World are derived, not from any physical Change caused in Light by Refraction or Reflexion, but only from the various Mixtures or Separations of Rays, by virtue of their different Refrangibility or Reflexibility.'

But this account of sunlight as consisting of a *mixture* of pre-existing separate, differently refrangible components that are simply directed along different paths in prismatic dispersion has been argued by Sabra and others to be straightforwardly inconsistent with the classical wave account, developed in its clearest form by Gouy in the 1880s. According to this wave account, the prism effects the physical equivalent of a Fourier decomposition of the incident white light. The natural thing to say seems to be that all that exists prior to the dispersion is one single complex wave train. Even had that complex wave train been produced in the first place by the *superposition* (*not*, of course, mixture) of monochromatic sinusoidal waves generated by simple harmonic oscillators in the sun, the best one could do towards reconciling this with Newton's account would be to say that those sinusoidal components are *re*-created by the prismatic dispersion—since those 'components' had on the wave account fully merged and lost their separate identities once they had superposed. But in fact there was no commitment to such a view of the generation of the irregular wave train producing the white light. *First*, of course, it provably matters not at all how the complex wave train that is the white light was created—whether the creation was by superposition of harmonic oscillations or of irregular, non-harmonic oscillators, the prism will always produce the monochromatic, sinusoidal 'components'. But, *moreover*, in so far as any clear-cut view about how sunlight was produced was held in the Nineteenth Century (the wave theorist needed to have no view on the matter) it was the irregular non-harmonic view: it seemed indeed massively improbable that the sun contains only harmonic oscillators and oscillators of all frequencies present in the eventual spectral decomposition.

Although the issue is not as clear-cut as one might like (Michael Redhead convinced me that one might still like to talk of the monochromatic components as 'present' in the complex non-monochromatic wave), it at least shows that some important conceptual revision in science's view of the nature of white light is involved in the shift from Newton's theory to Fresnel's. And this, together with the empirical refutations discussed earlier shows that the only way to see Newton's 'different refrangibility' proposition as surviving successive scientific revolutions 'substantially unchanged' is by interpreting it, quite contrary to Newton's intentions, as simply a codification of a certain limited range of phenomena. It is of course true that we still believe that if the same experiments Newton described are performed today (with exactly the same materials) they will have the same results as always. The fascination of Newton's method, however, lies in its apparent promise of a limited degree of transcendence of the phenomena, while conserving certainty—at least certainty 'to all scientific intents and purposes'. If we interpret Newton's proposition in the way that he intended, then not only has it not survived successive scientific revolutions, it fell at the first fence—the very first post-Newtonian 'revolution' in optics saw its rejection, though it continued, of course, correctly to codify those phenomena it had always codified.[22]

Given the above analysis of the method, the fact that Newton's proposition turned out to be false must reflect the fact that those elements of background knowledge on which he relied turned out to be—strictly speaking—false. And the fact that the proposition is strictly speaking false already from the point of view of the early nineteenth century wave theory must mean that the background knowledge assumptions were themselves overturned by the wave theory. This is indeed correct: light, according to the classical wave theory, has, of course, no discrete 'parts' that are, when left to themselves, transmitted along straightline paths. Although scientists continued to talk about rays in the sense of geometrical optics[23] after the wave 'revolution', the only fully legitimate sense of the term within the wave theory is simply as a mathematical direction along which light energy is transmitted: no individual, self-identical 'part' of light is transmitted along such a ray according to the Fresnel's theory, which entails instead that the optical disturbance along a given straight line is constantly reconstituted as the vector sum of disturbances emanating from *all* previously disturbed parts of the light-carrying medium and not just those along the line itself. (Of course the later theory explains why we cannot see round corners and through bent opaque tubes, but it does this, *not* by entailing the laws of geometrical optics, but rather by entailing that those laws are always strictly incorrect but none the less empirically adequate in a range of everyday circumstances. Light is *never* propagated rectilinearly according to the wave theory, but the diffraction patterns that really occur are, in a range of circumstances, empirically indistinguishable—or almost empirically indistinguishable—from the predictions of geometrical optics.)

The principle of rectilinear propagation and the other 'laws of geometrical optics' in fact turned out to be, not truths, but only empirically adequate approximations in certain domains of phenomena. Just as in the case of Newton's 'different refrangibility' proposition, something of them is of course retained through later developments in science, but certainly not the fully fledged 'laws' themselves.

6 Conclusion: where to go from here?

The idea that Newton's method can really deliver certainty—or, its pragmatic equivalent, effective incorrigibility—is, then, sustainable neither

[22] It might also be argued that Newton's proposition remains 'approximately structurally' correct—though perhaps only because of the unfortunate flexibility of the notion of structure pointed to by several critics of 'structural realism'.

[23] These are, recall, very different from Newton's 'Rays' which he took to be discrete parts of light that *travel along* the rays as understood in geometrical optics.

philosophically (this is of course hardly news) nor (more interestingly) practically. And, despite the occasional overstated remark, Newton himself in his more-considered moments seems to have agreed. In 1672 he had insisted that his positive doctrine of light is a 'most rigid consequence' of the phenomena, 'evinced by ye mediation of experiments, concluding directly and without any suspicion of doubt'. By the time of the second English edition of the *Opticks*, however, he was ready to allow (in Query 31) that a deduction from the phenomena is not demonstrative in the geometrical sense of producing absolute certainty; he continued to insist, though, that it is 'the best way of arguing that the Nature of Things admits of.' This 'best way' produces conclusions that may *temporarily* be 'pronounced generally'. But he acknowledged that experimental exceptions to these conclusions *may* turn up later, whereupon the general theory 'may then begin to be pronounced with such exceptions as occur.' This concession to fallibility in the *Opticks* in fact reflects the position he had already developed in Rule IV of the *Principia*:

> In experimental philosophy, propositions gathered from phenomena by [demonstrative] 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. This rule should be followed so that arguments based on [demonstrative] induction may not be nullified by hypotheses.

In Query 31 of the *Opticks*, Newton described his overall method as consisting of *two* arms: analysis and synthesis. Analysis is another name for the method of inference from the phenomena and consists of deriving causes (i.e. general theories) from effects (i.e. phenomena). But then 'the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phenomena proceeding from them' (p. 404).

Yet clearly it is not impossible that some of the 'Phenomena' that proceed from the 'Causes' may be new—not hitherto investigated. And hence it is clearly not impossible that some such alleged 'Phenomena' be *merely* alleged and so require some revision of the supposed 'Causes'. Newton seems to be acknowledging that it is indeed at least *possible* that the synthesis part of the method might produce counterexamples to the theories 'established' by analysis and hence require their modification.

Current defenders of the method of 'demonstrative induction' such as John Norton, who, after all have the advantage over Newton of the guidance provided by several centuries worth of scientific change, clearly ought then to reject any idea that the method produces—even 'effective'—certainty. One possible conclusion would be that, if so, then the whole distinction between hypotheses and 'experimental philosophy', and with it the whole Newtonian method collapses: that there is, in the end, no sustainable difference between

Newton's method and hypothetico-deductivism; that 'background knowledge' too really consists of hypotheses. And certainly, as we have seen, the extent of the difference between the two methods so far as the inductive steps in Newtonian proofs (especially those sanctioned by the *vera causa* principle) are concerned has been exaggerated by the recent defenders of the Newtonian method. The defensible position may turn out to be that the ideas behind the method provide an adjunct to H-D methods rather than an outright replacement for them.

Nonetheless, there are features of Newtonian 'demonstrative induction' that surely should not be rejected lightly—notably the greater plausibility of the account of *discovery* it supplies and the explanation it provides of the—surely incontrovertible—fact that the massive underdetermination of theory by data seemingly inevitably part of H-D accounts is, in practical terms, totally unrealistic. The defenders of the position perhaps need to make good on the following suggestion that seems to be implicit in Newton's own view: that even though the 'synthesis' (that is, further testing) of a theory arrived at by 'analysis' (that is, deduction from the phenomena) may of course lead to the modification of that theory, nonetheless a properly performed analysis will guarantee that any changes necessitated to the theory will be of the nature of 'modifications' rather than outright rejection. The 'yet other phenomena' that Newton allows in Rule IV may turn up as a result of the 'synthesis' will, at worse, make the proposition arrived at by analysis 'either more exact or liable to exceptions' rather than showing it to be completely off-beam. The (very taxing) task for the defenders of 'demonstrative induction' will be to give precise accounts of the many suggestive but vague notions involved in this claim and to reconcile them with the history of so-called 'revolutions' in science.

Having been ignored in philosophy of science for centuries, the undoubted virtues of the method of 'deduction from the phenomena' are currently in danger of being oversold. New specific theories are indeed often (perhaps even usually) arrived at by, in effect, plugging new empirical information into already accepted general schemes. But the *accreditational* impact of this fact is a different, and more difficult matter. Superficially, at least, the fact that specific theory T can be deduced from general theory G together with evidence e just seems to pass the accreditational buck to G. G is supposed to be already accepted—but if this acceptance, when analysed, is based essentially upon its having passed various tests, then deduction from the phenomena would appear to be an adjunct to hypothetico-deduction rather than a replacement for it. The main problem facing the present-day defenders of Newtonian method is to delineate some special status for the necessary principles of 'background knowledge' and to explain how and why particular principles achieve that status.

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