On Metaphysics and Method in Newton

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Let me remark first that my paper may have a somewhat skew relation to the announced subject of this symposium, "the methodology of scientific creativity." Whether there is such a methodology at all is something of which I feel unsure; doubtless the answer depends in part upon just what one means by the word 'methodology'. In any case, I do not have anything systematic to offer bearing directly upon that question. Nevertheless, after consulting with Professor Leplin, I have concluded that what I intend to say about Newton and method does fall within the general scope of the symposium as he conceives it. Perhaps the paper may best be thought of as treating, not the methodology of creativeness, but an example of methodical creativity. (I mean both originality *in* method, and what Newton would have called "invention" *guided and disciplined by* method.)

When I was a student, reigning opinion held that Newton, although unquestionably in the foremost rank of the great among scientists, was a shallow and unoriginal philosopher. In a work whose reputation at that time was high, E. A. Burtt put it thus: "In scientific discovery and formulation Newton was a marvellous genius; as a philosopher he was uncritical, sketchy, inconsistent, even second rate."[1]

Among Burtt's criticisms of Newton are the following two--one in the topic of method, the other in that of metaphysics. On the former head, Burtt says: "Would that in the pages of such a man we might find a clear statement of the method used by his powerful mind in the accomplishment of his dazzling performances, with perhaps specific and illuminating directions for those less gifted . . . !" (Note that he is asking for something like what Descartes proposed to develop in the *Regulæ:* as it were, an *algorithm* for scientific discovery--a *prescription* for "the accomplishment of dazzling performances.") "But," Burtt continues, "what a disappointment as we turn the leaves of his works! Only a handful of general and often vague statements about his method, which have to be laboriously interpreted and supplemented by a painstaking study of his scientific biography . . . ."

Yet this, Burtt adds, is better than what Newton gives us on the second head, that of "an exact and consistent logical analysis of the ultimate bearings of the unprecedented

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[1]Edwin Arthur Burtt, *The Metaphysical Foundations of Modern Physical Science* (2nd ed., 1932; reprinted Garden City, N. Y.: Doubleday, 1955), p. 208.

intellectual revolution which he carried to such a decisive issue." For Newton, according to Burtt, exhibits, in his metaphysical views, the characteristic vices of "the positivist mind" that "decries metaphysics";[2] in particular, "[h]is general conception of the physical world and of man's relation to it . . . was taken over without examination as an assured result of the victorious movement whose greatest champion he was destined to become."[3]

You will of course not expect here a defence of Burtt's opinions. On one point, however, he is right--or almost right: it is certainly true that for a proper understanding of Newton's general remarks about method, it is necessary to study their exemplification in his work--not, however (I think), as Burtt puts it, to study "his scientific biography," but his scientific *writings*.

The depreciation or patronizing of Newton by philosophical commentators is pretty limp in comparison to William Blake's blazing attack:

> The Atoms of Democritus
> And Newton's Particles of light
> Are sands upon the Red sea shore

Newton becomes a demon figure in Blake's poetic mythology, emblematic of that state of nightmare life-in-death-like sleep of the soul, of rigidly limited vision, that Blake called "Ulro." He writes:

> Now I a fourfold vision see
> And a fourfold vision is given to me
> Tis fourfold in my supreme delight
> And threefold in soft Beulah's night
> And twofold Always. May God us keep
> From Single vision and Newton's sleep.

I don't pretend to given evidence on Newton's behalf that might change Blake's mind; to *those* realms of the imagination that Blake was concerned with, Newton does indeed seem to have been quite blind (if one may judge from the few remarks

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[2]Ibid., p. 229.

[3]Ibid., p. 230.

attributed to him about poetry and music). But in another (and important) sense, Blake's characterization is very mistaken: "single vision" is just what does *not* characterize Newton. I shall argue that a key element of his scientific/philosophical mentality--contributing crucially, on the one hand, to "the accomplishment of his dazzling performances," and on the other hand making some of his accomplishments very hard for his contemporaries to accept or to grasp, and his philosophical position hard to appreciate at (what I believe to be) its true value--was what can be described as a remarkable capacity for multiple vision.

The conception of scientific inquiry held by the main community of natural philosophers in the period of Newton's first investigations--by those of the Royal Society of London, founded in 1660, where Hooke was Curator of Experiments; and of the Académie Royale des Sciences in Paris, founded in 1666, with Huygens the dominant figure--embraced three principal modes of inquiry: experiment, to discover phenomena; the search for (to use a phrase of Huygens's) "hypotheses by motion" to explain them; and the deduction, or mathematical derivation, of the consequences of established principles. The first two modes, pursued together, are abundantly illustrated in Hooke's *Micrographia,* published in 1665; the third, in its purest form, in the very beautiful work of Huygens on the problems of impact and of centrifugal force (both achieved in the 1650's, although for long unpublished) and on the compound pendulum (a subject that occupied him from the late '50's through the '60's; the results of these studies form one of the principal parts of Huygens's great treatise of 1672 on the pendulum clock).[4] And all three modes are exemplified, in mutual interaction, in Huygens's *Treatise on Light,* written (as he tells us) in 1677 (although not published until 1690).

That Newton, in those first investigations of the mid-to-late 1660's--in particular, his investigations of light--had discovered a new way of inquiry, is (I think) quite dramatically shown by the controversies they gave rise to. The most distinguished of Newton's critics were the two men I have mentioned: Hooke and Huygens. Hooke, as curator of experiments, had the duty of repeating experiments reported to the Royal Society, with a view to checking the accuracy of the reports. Newton's letter of February 6, 1672, to Oldenburg (Secretary of the Society and publisher of its

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[4]An equally brilliant accomplishment in the same mode, also embodies in that work, is the discovery of the curve of isochronous vibration for constrained gravitational motion (the so-called "tautochrone" curve).

Philosophical Transactions), containing his "New Theory about Light and Colors,"[5] was read to the assembled membership on February 8.[6] One week later, Hooke sent his critical discussion to Oldenburg.[7] The keynote is struck in its opening sentences:

> I have perused the Excellent Discourse of Mr. Newton about colours and Refractions, and I was not a little pleased with the niceness and curiosity of his observations. But though I wholy agree with him as to the truth of those he hath alledged, as having by many hundreds of tryalls found them soe, yet as to his Hypothesis of salving the phænomena of Colours thereby I confesse I cannot yet see any undeniable argument to convince me of the certainty thereof.

So the experiments are applauded, the explanatory hypothesis not. Hooke is at pains to make clear, however, that his objection is not to Newton's hypothesis as such, but to his claim to have *established* that hypothesis beyond reasonable doubt:

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[5]See *The Correspondence of Isaac Newton,* vol. I, ed. H. W. Turnbull (Cambridge University Press, 1959), pp. 92-102. A facsimile reproduction of the letter as it was published in the Royal Society's *Philosophical Transactions* (No. 80, February 19 1671/2, pp. 3075--87), is given in I. Bernard Cohen, ed., *Isaac Newton's Papers and Letters on Natural Philosophy* (Cambridge, Mass.: Harvard University Press, 1958) (cited hereafter as Newton, *Papers and Letters*), pp. 47-59. (It should be noted that Oldenburg, in publishing the letter, omitted one brief but rather interesting passage that will be of some concern to us below.)

[6]Newton, *Correspondence,* I, p. 103, n. 1 (continued from p. 102).

[7]Ibid., pp. 110-114; *Papers and Letters,* pp. 110-115. (One minute point: The text in *Papers and Let-ters* is a facsimile of the transcription given in volume III of Thomas Birch's *History of the Royal Soci-ety* [Oldenburg did not publish Hooke's critique in the *Philosophical Transactions*--although he did publish Newton's reply to Hooke]. In that version, some rectifications of Hooke's spelling are made [e.g., "wholly" for Hooke's "wholy" in the first passage here quoted]; but one alteration is curiously for the worse: in that same passage, "phenomæna" where Hooke had "phænomena.")

> For all the expts & obss: I have hitherto made, nay and even those very expts which he alledged, doe seem to me to prove that light is nothing but a pulse or motion propagated through an homogeneous, uniform and transparent medium: And that Colour is nothing but the Disturbance of yt light by the communication of that pulse to other transparent mediums, that is, by the refraction thereof: that whiteness and blackness are nothing but the plenty or scarcity of the undisturbed Rayes of light; and that the two colours (then which there are noe more uncompounded in Nature) are nothing but the effects of a compounded pulse or disturbed propagation of motion caused by Refraction. But how certaine soever I think myself of my hypothesis, wch I did not take up without first trying some hundreds of expts; yet I should be very glad to meet wth one Experimentum crucis from Mr. Newton, that should Divorce me from it. But it is not that, which he soe calls, will doe the turne; for the same phænomenon will be salved by my hypothesis as well as by his without any manner of difficulty or straining; nay I will undertake to shew an other hypothesis differing from both his & mine, yt shall do the same thing.[8]

And toward the end of his discussion, Hooke remarks:

> Nor would I be understood to have said all this against his theory as it is an hypothesis, for I doe most Readily agree with him in every part thereof, and esteem it very subtill and ingenious, and capable of salving all the phænomena of coulours; but I cannot think it to be the only hypothesis; not soe certain as mathematicall Demonstrations.[9]

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[8]Newton, *Correspondence,* I, pp. 110-111; *Papers and Letters,* p. 111.--There is in this passage a more substantial editorial alteration in the text given by Birch (from the Register Book of the Royal Society): where, in the first sentence quoted, Hooke writes that the experiments and observations seem to him to prove that "light is . . . a pulse or motion [etc.]," the text in Birch--and therefore that in *Papers and Letters* --reads, "white is . . . a pulse or motion [etc.]" It appears that the text in Birch corre-sponds to the copy Newton saw (cf. *Correspondence,* I, p. 115, n. 3).

[9]*Correspondence,* I, p. 113; *Papers and Letters,* p. 8.

Newton's reply to "ye Theoretique part" of Hooke's critique begins with the issue of "hypotheses": "I shall now take a view of Mr Hooks Considerations on my Theories. And those consist in ascribing an Hypothesis to me wch is not mine; in asserting an Hypothesis wch as to the principall parts of it is not against me; in granting the greatest part of my discourse if explicated by that Hypothesis; & in denying some things the truth of wch would have appeared by an experimentall examination."[10] After taking up in turn the topics of the first three of those clauses, Newton comments on the third of them--"granting the greatest part of [his] discourse if explicated by [Hooke's] Hypothesis"--that "I do not think it needful to explicate my Doctrine by any Hypothesis at all."[11] And proceeding to the fourth clause--"things the truth of wch would have appeared by an experimentall examination"--he repeats: "You see therefore how much it is besides the businesse in hand to dispute about *Hypotheses.* For wch reason I shall now in the last place proceed to abstract the difficulties involved in Mr Hooks discourse, & without having regard to any Hypothesis consider them in generall termes."

In a draft rebuttal, Hooke says of this, a little petulantly: "I see noe reason why Mr. N. should make soe confident a conclusion that he to whome he writ did see how much it was besides the busness in hand to Dispute about hypotheses. for I judge there is noething conduces soe much to the advancement of Philosophy as the examining of hypotheses by experiments & the inquiry into Experiments by hypotheses. and I have tha Authority of the Incomparable Verulam to warrant me."[12] Here we see an assertion ot the two modes of empirical inquiry I have mentioned, with an appeal to the authority of Bacon.

Huygens's point of view I should describe as significantly different from Hooke's, although the two agree both in general that hypotheses are not beside the point, and in special that Newton has not established that there are more than two fundamental colors. Thus Huygens writes:

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[10]*Correspondence,* I, p. 173; *Papers and Letters,* p. 118.

[11]*Correspondence,* I, p. 177; *Papers and Letters,* p123.

[12]*Correspondence,* I, p. 202; cf. Bacon, *Novum Organum* (trans. Spedding, Ellis, and Heath; reprinted in Francis Bacon, *The New Organon and Related Writings,* ed. Fulton H. Anderson [New York: Bobbs Merrill, 1960]), II, x--p. 130 of the cited edition: "Now my directions for the interpretation of nature embrace two generic divisions: the one how to educe and form axioms from experience; the other how to deduce and derive new experiments from axioms."

> It seems to me that the most important objection, put to him in the form of a Quære, is this: Whether there are more than two sorts of colors? For I, for my own part, believe that a Hypothesis that should explain mechanically and by the nature of motion the colors *Yellow* and *Blue* would suffice for all the others . . . . Moreover I do not see why Mr. Newton does not content himself with the two colors, Yellow and Blue. For it will be far easier to find a hypothesis by motion that should explain these two differences, than for so many diversities as there are of other colors. And until he has found that hypothesis, he will not have taught us in what the nature and difference of colors consists, but only this accident (which certainly is very considerable) of their different refrangibility.[13]

Whereas Hooke's grounds of contention with Newton can be characterized as essentially methodological (he rejects Newton's claim to have avoided hypotheses and to have established his results securely), those of Huygens can be characterized--in the sense in which I am using the word--as essentially metaphysical: he denies that Newton has instructed us as to the *nature* of light, and he objects to Newton's conclusion as hard to reconcile with any acceptable account of that nature.

The conception of "being as such," or of the *rerum natura,* that Huygens appeals to is that of the "mechanical philosophy," which in Huygens (who early threw off the shackles of Cartesianism) took the special form of the *atomistic* mechanical philosophy. Matter and space are distinct; there are void spaces, and matter itself consists of ultimate indivisible, impenetrable, rigid--or, in the terminology of Huygens and Newton,

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[13]Newton, *Correspondence,* I, pp. 255-256. The translation from Huygens's French is mine; Oldenburg published Huygens's remarks (without identifying him by name), in his own translation, in the *Philosophical Transactions,* No. 96 (July 21 1673), pp. 6086-7--see Newton, *Papers and Letters,* pp. 136-137.--In consulting the latter source, care should be taken to note that what Oldenburg prints immediately following (pp. 6087-92; *Papers and Letters,* pp. 137-142) as Newton's reply is in fact his reply to Huygens's *second* letter (printed by Oldenburg in the *next* number of the *Transactions* [p. 6112; *Papers and Letters,* p. 147] after Newton's actual reply to the first letter [pp. 6108-11; *Papers and Letters,* pp. 143-146]). (There is a half-apology for this mix-up in Oldenburg's heading to Newton's reply, p. 6108.)

"hard"--particles. All natural process consists in the motions of these particles. The motion of any one particle is uniform and rectilinear unless and until it is constrained to change by its impingement upon another particle (or particles). Therefore, the fundamental laws of nature, namely the laws of the motion of the fundamental particles, are *the laws of impact of hard bodies.* These laws of impact Huygens himself had discovered (his recognition of the absurdity of the rules of impact proposed by Descartes was the crucial moment in his break with the Cartesian philosophy and the beginning of his own great independent work in physics). The task that remained, then, for fundamental physics was the discovery of suitable "mechanical models," as we should now say, for the representation of natural phenomena.--But, it should be emphasized, mechanical models of a quite special kind. Comparison with a much later view may be instructive: Toward the end of the last century, a number of physicists were dissatisfied with the received view of the structure of mechanics, and one kind of improvement that was sought was the elimination of the concept of *force.* Independently, J. J. Thomson[14] and (later) Heinrich Hertz[15] proposed to eliminate "forces" in favor of what, in analytical mechanics, are called "constraints." A way of putting this is to say that *there is no potential energy:* all energy is kinetic, what appears macroscopically as potential energy is really kinetic energy of "hidden" motions. Now, a Huygensian mechanical model, or "hypothesis by motion," is a system of this type--with two qualifications: First, Hertz requires his "constraints" to satisfy a rather strong condition of *continuity,* and (as we shall see in a moment) this condition can *never* be satisfied in a Huygensian system. Second, and on the other hand, Hertzian constraints are of a very general kind, whereas for Huygens the only constraints are those implied by the strict impenetrability and unalterable shape of the fundamental particles. An immediate consequence is that the changes of motion of the fundamental particles are

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[14]J. J. Thomson, "On some Applications of Dynamical Principles to Physical Phenomena," *Philosophical Transactions of the Royal Society of London* **176** (1885), pp. 307-342.

[15]Heinrich Hertz, *The Principles of Mechanics presented in a New Form,* trans. D. E. Jones and J. T. Waley (reprinted New York: Dover Publications, 1956).

not smooth--not continuous--but instantaneous. (This is a point raised by Leibniz as one of the fatal objections against atomism.)[16]

There is one point in which the sketch I have given does not quite do justice to Huygens's basic position: I have ignored here his very interesting reservations about the "absolute" conception of motion. But waiving this matter, what I have called the Huygensian metaphysical principles are also, by all the evidence known to me, the principles accepted by Newton in the period we are now considering. In particular, when he writes of "the lawes of motion" he means the laws of impact,[17] and what *we* call the laws of "perfectly elastic" impact *he* ascribes to "bodyes which are absolutely hard."[18]

How does all this bear on Newton's optical investigations and the controversy over them? So far as the metaphysical issue--the "nature" of light and colors--is concerned, Newton has in fact set it almost entirely aside. This is Huygens's complaint; and just as in his answer to Hooke Newton had written, "I do not think it needful to explicate my Doctrine by any Hypothesis at all," so to Huygens's remark that until he has found a mechanical explanation "he will not have taught us in what the nature and difference of colors consists," Newton replies: "[T]o examin how colours may be thus explained Hypothetically is besides my purpose. I never intended to show wherein consists the nature and difference of colours, but onely to show that *de facto* they are originall & immutable qualities of the rays which exhibit them, & to leave it to others to explicate by Mechanicall Hypotheses the nature & difference of those qualities."[19]

Three questions arise: What is the actual content of Newton's allegedly non-hypothetical "Doctrine"? How in fact does he claim to have established this doctrine?

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[16]See, e.g., Part II of *Specimen Dynamicum;* in Gottfried Wilhelm Leibniz, *Philosophical Papers and Letters,* ed. and trans. Leroy E. Loemker (2nd ed.; Dordrecht-Holland: D. Reidel Publishing Co., 1970), p. 447.

[17]See the early manuscript "The Lawes of Motion," now published in A. Rupert Hall and Marie Boas Hall, eds., *Unpublished papers of Isaac Newton* (Cambridge University Press, 1962), pp. 157-164.

[18]Ibid., p. 162.

[19]Newton, *Correspondence,* I, p. 264; *Papers and Letters,* p. 144.

And is his claim to have established it with "certainty," with no hypothetical or "conjectural" element,[20] a defensible one?

Let us begin from Hooke's challenge to what Newton called the *Experimentum Crucis.* That experiment, Hooke says, cannot "divorce him" from his own hypothesis. What was the experiment, and what did Newton claim to have established by it?

Having observed--as no one had before him[21]--that the shape of the colored image produced by a prism was incompatible with the presumed constancy of the ratio of the

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[20]*Correspondence,* I, p. 100; *Papers and Letters,* p. 57 (and cf. infra).

[21]An exception should perhaps be made of Thomas Harriot (the name is spelled variously, but this is now the most usual spelling), whose remarkable work included, about the year 1601 or 1602 (thus some twenty years earlier than Snell, and perhaps twenty-five years earlier than Mydorge and Descartes) the discovery of the law of refraction; and who also discovered dispersion--and measured the indices of refraction of (some type of) glass, and several liquid media, for rays of different sorts--about sixty years before Newton. It appears, however, that Harriot's procedure was to view objects through a prism; and in his measurements of dispersion, to view the colors at the edge of the image of a white sheet. Therefore, although his results would indeed *predict* an oblong shape for the dispersed solar image, the evidence does not suggest that he made observations of that sort at all.

Published information about Harriot's optical work is rather meager. He himself published none of it; and when Kepler, having received a report that an Englishman named "Herriot" possessed "new and unknown principles," wrote eagerly to ask for instruction about colors and refractions, Harriot confirmed that he had mastered "great mysteries," but declined to reveal them yet, and counseled Kepler to "wait patiently." Harriot's manuscripts are, by all accounts, almost Babylonian in character, consisting of diagrams, computations, and tables merely. It is therefore far from clear what interpretation he put upon his numerical results--whether, for example, he knew that light of a given spectral color does not suffer dispersion upon further refraction.

On Harriot's discovery of the law of refraction, see John W. Shirley, "An Early Determination of Snell's Law," *American Journal of Physics* **9** (1951), pp. 507-508. For further information on this subject, and on Harriot's relations with Kepler, See Johannes Lohne, "Thomas Harriott (1560-1621): The Tycho Brahe of Physics," *Centaurus*

6 (1959), pp. 113-121, and (by the same author) "Zur Geschichte des Brechungsgesetzes," *Sudhoffs Archiv für Geschichte der Medizin und der Naturwissenschaften* **47** (1963), pp. 152-172 and "Kepler und Harriot: ihre Wege zum Brechungsgesetz," *Internationales Kepler-Symposium Weil du Stadt, 1971* (Hildesheim, 1973), pp. 187-214. Of these papers, all except that of Lohne in *Sudhoffs Archiv* are reprinted in John W. Shirley, ed., *A Source Book for the Study of Thomas Harriot* (New York: Arno Press, 1981).

On dispersion, Lohne first tells us (*Centaurus* **6**, pp. 120-121): "Nowhere we miss Harriott's own explanations as much as here. We find his diagrams and his computations, but not his views on dispersion and no description of his experimental method. However, his tables and his diagrams . . . tell us that he looked at a piece of white paper through a hollow glass prism filled with a transparent fluid. He also used a solid glass prism. Somehow he measured the coloured margins that the paper seemed to have when looked at through the prism. He calls the rays of different refrangibility the 'Cardinales primarij' and 'Cardinales secondarij'. . . . If we have interpreted the scanty information rightly (we are by no means sure of this), Harriott misunderstood the spectrum he saw, and though he got a fairly good value for the dispersion, he erroneously believed that red rays were refracted more than middle rays."--This is rather puzzling. The table Lohne publishes (ibid., p. 120 and Plate 5) shows higher refractive indices for the "secondary" than for the "primary cardinals." Lohne does not tell us why he takes the former to be the red; and it is hard (I should think impossible) to see how Harriot could by a "misunderstanding" have measured indices of refraction systematically (for various media) and found them larger for red than for some intermediate spectral color. In his 1971 symposium paper, Lohne says (p. 205): "Er [*sc.*, Harriot] setzte 1604 und 1605 seine Brechungsmessungen fort, indem er mit rotem, gelbem und grünem Licht experimentierte und für jede Farbe eigene Brechungsverhältnisse bzw. maximale Brechungswinkel ermittelte. Seine Messungen der Farbendispersion in Prismen sind jedoch nicht so genau wie die Bestimmung der mittleren Brechungszahlen. Blaue und violette Prismenfarben werden in seinen Manuskripten nicht berücksichtigt."

To the best of my knowledge, it was in Lohne's paper of 1959 that Harriot's discovery of dispersion was first made known; and the passages I have quoted contain

sines of the angles of incidence and refraction, Newton isolated, successively, different portions of the dispersed spectral light, and found that each portion by itself, when refracted through a second prism, yielded an image compatible with that law; but that these several portions were refracted differently from one another: the light at the end of the spectrum that was refracted least in the production of that spectrum continued to be refracted less, when segregated, than that at the other end; and correspondingly for the intermediate places in the spectrum. You will notice that I have--perhaps rather clumsily--avoided in this statement any mention of colors. So does Newton: his description of the *Experimentum Crucis* itself contains not a word about color. And the conclusion he draws is very precisely stated: "And so the true cause of the length of that Image was detected to be no other, then that *Light* consists of *Rays differently refrangible,* which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards divers parts of the wall."[22] In a terminology that Newton proceeds immediately to introduce, he claims to have established a distinction--that is, an actually existing one: a distinction *in rerum natura*--between "uniform" or "homogeneous" light, and "difform" or "heterogeneous" light; to have established, moreover, that there is an indefinite variety of kinds of the former; and that ordinary sunlight is a "heterogeneous mixture" of rays of a continuous range of refrangibilities.[23] Since the *Experimentum Crucis* actually *exhibits* this distinction--that is, *produces* (approximately) homogeneous lights whose different refrangibilities it shows, and shows moreover that the diverse refrangibilities of the several homogeneous lights correspond exactly with the different degrees of refraction of the refracted parts of sunlight from which they were obtained, it is very hard to fault Newton's assertion that his experiment had "detected" these facts.

It is in the second part of his paper that Newton turns to the theory of colors. Here he contents himself with a statement of the "doctrine" itself, with some brief indications

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the fullest information on the subject that I have been able to find in the literature. Surely, after thirty years, it is much to be desired that a fuller and more systematic account should appear. Of course this work of Harriot's--because it appears to have remained completely unknown (except to a few of his immediate disciples)-- is of no real relevance to the subject of the present paper.

[22]Newton, *Correspondence,* I, p. 95; *Papers and Letters,* p. 51.

[23]*Correspondence,* I, pp. 95, 96; *Papers and Letters,* pp. 51, 53; cf. also below, "Further Considerations on Newton's Methods."

of a few of the many experiments on which it rests.[24] The doctrine of colors can be summed up more briefly than in Newton's original account. It consists essentially in two points: (1) The visible appearance, the *"Species,"* of any light--that is, the appearance it gives rise to when it falls on the retina of a seeing eye--is entirely determined by its physical constitution out of the rays of the various homogeneous sorts in definite quantities. (2) On the other hand, lights that differ in their physical constitution can produce the same visual appearance of color; thus, Newton remarks, "The same colours *in Specie* [that is, *in visual appearance*] with [those of homogeneous light] may also be produced by composition."[25] Or in other words, combining the two points: there is a *many-to-one mapping* from the physical constitutions of lights, in the sense of "physical constitution" established by the *Experimentum Crucis,* to their perceptual effects or "Species."

There are many points of detail worth considering, with respect to Newton's investigation itself, to his account of it, and to the controversy;[26] but like Newton in his letter to Oldenburg, I have to limit myself here to a few instances. I have said earlier (and not yet explained the point) that Newton's capacity for multiple vision has been a source of confusion--or misconception--to his readers from his time to our own. I have now to add that another such source lies in the fact that Newton is a quite exceptionally precise writer; making allowances for human fallibility, a remarkable proportion of his statements say exactly what he means to say. Perhaps it seems paradoxical that precision of statement should be a cause of misapprehension; but in fact most readers do not *read* with the precision Newton expects of them. To take a rather small point: it has been instanced, in favor of the view that "there is not . . . a single term of which it is true to say that it *could not* (without changing or extending its meaning) be used to refer to unobservables," that "'Red,' for example, was so used by Newton when he

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[24] *Correspondence,* I, p. 97; *Papers and Letters,* p. 53.

[25] *Correspondence,* I, p. 98; *Papers and Letters,* p. 54.

[26] Cf. my paper, "Further Considerations on Newton's Methods," below in this volume.

posited that red light consists of *red corpuscles*."[27] But what Newton actually says, in his letter to Oldenburg, is that some rays--the least refrangible ones--are "disposed to exhibit a Red colour";[28] not that they "are red." If, therefore, he does occasionally apply the adjective directly to the word "ray," that is patently a derivative use--an "extension of meaning." Indeed, so sensitive is Newton to this that later, in the *Opticks*, he gives us the following as a formal "Definition":

> The homogeneal Light and Rays which appear red, or rather make Objects appear so, I call Rubrifick or Red-making; those which make Objects appear yellow, green, blue, and violet, I call Yellow-making, Green-making, Blue-making, Violet-making, and so of the rest. And if at any time I speak of Light and Rays as coloured or endued with Colours, I would be understood to speak not philosophically and properly, but grossly, and accordingly to such Conceptions as vulgar People in seeing all these Experiments would be apt to frame. For the Rays to speak properly are not coloured. In them there is nothing else than a certain Power and Disposition to stir up a sensation of this or that Colour. For as Sound in a Bell or musical String, or other sounding Body, is nothing but a trembling Motion, and in the Air nothing but that Motion propagated from the Object, and in the Sensorium 'tis a sense of that Motion under the Form of Sound; so Colours in the Object are nothing but a Disposition to reflect this or that sort of Rays more copiously than the rest; in the Rays they are nothing but their Dispositions to propagate this or that Motion into the Sensorium, and in the Sensorium they are Sensations of those Motions under the Forms of Colours.[29]

Another recent commentator--our fellow symposiast Professor Shapiro, whose positive service in clarifying Newton's optical work (and in making his Cambridge

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[27]George Pitcher, *Berkeley* (London: Routledge & Kegan Paul, 1977), p. 261, n. 7; with a reference there to Hilary Putnam, "What Theories Are Not," in Ernest Nagel *et al.*, eds., *Logic, Methodology and Philosophy of Science* (Stanford University Press, 1962), p. 243.

[28]Newton, *Correspondence*, I, p. 97; *Papers and Letters*, p. 53.

[29]Sir Isaac Newton, *Opticks* (repreinted from the 4th ed.; New York: Dover Publications, 1952), p. 124.

lectures available in a splendid edition) is surely well known--has claimed that the theory presented in the letter to Oldenburg is flawed, and is so because of a confusion of Newton's: "The source of this flaw is that he did not initially define the entities to which his theory applied. For instance, in the third proposition [of the section on color] he says that 'the species of colour, and degree of Refrangibility proper to *any particular sort of Rays,* is not mutable by Refraction, nor by Reflection . . . .' He does not define 'particular sorts of rays'; thus both greens, for example"--that exhibited by a homogeneous light, and that, "the same *in Specie,*" exhibited by a mixture of yellow and blue--"could be called 'particular sorts of rays,'[30] since, as he has just told us, their color is the same."[31]

But Newton has not told us that "their color is the same"; he has told us that their colors are "the same *in Specie* "--in appearance. And he *has* told us what he means by "a particular sort"; for this is implied both by his use of the words "uniform" or

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[30]Evidently a slip of the pen for "could be called *rays of the same particular sort*." It should perhaps be noted that Newton was mistaken in thinking that a green could be made by composing homogeneous blue and yellow lights. At best, one can obtain in this way a very unsaturated green--a "greenish white." It is possible that Newton eventually recognized this fact, although he never (so far as I know) announced it; for in the *Opticks,* in discussing the composition of colors (Book I, Part II, Proposition IV; pp. 132-133), whereas he tells us that "a Mixture of homogeneal red and yellow compounds an Orange, like in appearance of Colour to that orange which in the series of unmixed prismatick Colours lies between them," he says something rather different about green in relation to yellow and blue: "And after the same manner other neighboring homogeneal Colours may compound new Colours, like the intermediate homogeneal ones, as yellow and green, the Colour between them both, and afterwards, if blue be added, there will be made a green the middle Colour of the three which enter the Composition. For the yellow and blue on either hand, if they are equal in quantity they draw the intermediate green equally towards themselves in Composition, and so keep it as it were in Æquilibrium, that it verge not more to the yellow on the one hand, and to the blue on the other, but by their mix'd Actions remain still a middle Colour."

[31]Alan E. Shapiro, "The Evolving Structure of Newton's Theory of White Light and Color, *Isis* **71** (1980), p. 222.

"homogeneous" *versus* "difform" or "heterogeneous" ("one particular sort" = "one particular *form* or *kind*," therefore a particular sort is uniform or homogeneous), and by the very sentence quoted from Proposition 3, where a "proper degree of refrangibility" is ascribed to a particular sort. But when Newton, in his reply to Hooke, refines his formulation to make the point clearer, Shapiro says that Newton recognized the inadequacy of his original distinction but "in his typically sly manner" wrote Hooke that he was merely restating that distinction. This seems to me an all too typical instance of the strange fashion for Newton-bashing: examples abound of cases in which attempts by Newton to clarify his meaning, even by citing his own words and pointing out their content, have subjected him to charges of evasion, deception, dishonesty, and malice.

Let us consider an example of greater moment. Recall that in the passage I have quoted above on "unobservables," it was said that Newton "postulated that red light consists of *red corpuscles*." I have commented on the adjective; but what of the noun: "corpuscles"?

The "hypothesis" that Hooke (in contrast to Huygens) thinks that Newton's paper was chiefly concerned to defend is described by him in the following passage:

> But grant his first proposition that light is a body, and that as many colours or degrees thereof as there may be, soe many severall sorts of bodys there may be, all wch compounded together would make white, and grant further, that all luminous bodys are compounded of such substances condensd, and that, whilst they shine, they doe continually send out an indefinite quantity thereof every way *in orbem*, which in a moment of time doth disperse itself to the outmost and most indefinite bounds of ye universe; granting these, I say, I doe suppose, there will be noe difficulty to demonstrate all the rest of his curious Theory: Though yet, methinks, all the colourd bodys in the world compounded together should not make a white body; and I should be glad to see an expt of the kind.[32]

Apart from his serious misunderstanding of the relation of light to the composition of bodies and the way in which the various colors together make white, and his intimation of something like instantaneous propagation of light, the view Hooke here

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[32]See Hooke's paper in Newton, *Correspondence*, I, pp. 113-114; *Papers and Letters*, p. 114.

attributes to Newton is indeed one that Newton inclined to--as probable. On the other hand, the only reference to such a view in Newton's paper occurs in a paragraph that follows the presentation of the entire "Doctrine," and suggests *consequences* of that doctrine. What Newton wrote is: "These things being so, it can be no longer disputed, whether there be colours in the dark, nor whether they be the qualities of the objects we see, no nor perhaps, whether Light be a body." Then, after a brief argument (surprisingly scholastic and unconvincing) in favor of this last claim, he continues: "But, to determine more absolutely, what Light is, after what manner refracted, and by what modes or actions it produceth in our minds the Phantasms of Colours, is not so easy. And I shall not mingle conjectures with certainties."[33]

In his reply to Hooke, Newton acknowledges that "from my Theory I argue the corporeity of light," but denies--quite accurately--that this is a "Hypothesis" or "fundamentall supposition" upon which the theory rests, or, indeed, "any part of [the theory itself], wch was wholly comprehended in the precedent Propositions." (For this he is condemned as "dishonest" by another eminent critic.)[34] And he points out further that he has taken great care, in the exposition of his theory proper, to avoid language that in any way implies commitment to any explanatory mechanical hypothesis: "But I knew that the Properties wch I declared of light were in some measure capable of being explicated not onely by that, but by many other Mechanicall Hypotheses. And therefore I chose to decline them all, & speake of light in generall termes, considering it abstractedly as something or other propagated every way in streight lines from luminous bodies, without determining what that thing is . . . ."[35] Newton, in fact, *never* speaks of "red corpuscles"; whether using the "vulgar" or the "philosophic" mode, he always say "red--or rubrific--*rays.*"

What one sees here is characteristic of Newton's public utterances: he has exercised a very remarkable discipline to separate--as he puts it--"conjectures" from "certainties"; that is to say, from results supported sufficiently strongly by experiments to merit confident belief. Three aspects of this performance were quite novel for the time, and--as (I hope) we have in part seen--were, and to some extent still are, widely misunderstood: first, the claim of such confidence for theoretical conclusions about the

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[33]Newton, *Correspondence,* I, p. 100; *Papers and Letters,* p. 57.

[34]Thomas S. Kuhn, "Newton's Optical Papers," in Newton, *Papers and Letters,* p. 40.

[35]Newton, *Correspondence,* I, pp. 173-174; *Papers and Letters,* pp. 118-119.

non-apparent *physical constitution* of something; second, the claim of "theoretical" status--that is, the status of principles that genuinely concern physical constitution--for anything other than "mechanical" explanations; and third, that attitude of mind that can simultaneously incline towards one sort of mechanical explanation, be willing to consider possibilities alternative to that one, and separate out those results of investigation that are secure irrespective of such explanation. Newton's latter-day critics have been too prone to find in this capacity of his for holding both several levels of theory, and several alternative hypothetical explanations, simultaneously in view, evidence of vacillation, inconsis-tency, and even hypocrisy.[36]

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[36]Thus, for instance, Kuhn--"Newton's Optical Papers," pp. 43-44--speaks of Newton's "retreat from the defense of metaphysical hypotheses which [he] believed and employed creatively," as "attested by the inconsistencies in his discussions and use of hypotheses throughout the optical papers printed below [*sc.,* in *Papers and Letters*]"; and details these "inconsistencies" as follows: "In the first paper light was a substance. In the letters to Pardies light was either a substance or a quality, but the definition of light rays in terms of 'indefinitely small . . . independent' parts made light again corpo-real. In the same letter Newton proclaimed that his observations and *theories* could be reconciled with the pressure *hypotheses* of either Hooke or Descartes, but in the letter to Hooke he forcefully demonstrated the inadequacy of all pressure hypotheses to explain the phenomena of light and colors. . . . In 1672 he denied the utility of hypotheses when presenting a theory which he believed could be made independent of them, but in dealing with the colors of thin films in the important letters of 1675/6 he employed explicit hypotheses, presumably because the new subject matter of these letters could not otherwise be elaborated."

Newton's critics, as I have remarked, not infrequently attach moral culpability to what they see as his errors. Kuhn in particular attributes to Newton (ibid., p. 39) a "fear of exposure and the correlated compulsion to be invariably and entirely immune to criticism," accuses him (p. 40) of dishonesty in his response to Hooke (this has already been noted--cf. n. 32 above), and asks whether Newton "is not . . . convicted of an irrationally motivated lie" in his reply to Huygens. These are very strong charges; let us consider to what extent they are supported by the evidence.--In what follows, the correspondence with Pardies is quoted only from *Papers and Letters* (ed. Cohen),

because the *Correspondence* gives, with the Latin originals, only an English paraphrase, not a full translation. In all other cases I cite both sources, but follow the *Correspondence*--and thus the original manuscripts--in orthography and pointing.

(1) The statement that "in the first paper light was a substance" is strictly true--as Newton in fact acknowledged--but (as we have seen) misleading, since the point occurs only as a postscript to the theory presented in that paper.

(2) In the (second) letter to Pardies Newton says: "By light . . . I understand any being or power of a being (whether a substance or any power, action, or quality of it) which proceeding directly from a lucid body, is apt to excite vision. And by the rays of light I understand its least or indefinitely small parts, which are independent of each other; such as are all those rays which lucid bodies emit in right lines, either successively or all together." This does not in fact "make light again corporeal," for all opticians were agreed that light is (in the words quoted above from the reply to Hooke) "something or other propagated in straight lines" from shining or illuminated bodies. That the rays in different lines are different--thus can be termed "different parts"--was not in any way controversial; and Newton points out (what is also hardly controversial) that the light propagated "successively"--that is, *at different times*--must also be regarded as different. (On this point, cf. *Opticks,* Book I, Part I, Definition I, with the discussion following Definition II, where Newton abstains [provisionally] from assuming a finite velocity of propagation of light, and remarks that if propagation is instantaneous, the rays will be entire [instantaneous] *lines* [more accurately, line-segments], whereas if propagation is with finite velocity, the rays will be quasi-punctiform "parts" of light that *travel* along those lines.) So much, then, for the accusation that Newton has made a concession and at once withdrawn it. As for the concession itself, it occurs in the context of Newton's remark that "the doctrine which I explained concerning refraction and colours, consists only in certain properties of light, without regarding any hypotheses, by which those properties might be explained." This is exactly the distinction Newton makes in his reply to Hooke: the corpuscular conception of light, which is indeed alluded to in the first paper, is no part of the positive "doctrine" there presented.

(3) Coming next to the issue of the reconcilability with Newton's results of "the pressure hypotheses of either Hooke or Descartes," some distinctions have to be noticed. First, Descartes did certainly hold that light is a kind of pressure--in an

incompressible medium--propagated instantaneously, without any actual motion. Hooke's hypothesis is significantly different: he says (see Newton, *Correspondence,* I, p. 110; *Papers and Letters,* p. 111) that all his own experiments and observations seem to him to prove that white is nothing but a *pulse or motion,* propagated through a homogeneous uniform transparent medium; and, again (*Correspondence,* I, p. 112; *Papers and Letters,* p. 112), that "the motion of Light in an uniform medium . . . is propagated by simple & uniform pulses or waves, which are at Right angles with the line of Direction; but falling obliquely on the Refracting medium, it Receives an other impression or motion, which disturbs the former motion:--somewt like the vibration of a string." It is true that a little later (*Correspondence,* I, p. 114; *Papers and Letters,* p. 114) Hooke refers to this "motion or puls" as "propagated . . . to all imaginable distances in a moment of time," which is hard to reconcile with the character of a "motion or pulse" at all (as opposed to a state of pressure in an incompressible medium)--unless, that is, one takes "all imaginable distances" to mean "vast distances," and "a moment of time" to mean "a very short time" (readings that are at least reconcilable with contemporary idiom). That the latter--colloquial--interpretation may be what Hooke intended is rendered somewhat more likely by the fact that we have seen him use similar phrases about the *corpuscular* hypothesis he attributes to Newton. In any case, Newton, when he explains, in his reply to Hooke, the sense in which "Mr Hooks *Hypothesis* as to ye fundamentall part of it is not against me," describes that "fundamentall supposition" in these terms (*Correspondence,* I, pp. 174-175; *Papers and Letters,* p. 120): "that the parts of bodies when brisquely agitated, do excite vibrations in the Æther, wch are propagated every way from those bodies in streight lines, & cause a sensation of light by beating & dashing against ye bottom of the eye, something after the manner that vibrations in the Air cause a sensation of Sound by beating against the Organs of hearing." This is a very clear statement of the fundamental conception of the wave theory, and provides the groundwork necessary for Newton's suggestion that the distinction of the kinds of (homogeneous) light is that of waves of various "bignesses" (i.e., wave-lengths). Hooke, who was not shy about complaining, never complained that Newton had here misrepresented his hypothesis; so we may take the characterization to be, at the least, defensible.

Now, Kuhn has remarked that whereas Newton proclaimed to Pardies the reconcilability of his results with the hypothesis of Hooke, in the letter to Hooke "he forcefully demonstrated the inadequacy of all pressure hypotheses to explain the phenomena of light and colors." But this is seriously inaccurate. In the reply to Hooke there is a section *Correspondence,* I, pp. 176-177; *Papers and Letters,* pp. 121-123) in which Newton expresses the view that Hooke's own special theory of colors and of the distortion of the spectral image is "not onely *insufficient,* but in some respects *unintelligible*"; but this is beside the point, since it concerns only a subordinate part of Hooke's conception, not what Newton describes as "ye fundamentall part of it"--and certainly not "all pressure hypotheses." As to the latter, Newton does indeed raise an objection *Correspondence,* I, pp. 175-176; *Papers and Letters,* p. 121) against "all . . . *Mechanicall Hypotheses* in wch light is supposed to be caused by any pression or motion . . . excited in the Æther by the agitated parts of luminous Bodies." He puts this as follows: "[T]o me the fundamentall supposition it selfe seems impossible; namely that the waves or vibrations of any fluid can like the rays of Light be propagated in streight lines, without a continuall & very extravagant spreading & bending every way into ye quiescent Medium where they are terminated by it. I am mistaken if there be not both *experiment & Demonstration* to the contrary." This is certainly a strong objection. But--another distinction that it seems to me most important to notice--it is *not* an objection based upon Newton's own results, which were under attack; it has nothing at all to do with the theory of homogeneous lights with immutable properties, or the compositeness of white light, but rests on the contrary upon that basic property of light on which all theorists were agreed: that it is propagated in definite lines, and does not spread away from them into regions of shadow. The straightforward reading of Newton's concessions and objections is simply this: that there may be many hypotheses that can account for optical phenomena; that *already established* phenomena lead him to have very strong doubts about those hypotheses that regard light as a pressure or undulation in a medium; but that his new theory of light and colors does not of itself stand against the latter--if a wave theory can account for the previously established properties of light, there is a promising way to incorporate his new results into such a theory. (To be sure, Newton has not really shown how the like could be done for the hypothesis of Descartes--although in the letter to Pardies he did offer a feeble

suggestion on this point. Whether that leaves Kuhn a strong case for the prosecution is a question I am content to submit to the jury.)

(4) Finally, we have Kuhn's assertion that Newton denied the utility of hypotheses in 1672, but used them in his communications to the Royal Society of 1675/6, "presumably" because he could not then do without them in dealing with "the new subject matter of these letters," namely, the colors of thin films. But Newton had in fact no difficulty whatever in describing his results concerning the colors of thin films without recourse to the hypotheses about light presented in these letters. As to his motive in presenting those hypotheses, here is what he says in his covering letter to Oldenburg (*Correspondence,* I, p. 361; *Papers and Letters,* p. 178--emphasis added): "Sr. I had formerly purposed never to write any Hypothesis of light and colours, fearing it might be a means to ingage me in vain disputes: but I hope a declar'd resolution to answer nothing that looks like a controversy . . . may defend me from yt fear. And therefore *considering that such an Hypothesis would much illustrate ye papers I promis'd to send you* . . . : I have not scrupled to describe one . . . , *not concerning myself whether it shall be thought probable or improbable so it do but render ye papers I send you, and others sent formerly, more intelligible.*" In view of the fact that he had been previously tasked for abstaining from explanatory hypotheses, and had encountered serious misunderstanding, the reason he gives surely has *prima facie* plausibility; and the fact that the new communications about light, unlike his first paper, did not in fact arouse serious contention would seem to confirm the soundness of the strategy he professes. His statement is in no way inconsistent with what he has said earlier (or with what he will say later) about hypotheses; Kuhn's statement about the "presumable" motive stands (in my own opinion) as a quite *groundless* presumption.

One more example of the sort of accusation I have been speaking of may be given here. In his preface to the Dover reprint of Newton's *Opticks,* I. B. Cohen says (p. xxxv): "To be sure, the speculations of the *Opticks* were not hypotheses, at least to the extent that they were framed in questions. Yet if we use Newton's own definition, that 'whatever is not deduced from the phenomena is to be called an hypothesis,' they are hypotheses indeed. The question form may have been adopted in order to allay criticism, but it does not hide the extent of Newton's belief. For every one of the Queries is phrased in the negative!"--As if Newton did not know this! In fact, Newton knew

When Newton says that hypotheses "are not to be regarded in,"[37] or "have no place in,"[38] experimental philosophy, he in no way intends to imply that they have no *use* in such philosophy. In 1672, in his reply to the second letter on his theory of light by I. G. Pardies, Newton wrote that "hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; *unless so far as they may furnish experiments.* For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties."[39] And in Book III of the *Opticks,* whose first edition was published in 1704, he introduces the famous series of conjectures that constitutes the major part of that Book as follows:

> When I made the foregoing Observations, I design'd to repeat them with more care and exactness, and to make some new ones . . . . But I was then interrupted, and cannot now think of taking these things into farther Consideration. And since I have not finish'd this part of my Design, I shall conclude with proposing only some Queries, *in order to a farther search to be made by others.*[40]

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exactly what he was doing--and made not the slightest attempt to conceal it. For instance, in a letter he sent to Fontenelle with the gift of a copy of the *Opticks* he wrote (see *Correspondence,* vol. VII, ed. A. Rupert Hall and Laura Tilling [Cambridge University Press, 1977], p. 72): "Here I cultivate the experimental philosophy, as that which is worthy to be called philosophy, and I treat hypothetical philosophy, not as philosophy, but by means of queries." (On the perfect consistency of Newton's practice here with his preachment elsewhere--notably in his letter to Pardies--see the main text just below.)

[37]Newton, *Opticks,* p. 404.

[38]Newton, *Philosophiæ Naturalis Principia Mathematica,* Scholium Generalis; in the edition of Florian Cajori--*Sir Isaac Newton's Mathematical Principles of Natural Philosophy and his System of the World* (Berkeley, California: University of California Press, 1934)--p. 547.

[39]Newton, *Papers and Letters,* p. 106 (emphasis added).

[40]*Opticks,* pp. 338-339 (emphasis added).

--It is precisely the use he had assigned as proper to hypotheses some thirty-two years earlier.

It is worth remarking that one chief source of the view of hypotheses prevalent in the period we are considering was the strange history of Descartes's program. For although Descartes, in the *Regulæ,* deprecated "conjecture" in stronger terms than Newton ever did, going so far as to say that what we "conjecture" is not even worth *investigating,*[41] yet in his work in natural philosophy he did find it either necessary or useful to introduce hypotheses; and, although I myself think (in opposition to some recent opinion) that Descartes never abandoned his belief both that he had derived his own most basic physical views--e.g., his theory of the nature of light--from stringently established principles, and that eventually such a derivation of *all* natural knowledge was to be attained, it is clear that the hypothetical mode of constructing mechanical explanation was what continued to influence such natural philosophers as Huygens, who had lost all confidence in Descartes's program of *demonstration* while continuing to embrace the program of *mechanical explanation.*

In *Le Monde,* Descartes introduces what he characterizes as a "fable" of the creation by God of a "new world," governed by principles which (in the sequel) we are intended to conclude are those that in fact govern our own world. In developing the structure of this world, he remarks, "nous prenons la liberté de *feindre* cette matière"--the "new matter" created for the new world--"à nostre fantaisie": "we are taking the liberty to *feign* this matter to our fancy."[42] In his *Principia,* the place where the method of hypothesis is set out, and which corresponds in content to the creation fable of *Le Monde,* is Part III, §§42-47. The headings of §§43, 44, 45, and 47 make a rather extraordinary sequence: "§43. That it is hardly possible but that causes from which all the phenomena are clearly deduced are true. . . . §44. That I nevertheless wish those I set forth here to be considered only as hypotheses. . . . §45. That I shall even assume

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[41]See Rule Three--in *The Philosophical Writings of Descartes,* trans. John Cottingham, Robert Stoothoff, and Dugald Murdoch (Cambridge University Press, 1985), vol. I, p. 13; and cf. also the optical example in Rule Eight--ibid., pp. 28-29.

[42]René Descartes, *Le Monde,* trans. Michael Sean Mahoney (French and English on facing pages; New York: Abaris Books, 1979), pp. 50 (French)/51 (English)--emphasis added; my own translation of *feindre* (Mahoney has "the liberty of *imagining* this matter").

here some which are indisputably false. . . . §47. That the falsity of these suppositions does not prevent what will be deduced from them from being true and certain." Part IV opens with a section headed: "That the false hypothesis we have already used is to be retained here, in order to explain the true nature of things." And the next section begins: "And so let us feign that this Earth we inhabit was formerly composed [like the Sun], . . . and that it was situated in the center of a vast vortex." The phrase "and so let us feign" is, in Latin, "fingamus itaque": the verb, *fingere,* to which the French *feindre* is cognate, is the same as used by Newton in his celebrated declaration, *"Hypotheses non fingo"*--"I do not feign hypotheses." To "feign" hypotheses is, for Newton, to put forth conjectures--indeed, even confessed falsehoods--as a basis on which to establish truths. Newton knew his Descartes very well, and there seems to me little doubt that his choice of words made a deliberate reference to these passages.[43] It seems likely, too, that in his first Rule of Philosophizing in Book III of his own *Principia* Newton's demand that we admit only *"causas rerum naturalium . . . quæ et veræ sint et earum phænomenis explicandis sufficiant"*[44]--"causes of natural things that are true and suffice for the explanation of their appearances"--contains an equally deliberate reference to Descartes's introduction of *false* causes.

I must still comment on the sense of "certainty" in Newton's refusal to "mingle conjectures with certainties." Hooke, it will be remembered, said of Newton's theory, "I cannot think it to be the only hypothesis; not soe certain as mathematicall Demonstrations." This last clause alludes to the passage in Newton's paper in which he makes the transition from the discussion of refrangibility to that of color. Newton had written: "A naturalist would scearce expect to see ye science of those"--that is, of colors--"become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall Topick), but evinced by the mediation of experiments concluding directly & without any

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[43]I am not sure that Newton knew *Le Monde;* but it is certain that he had read the *Principia* very closely.

[44]In the first edition, this rule is "Hypothesis I"; and there are small verbal differences--in particular, there is some grammatical confusion of number and mood, with *vera* and *sufficiunt* for *veræ* and *sufficiant*.

suspicion of doubt."[45] Hooke's reading of the passage as implying "the certainty of mathematical demonstrations" is certainly understandable; but careful reading shows that that is not what Newton in fact implied: rather, he said that he had discovered a *mathematical theory* (or science) of colors, and that this theory--based upon experiment--was as certain as any part of optics. In his reply to Hooke, he points this out very clearly, and goes on to explain (a) that *physical* principles, which are always based on *experiment,* can never rise to the certainty that attaches to mathematical demonstrations, and (b) that a science is mathematical if its principles are such as to allow the determination of phenomena by mathematical argument *from* those principles. (One should remember Newton's statement about geometry, fourteen years later, in his preface to the *Principia:* that "geometry is founded in mechanical practice," and that "it is the glory of Geometry that from those few principles, fetched from without, it is able to produce so many things.") But the passage is worth quoting, in part, verbatim, for a little further instruction it contains on Newton's conception of method--both in 1672, and for the rest of his life:

> [T]he Propositions themselves [of the theory of colors] can be esteemed no more then *Physicall Principles* of a Science. And if those Principles be such that on them a Mathematician may determin all the Phænomena of colours that can be caused by refractions . . . I suppose the *Science of Colours* will be granted *Mathematicall* & as certain as any part of *Optiques.* And that this may be done I have good reason to beleive, because ever since I became first acquainted with these Principles, I have with constant successe in the events made use of them for this purpose.[46]

What this last sentence intimates is that, in Newton's view, what he calls "deduction from experiments" is not the last word in establishing the "certainty" of physical principles. Indeed, there *is* no last word--that is the difference between "mathematical" and "physical" certainty. Rather, physical principles once established, the process of

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[45]*Correspondence,* I, pp. 96-97. The passage was omitted by Oldenburg when he published Newton's paper, and therefore does not appear in the Cohen edition; likewise for the corresponding passage in Newton's reply to Hooke.

[46]*Correspondence,* I, pp. 187-188.

their subsequent "proof" by experiments in principle never ends[47] (Newton, I think, has this point in a far more cogent form than does Karl Popper). And a satisfactory level of "certainty" is achieved, not, in general, by an *"Experimentum Crucis,"* but by a sufficiency of such experimental "proof."

Thus far, under the head of "multiple vision," I have touched on the distinction of metaphysical principles, or ultimate explanatory aims, from a level of theory securely established by evidence, although short of such aims; on the distinction of the latter--the "certainties"--from the conjectures or hypotheses that may serve to adumbrate possible explanations and to guide research; and, so far as concerns these conjectures, on Newton's willingness to entertain alternative hypotheses--a clear corollary of the distinction itself between "certainties" and "conjectures." Another (related) aspect of this *multiply nuanced* visionary capacity is Newton's vision of the future progress of science--which contrasts rather strikingly with the almost desperate claim of Descartes in his *Discours de la Méthode* to be on the verge of establishing, himself alone, essentially all possible human scientific knowledge.[48] It is, to me, rather poignant to read in the inaugural set of lectures delivered by the twenty-seven year old Newton (to what

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[47]This is discussed in considerable detail in "Further Considerations on Newton's Methods," as is the claim made in the following sentence, and the related question of just what Newton meant by a "deduction from phenomena."

[48]See, e.g., Descartes, *Philosophical Writings,* I, p. 145 (in Part VI of the *Discourse*): "For my part, if I have already discovered a few truths in the sciences . . . , I can say that these discoveries merely result from and depend upon my surmounting of five or six principal difficulties . . . . I even venture to say that I think I need to win only two or three other such battles in order to achieve my aims completely, and that my age is not so far advanced that I may not in the normal course of nature still have the time to do this." Cf. his remarks in Part II (ibid., p. 116) that "there is not usually so much perfection in works composed of several parts and produced by various different craftsmen as in the works of one man," and in Part VI (ibid., pp. 146-148) that, in effect, no cooperative effort of thought has been or is likely to be of any use to him; that "if there was ever a task that could not be accomplished so well by anyone other than its initiator, it is the one on which I am working"; and that the one kind of aid he needs is that of hired hands to carry out experiements under his supervision.

audience?!) at Cambridge this passage, occurring in a digression that excuses the introduction of the subject of colors into the lectures of a professor of mathematics:

> [S]ince an exact science of [colors] seems to be one of the most difficult things that Philosophy is in need of, I hope to show--as it were, by my example--how valuable mathematics is in natural Philosophy. I therefore urge geometers to investigate Nature more rigorously, and those devoted to natural science to learn geometry first. Hence the former shall not entirely spend their time in speculations of no use to human life, nor shall the latter, while working assiduously with a preposterous method, perpetually fail to reach their goal. But truly with the help of philosophizing Geometers and Philosophers who practice Geometry, instead of the conjectures and probabilities that are being marketed everywhere, we shall finally achieve a natural science secured by the highest evidence.[49]

But also in metaphysics proper we find examples of this disciplined ability of Newton's to entertain conceptions at more than one level, and to exercise critical discrimination among these levels. Here the most astonishing document is the unfinished paper *De gravitatione et æquipondio fluidorum,* first published in 1962 and dated by most experts to the mid-to-late 1660's, roughly three-fourths of which--some 23 pages in Latin, 26 in the English translation--is occupied with a metaphysical digression concerning the nature of space and body, discussed in the light of the exigencies of physics on the one hand, and the teachings of theology on the other.[50] In my opinion, this fragment deserves to be considered one of the most interesting metaphysical disquisitions of the seventeenth century; and if it does indeed derive from Newton's early years, it is remarkable testimony not only to the depth of his thought in that period, but to the extraordinary coherence of his scientific and philosophical

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[49]*The Optical Papers of Isaac Newton,* vol. I, ed. Alan E. Shapiro (Cambridge University Press, 1984), pp. 87, 89 (Latin original on pp. 86, 88); I have departed slightly from Shapiro's translation.

[50]Newton, *Unpublished Papers,* pp. 90-121 (Latin), 121-156 (English). The metaphysical digression occupies pp. 91-114, 123-148.--Unfortunately, the English translation given by the Halls is very seriously defective; some instances will be of concern to us below.

development. I should like, in the rest of this paper, to outline the doctrine there expounded, and to put it in relation to what I believe to have been a great transformation of Newton's vision of physics that occurred during the composition of the *Principia.*

The metaphysical digression occurs almost at the very beginning of the essay, immediately following four Definitions: of place, body, rest, and motion. Place is defined as "a part of space which a thing fills adequately":[51] that is, the basic notion is "the place of a thing"--*p(A);* and its definition identifies the place of *A* as that precise part of space which is occupied, point for point, by *A,* to the exclusion of anything else of the same kind as *A.* For Newton explains that in using the verb *implere*--"to fill"--it is just this exclusion of like things that he intends. Body is defined, correspondingly, as that which fills a place; and Newton remarks that if he were not considering only bodies--*impenetrable* things--he would have defined place more generally as a part of space in which a thin *is* adequately. Rest then is defined as remanence in the same place, motion as change of place.

Remarking, next, that these definitions are at variance with the notions of Descartes, both in taking space to be distinct from body and in referring motion to the parts of space rather than to the positions of contiguous bodies, Newton undertakes to defend his view and (as he puts it) "to dispose of [Descartes's] fictions"--or "feignings": *Figmenta* (derived in fact from *fingere*).

The first part of the ensuing discussion is a summary of Descartes's doctrine about the nature of motion, carefully supplied with references to the *Principia Philosophiæ,* followed, first, by a citation of further passages in which Descartes himself contradicts his own position, and then by a series of arguments demonstrating the utter incoherence of Descartes's conceptions as a foundation for the physical theory of motion. (Some commentators persist in finding in this text an essentially theological basis for Newton's rejection of Descartes.[52] For my part, I can only say that, although it is true that theology figures significantly in the later portions of Newton's discussion, which also touch on the defense against atheism, a reading of this argument as a whole that finds its *basis* to be theological seems to me as absurd a *misreading* as Hooke's of

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[51]Not, as the Halls have it, "evenly."

[52]E.g., Richard S. Westfall, *Never at Rest: A Biographys of Isaac Newton* (Cambridge University Press, 1980), p. 302: "The gravamen of [Newton's] charge [against Descartes] was atheism"(!).

the paper on light and colors, which found its basis to be the corpuscular theory of light.)

After this negative critique of Descartes on place and motion, Newton turns to his own positive view, first of space, and then--more tentatively--of body

He begins his account of space with a strange-sounding declaration: "Perhaps," he says, "it may now be expected that I shall define extension to be substance or accident, or else nothing at all"; and at once denies all of these. I take it that the possibility "nothing at all" is a reference to the Greek atomists, who called the void τὸ μ**Error!)Error!** ν, "non-being"; Newton is thus dissociating himself from the atomist, Aristotelian, and Cartesian traditions, all three. His principal reason for denying that space is substance is that "it does not support [or "stand under"] characteristic affections of the sort that denominate substance,[53] namely actions, such as are thoughts in a mind and motions in a body." This reference to *action* as characteristic of substance is, he says, not the usual definition of the philosophers, but it is understood tacitly (he claims) by them all. On the other hand, in an important respect, space--so far from being "nothing"--is much more "something" than is any accident, and approaches rather to the nature of substance: for we can clearly conceive it as existing without any subject, as when we imagine extramundane spaces or places empty of bodies.

Space has, according to Newton, "its own mode of existing which fits neither substances nor accidents"; it is (and here some *explication de texte* will be required--and some discussion of the translation) "as it were an emanative effect of God, and an affection of every being [or "thing"]." In the published translation, the last phrase is rendered: "a disposition of all being."[54] The Latin is *omnis entis affectio.* Now, the standard rendering of "affectio" into philosophical English is certainly "affection"--not "disposition," whose connotation is quite inappropriate. As for "omnis entis," it can bear either the abstract construction, "of all being," or the concrete one I have preferred; evidence of Newton's intention is afforded by comparison with a later passage, which we shall come to presently.

The statement that space is *tanquam Dei effectus emanativus,* "as it were an emanative effect of God," has unquestionable affiliations with Neoplatonic philosophy;

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[53]The Hall's translation here (*Unpublished Papers,* p. 132) is very bad: "it is not among the proper dispositions that denote substance." The Latin reads (ibid., p. 99): non *substat* ejusmodi proprijs *affectionibus* [etc.]" (emphasis added).

[54]Ibid., p. 132; Latin, p. 99.

but as we shall see in a moment, Newton gives this notion a very unusual twist. First, we must understand that the term "emanation" implies a source of existence that does not involve an act of creation, but rather a derivation or "flowing" from the essence of something. It is of some interest that the *Oxford English Dictionary* quotes a poem of Henry More to exhibit this usage; and also cites a use in a treatise on logic of 1628 with the simple meaning "logical consequence."[55]

Now, the crucial passage in Newton's ontological discussion of space (and also of time, or duration) is rendered as follows in the published translation:

> Space is a disposition of being *qua* being. No being exists or can exist which is not related to space in some way. God is everywhere, created minds are somewhere, and body is in the space that it occupies; and whatever is neither everywhere nor anywhere does not exist. And hence it follows that space is an effect arising from the first existence of being, because when any being is postulated, space is postulated. And the same may be asserted of duration.[56]

Once again we find "disposition" instead of "affection" for *affectio.* But two other defects are more serious, both concerning the phrase "an effect arising from the first existence of being." One would not guess from the translation that "effect arising from" is in the Latin *effectus emanativus*--"emanative effect," the same words as before. And "the first existence of being"--which is a monstrous phrase, impossible for Neoplatonism or any other philosophy I know of--is in violent discord with the very syntax of the original: *"et hinc sequitur quod spatium sit entis primario existentis effectus emanativus,"* "and hence it follows that space is an emanative effect of the first-existent being" (*existentis* is an adjective, modifying *entis,* whose genitive case it shares, and *primario* is an adverb, not an adjective: "first-existent," not "first existence"; and not "existence of being," but "of first-existent being." That the concrete construction (with an article supplied in English) rather than the abstract construction is here intended seems to me clear beyond a doubt from Newton's own explication: *"quia posito quodlibet ente ponitur spatium"*--"for if I posit any being whatever, space is posited." So I translate the whole passage thus:

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[55]The latter citation reads: "1628. T. Spencer *Logick* 199 This truth is necessary by emanation, and consecution."

[56]*Unpublished Papers,* p. 136; Latin, p. 103.

> Space is an affection of a being just as a being. No being exists or can exist that is not in some way related to space. God is everywhere, created minds are somewhere, and a body [is] in the space it fills, and whatever is neither everywhere nor anywhere is not. And hence it follows that space is an emanative effect of the first-existent being, for if I posit any being whatever space is posited. And the like may be affirmed of Duration.

Note particularly here that although Newton has said that space is "as it were an emanative effect of God," this passage explicitly *does not* derive space from theology. Space is "an emanative effect of *the first-existent thing*," and that, according to Newton's theology, is indeed God; but "if I posit *any* thing, space is posited."

The remaining part of the metaphysical digression discusses the nature of body. Here, Newton says, the explication will be more uncertain, because body is a divine *creation*, i.e., exists by an act of the divine *will*, and it is not given us to know all possible ways in which God could have produced the effects we discern. But he proceeds to describe one possible manner in which God could have created beings similar, in all ways known to us, to the bodies we are acquainted with. He quite clearly mimics, in this, the creation story of Descartes; and mimics, also, Descartes's phrasing in the *Principia Philosophiæ:* "And so let us feign"--*"fingamus itaque"*--says Newton, that God causes some previously empty region of space to become impervious to penetration by bodies; that, as it were (this is my own gloss), he creates a *field of impenetrability.* "Feign" in the second place that this impenetrability is not always preserved in the same part of space, but rather is allowed "to be transferred hither and thither according to certain laws," but in such a way that the shape and size of the impenetrable region are conserved. If there are several such *mobile impenetrability fields,* then since *ex hypothesi* they cannot penetrate one another, the laws governing their migrations must be such as to determine their behavior in the event of impingement upon one either of another, or--if we make such a distinction--of a body of the "ordinary" kind; in other words, those laws must include laws of impact.

Finally, according to Newton, a third supposition is necessary, if these newly created regions of impenetrability are to have all the essential attributes of the matter we know: they must be able to interact with minds--able, that is, to excite perceptions in the latter, and susceptible in turn of being moved by them. If all three conditions are

met, Newton says, the mobile impenetrable spaces would be, to us, entirely indistinguishable from what we call "bodies."[57]

In his further discussion of this analysis of the nature of body, Newton remarks that it replaces the obscure--he says "unintelligible"--notion of a substrate or "substance" endowed with a "substantial form" with the clear one of extension (of this, he has said, we have "an Idea the clearest of all")[58] to which there have been imparted clearly specified "forms" (impenetrability, laws of motion, laws of interaction with minds);[59] he adds: "If there is any difficulty in this conception it is not with the form God imparts to space, but with the manner by which he imparts it. But that is not to be taken for a difficulty since the same [point] occurs with regard to the manner by which we move our limbs, and nonetheless we believe ourselves able to move them."[60]

This one obscurity--our deficiency of knowledge of the *laws* of minds and of their interaction with bodies--is touched on by Newton in one more place, after he has passed on to argue the "usefulness of the Idea of body" he has described in illuminating the principal truths concerning God and his relation to the world. "Substantial reality," he says, "is rather to be ascribed to Attributes of this kind which are of themselves real and intelligible and do not need a subject in which they inhere, than to some subject . . . of which we can in no way form any Idea. . . . In the same way if we had an Idea of that Attribute or power by which God through the sole action of his will can create beings, we might perhaps conceive that Attribute subsisting as it were of itself and involving his other attributes."[61] In other words, this "substance-free" conception of substances--as regularly connected systems of attributes--might be extended, not only to created minds as well as bodies, but even to God himself. But, with his characteristically firm discrimination between (a) what is clear and not clear and (b) what we know and do not know, Newton ends this section of his account with what seems to me still the wisest statement on record concerning the mind-body problem: "But so long as we remain unable just to form an Idea of that Attribute [of God], or of our own power by which

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[57]For all of this, see *Unpublished Papers,* pp. 138-140; Latin, pp. 105-106.

[58]Ibid., p. 99: "extensionis Ideam habemus omnium clarissimam"; translation, p. 132: "we have an exceptionally clear idea of extension."

[59]Ibid., p. 140; Latin, p. 106.

[60]Ibid., p. 141; Latin, p. 107.

[61]Ibid., pp. 144-145; Latin, pp. 110-111.

we move our bodies, it would be rash to say what may be the substantial foundation of minds."

Substantiality, then, is to be divorced from the notion of a "substrate," but associated with that of *powers of acting;* and such powers are to be conceived, to be made intelligible, to be understood, through *laws* of motion.--Whitehead, in Chapter IX of *Science and the Modern World,* cites William James's essay "Does Consciousness Exist?" as inaugurating a new stage in philosophy, and compares that essay with Descartes's *Discourse on Method* and *Meditations*.[62] Descartes, Whitehead tells us, sounded the keynote of modern philosophy: the notions of "ultimate mind" and "ultimate matter," and the problem of their relationship. James "clears the stage of the old paraphernalia; or rather he entirely alters its lighting": he denies that consciousness is a "stuff," asserting that it is a "function." In these terms, Newton, in the text we have been considering, suggested not only that minds, but--and more emphatically--that *bodies* are best conceived, not through the notion of "stuff," but through that of "functions."

In its application to bodies, this Newtonian metaphysic readily accommodates the metaphysical view of fundamental explanation I have earlier associated with Huygens (which, as I have also said before, Newton seems in his early years to have shared). One need only stipulate that the laws of migration, or motion, of the regions of impenetrability are just the laws of impact that Huygens had discovered. These are--again, as already noted--exactly what Newton did refer to in his early manuscripts as "the laws of motion," and as the laws of impact of "hard bodies."

In Newton's terminology, a body is called "solid" (in contrast to "fluid") if it resists division; and called "hard" (in contrast both to "soft" and to "elastic") if it is perfectly rigid, or nondeformable. The hypothesized regions of impenetrability created by God, having constant shape and size, are, therefore, *solid* and *hard.* They are of course--*ex hypothesi--impenetrable.* And if they satisfy suitable laws of motion--e.g., the known laws of impact--they must have *mass,* since mass is a parameter entering those laws.

Now, it is very interesting to note that in Newton's *Principia* a certain reservation appears concerning the laws of impact of hard bodies. In the scholium to the Laws of Motion, Newton mentions "the theory of *Wren* and *Huygens,*" by which "bodies absolutely hard return one from another with the same velocity with which they meet"; and then adds: "But this may be affirm'd with more certainty of bodies perfectly

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[62]Alfred North Whitehead, *Science and the Modern World* (reprinted New York: The Free Press, 1967), pp. 141-144.

elastic."[63] It is still more interesting to see that in his last pronouncement on this subject--in Query 31 of Book III of the *Opticks*--Newton *definitively rejects* the Huygensian theory of ultimate interaction: he writes, "Bodies which are either absolutely hard, or so soft as to be void of Elasticity, will not rebound from one another. Impenetrability makes them only stop."[64]

It is quite clear that this revision in Newton's view about the impact of ultimate particles is no mere technical change: the new position is utterly incompatible with the principle that all interaction is "mechanical" by contact.[65] In other words, although Newton never explicitly commits himself to action at a distance as a fundamental principle, that is what is unequivocally implied by this statement--introduced in the guise of a passing remark.

The *Principia* itself, in its opening sections--the Definitions and Laws--develops the conceptual structure for the new form of explanation, precisely to the extent required for physics itself. The central notion is that of a *vis naturæ*--a "force of nature"--or, as Newton also calls it in his Preface (here once more echoing Cartesian terminology),[66] a *potentia naturalis*--a "natural power." A significant body of comment on Newton has been led astray by failure to recognize just what Newton's concept of "force" is. One such force of nature is what Newton calls the *vis insita* of bodies: their "innate (or inherent) force" or "force of inertia" *(vis inertiae).* All other forces of nature are forces of *interaction;* such a force, when it acts on a body, is said to be "impressed" upon it; and the relationships among the innate and impressed forces and the motions of bodies are governed by the three Laws of Motion. These, although Newton says with some--

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[63]Newton, *Principia,* Scholium to the Laws of Motion and their Corollaries; Cajori ed., p. 25.

[64]Newton, *Opticks,* p. 398.

[65]Indeed, on this view, if all interactions occurred through impacts of fundamental particles every interaction would entail an agglomeration of matter, and elasticity of any sort would be impossible: there would be nothing that could cause a rebound, or a repulsive force of any description. In particular, the ether Newton suggests in the celebrated twenty-first Query of the *Opticks* (pp. 350-352), whose "exceeding great elastick force" may be the cause of gravity, is itself inexplicable on "mechanical" principles, given Newton's cited position about the impact of "hard" bodies.

[66]See, e.g., Descartes's Rule Eight; *Philosophical Writings,* I, p. 29.

not complete--justice that they are implicit in the work of his predecessors, are, in the *Principia,* put to the service of a radically new plan of inquiry.

Of course what precipitated the new conceptual scheme was Newton's discovery of the first known interactive force of nature. Notice, by the bye, that what may seem (to put it anachronistically) Newton's heterodox use of the word "force" is in fact essentially the same usage that is employed today, when physicists speak of the "four fundamental forces"; the one Newton discovered--gravitation--remains still among the fundamental ones.

But it is in the *Opticks* rather than the *Principia* that Newton makes most clear and explicit just what this general notion of a force of nature *is.* A force of nature is a *law* of nature--of a suitable type; thus, for instance, we can speak of "the same force" when the same law governs--a point of critical importance for Propositions IV-VII of Book III of the *Principia.* Among forces there is one major dichotomy: between the force of inertia, which Newton calls a "passive Principle," and all others, which he calls "active Principles" (what I have referred to as "forces of interaction"). Here are the central passages. First: "The *Vis inertiæ* is a passive Principle by which Bodies persist in their Motion or Rest, receive Motion in proportion to the Force impressing it, and resist as much as they are resisted."[67] Thus it is the *conjunction* of *all three* "Laws of Motion" that characterizes the "force of inertia." And then: "It seems to me farther, that these [fundamental] Particles have not only a *Vis inertiæ,* accompanied with such passive Laws of Motion as naturally result from that Force, but also that they are moved by certain active Principles, such as that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies. These Principles I consider, not as occult Qualities, supposed to result from the specifick Forms of things, but as general Laws of Nature, by which the things themselves are form'd; their Truth appearing to us by Phænomena, though their Causes be not yet discover'd."[68]

It should be clear how naturally this new scheme fits into the deeper metaphysics of *De gravitatione.* The "general Laws of Nature" associated with each *vis naturæ*--or rather, those associated with the truly *fundamental* or *ultimate* forces--are simply to be included among the laws governing the migrations of the ultimate bodies (the fields of impenetrability). Newton suggests, both in the preface to the *Principia* and in the

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[67] *Opticks,* p. 397.

[68] Ibid., p. 401.

Opticks,[69] that all the active forces may be what we should call "central force fields" associated with the fundamental particles. Among his speculative suggestions as to what "active principles" of this kind there may be to be found out in later research, one in particular deserves to be mentioned. Near the beginning of the lengthy last Query in the *Opticks* he writes: "The Attractions of Gravity, Magnetism, and Electricity, reach to sensible distances, and so have been observ'd by vulgar Eyes, and there may be others which reach to so small distances as hitherto escape Observation; *and perhaps electrical Attraction may reach to such small distances, even without being excited by Friction.*"[70] I cannot resist once more quoting Blake--one of his "Proverbs of Hell": "What is now prov'd was once, only imagin'd."

Concluding remarks: Descartes begins his philosophy with metaphysics; immediately after the *cogito,* with God, upon whom, he maintains, all of his physics rests (and by whose guarantee it is true beyond a doubt). Newton introduces into the beginning of his natural philosophy only just that part of what I have called his metaphysics that he regards as (a) adequately supported by prior evidence, and (b) necessary for the development of physics. The rest, in so far as it appears at all in his scientific work, does so at the *end:* in the *Principia,* in the General Scholium, where after some theological discussion he concludes:[71] "And thus much concerning God; to discourse of whom, *from the appearances of things,* does certainly belong to Natural Philosophy"; in the *Opticks,* at the end of the last Query, in a section that begins: "All these things being consider'd, *it seems probable to me* that God in the Beginning form'd Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties, and in such Proportion to Space, as most conduced to the End for which he form'd them . . . ."[72] Observe that this is the same creation story as in *De gravitatione*--with its deeper but more speculative ontology suppressed. The "other Properties," besides those detailed in the list "solid, massy, hard, impenetrable, moveable," with "Size" and "Figure," all of which we encountered in *De gravitatione,* are those involved in the active forces of nature. Note, too, Newton's striking phrase about these (quoted

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[69]Ibid., p. 397.

[70]Ibid., p. 376 (emphasis added).

[71]That is, concludes the theological discussion--not the scholium as a whole. In the Cajori edition, the passage occurs on p. 546. (Emphasis added.)

[72]*Opticks,* p. 400

earlier): "general Laws of Nature, *by which the Things themselves are form'd*," echoing *De gravitatione'* s remark about the clear "forms" (contrasted with the obscure--occult--scholastic "substantial forms") imparted, not to obscure "prime matter," but to clear "parts of extension": the clear forms are fields of force, and Newton is suggesting that some among these--the most fundamental of them--may be what constitutes the ultimate *natura rerum.*

But always only "may." In the *Opticks,* Newton says of the active principles that their truth appears by the phenomena, "though their Causes be not yet discover'd"--and a little later, "I scruple not to propose the Principles of Motion above-mention'd, they being of very general extent, *and leave their Causes to be found out*." In the preface to the *Principia* he says, "I hope the principles here laid down will afford some light either to that, *or some truer,* method of Philosophy."

Newton was neither a modest nor a moderate man; but these expressions are not a pose. They express a deep conviction about the nature of scientific inquiry itself, and are one in spirit with the appeal for a disciplined search for mathematical principles of natural science that we have seen at the beginning of Newton's career, in the *Optical Lectures.* That a man of such imperious disposition never--into his most advanced age--made disproportionate claims about his contributions to science is one aspect of a tremendous discipline which he exerted--on the whole with remarkable success--in keeping the "imagin'd" parts of his vision distinct from the "prov'd"; the "conjectures" from the "certainties."