Newton's "Experimental Philosophy"

by Alan E. Shapiro

My talk today will be about Newton's avowed methodology, and specifically the place of experiment in his conception of science, and how his ideas changed significantly over the course of his career. I also want to look at his actual scientific practice and see how this influenced his views on the nature of the experimental sciences. I will begin with an investigation of the meaning of "experimental philosophy" for Newton.

Newton is deservedly renown as an experimentalist, and the term "experimental philosophy" has long been associated with his name. It is, however, even more closely associated with Restoration science and the early Royal Society. When Newton was just beginning his scientific studies the term was widely used and appeared in the titles of such works as Robert Boyle's *Some Considerations Touching the Usefulness of Experimental Natural Philosophy* (1663) and Henry Power's *Experimental Philosoph* (1664). There is some irony in this situation, for there is little continuity between the early use of the term and Newton's. Newton consciously avoided using the term "experimental philosophy" until the beginning of the eighteenth century, when its early proponents were gone from the scene and he could impose a new meaning on it.

Newton's 18th-century claims that his science is an experimental one stand in strong contrast to his early statements on scientific methodology in which he claimed that his was a mathematical science and he assigned experiment a subsidiary role. For example, in his *Optical Lectures* in 1670, he announced his new experimental theory of color with the declaration that:

> The generation of colors includes so much geometry, and the understanding of colors is supported by so much evidence, that for their sake I can thus attempt to extend the bounds of mathematics somewhat, just as astronomy, geography, navigation, optics, and mechanics are truly considered mathematical sciences even if they deal with physical things…. Thus although colors may belong to physics, the science of them must nevertheless be considered mathematical, insofar as they are treated by mathematical reasoning.[1]

While in the heading to this passage Newton avows that "these propositions are to be treated not hypothetically and probably, but by experiments or demonstratively," in the passage itself he speaks only about mathematics and not experiment, except for a vague reference to "evidence."

He made an even stronger claim as to the mathematical nature and hence certainty of his theory in its initial publication in *Philosophical Transactions* in 1672:

> A naturalist would scearce expect to see ye science of [colours] become mathematical, & 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 phaenomena (the Philosophers universall Topick), but evinced by ye mediation of experiments concluding directly & wthout any suspicion of doubt.[2]

A notable feature of this claim, besides the extreme confidence in his results, is that Newton seems to see experiments as essentially transparent, yielding their meaning with no interpretation. And once again we see the association of certainty and mathematics.

A few months later in a draft of a paper on Newton's rings he made the strongest claims for the certainty of his theory. After presenting a physical explanation of the appearance of Newton's rings when viewed through a prism, he asserted,

> For confirmation of all this I need alledg no more then that it is mathematically demonstrable from my former Principles. But yet I shall add that they w[ch] please to take the paines may by the testimony of their senses be assured that these explications are not Hypotheticall but infallibly true & genuine.[3]

Note how confirmation is by mathematical demonstration and secondarily—only if you think it is worth the bother—by experiment. Newton clearly believed that a mathematical deductive approach would lead to great certainty and that experiment could provide the requisite certain foundations for such a science.

Let me now jump ahead more than forty years to the "experimental philosophy," so that we can see the great change that occurred. Newton's first published use of that term appears in a widely quoted paragraph in the General Scholium added to the second edition of the *Principia* in 1713. After enumerating the various qualities that he had discovered, he declared:

> Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I feign no hypotheses [*hypotheses non fingo*]; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy propositions are deduced from the phenomena, and rendered general by induction .... And to us it is enough that gravity does really exist and act according to the laws which we have expounded….[4]

In a draft of the letter to Roger Cotes, the editor of the second edition of the *Principia*, in which he forwarded the General Scholium to him, Newton made it quite clear who the advocates of the Hypothetical Philosophy were:

> Experimental Philosophy reduces Phaenomena to general Rules & looks upon the Rules to be general when they hold generally in Phaenomena.... Hypothetical Philosophy consists in imaginary explications of things & imaginary arguments for or against such explications, or against the arguments of Experimental Philosophers founded upon Induction. The first sort of Philosophy is followed by me, the latter too much by Cartes, Leibnitz & some others.[5]

It is not a coincidence or accident that Newton first introduced the term “experimental philosophy” in the General Scholium in defense of the *Principia* and the concept of gravity. Indeed, I claim that this was precisely why he introduced the term. All the other passages where Newton introduced the “experimental philosophy” are in the context of defending his natural philosophy and gravity.

The first use of the term “experimental philosophy” by Newton that I have found is in a draft of the paragraph in Query 23 for the Latin translation of the *Opticks* in 1706 where he discusses the method of analysis and synthesis. (This Query was renumbered 31 in the second English edition of the *Opticks*.):

> The business of Experimental Philosophy is only to find out by experience & Observation not how things were created but what is the present frame of nature. This inquiry must proceed first by Analysis in arguing from effects to causes & from compositions to ingredients.[6]

It is revealing of the mathematical foundation of so much of Newton's thought that he chose to explain the method of the experimental philosophy by means of concepts drawn from mathematics, the method of analysis and synthesis or, as he first called it, resolution and composition.

Newton first introduced an account of the method of experimental philosophy, though without using that term, in an intended preface for the first edition of the *Opticks,* which Ted McGuire discovered and published in 1970. What is particularly interesting about this account is that it shows that Newton recognized that physics or natural philosophy is in fact more complex and difficult than mathematics, and we find him in effect rejecting his earlier, virtual identification of the two.

> As Mathematicians have two Methods of doing things w^{ch} they call Composition & Resolution & in all difficulties have recourse to their method of resolution before they compound[,] so in explaining the Phaenomena of nature the like methods are to be used & he that expects success must resolve before he compounds. For the explications of Phaenomena are Problems much harder then those in Mathematicks.[7]

In the version that he actually published Newton eliminated the phrase "experimental philosophy."[8]

It was however only in the last English edition of 1717 (that is, in the edition with which we are all familiar) that he restored the term "experimental philosophy" and more fully elaborated the method of induction:

> This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For

> Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phænomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur.[9]

Newton has at last arrived at a reasonably good description of the contingency of experimental investigation. In a draft for the *Principia* from about the same time he makes it clear that by a more general induction, he means more experiments.[10]

The *Commercium epistolicum* was the report that was issued by the Royal Society to adjudicate the calculus priority dispute between Newton and Leibniz. As if it were not sufficient for Newton to have anonymously written it, he also wrote the anonymous review of it in the *Philosophical Transactions* in 1715. At the conclusion of the review he turns to the many issues of natural philosophy that divide him from Leibniz, and he uses "experimental philosophy" to distinguish his views from Leibniz's:

> The Philosophy which Mr. *Newton* in his *Principles* and *Optiques* has pursued is Experimental; and it is not the Business of Experimental Philosophy to teach the causes of things any further than they can be proved by Experiments. We are not to fill this Philosophy with Opinions which cannot be proved by Phaenomena. In this Philosophy Hypotheses have no place, unless as Conjectures or Questions proposed to be examined by Experiments

For a page and a half he then proceeded to cite the numerous passages where he conceded that he did not know the cause of gravity. In exasperation he lamented:

> And after all this, one would wonder that Mr. Newton should be reflected upon for not explaining the Causes of Gravity and other Attractions by Hypotheses; as if it were a Crime to content himself with Certainties and let Uncertainties alone.[11]

In conclusion he further contrasted the two philosophies and continued to use the resurrected "experimental philosophy" as a powerful resource to ridicule Leibniz:

> It must be allowed that these two Gentlemen differ very much in Philosophy. The one proceeds upon the Evidence arising from Experiments and Phaenomena, & stops where such evidence is wanting; the other is taken up with Hypotheses, and propounds them, not to be examined by Experiments, but to be believed without Examination. The one for want of Experiments to decide the Question, doth not affirm whether the Cause of Gravity be Mechanical or not Mechanical; the other one that it is a perpetual Miracle if it be not Mechanical.[12]

When Newton reintroduced the term "experimental philosophy" he changed its meaning from its earlier Restoration one. For Boyle and Hooke, for example, according to Charles Webster, the terms mechanical and experimental philosophy were synonymous, but Newton used the term as a weapon against the classical mechanical philosophy, especially the Cartesians and neo-Cartesians, who demanded that all physical causes act by contact action.[13] This becomes evident from Cotes' preface to the second edition of the *Principia*, in which he presented and defended Newtonian natural philosophy. He begins the preface by dividing the development of natural philosophy into three groups. First, there are those, in particular, Aristotelians, who endow things with occult qualities. The second group are the mechanical philosophers who opposed the first, but, Cotes tells us, they have drifted off to speculative "dreams" by imagining invisible particles and fluids and are left with an "elegant and charming romance."

> There remains then the third group, namely, those who profess experimental philosophy. Although they too hold that the causes of all things are to be derived from the simplest possible principles [i.e., the mechanical philosophy], they assume nothing as a principle that has not yet been thoroughly proved from phenomena. They do not contrive hypotheses, nor do they admit them into natural science otherwise than as questions whose truth may be discussed. Therefore they proceed by a twofold method, analytic and synthetic. … This is that incomparably best way of philosophizing which our most

> celebrated author thought should be justly embraced in preference to all others. This alone he judged worthy of being cultivated and enriched by the expenditure of his labor.[14]

Cotes thus presents Newton with his “experimental philosophy” as reforming and transcending the mechanical philosophy.

Now that I have presented the evidence that Newton’s understanding of scientific methodology, especially the place of experiment, changed dramatically over a span of half a century, I would like to turn to the reasons for this and look at his actual scientific practice. While Newton, as I have just shown, used “experimental philosophy” as a rhetorical weapon against his critics, his views on experiment definitely changed and we have to look for the sources of that change. I have no definitive answer to this question, but I will suggest a number of likely reasons. In the first place, there is the *Principia.* In developing and establishing his ideas he was compelled to wrestle with such problems as the relation of mathematics and natural philosophy and the problem of evidence. His particular solutions still engross philosophers but they need not concern us here. What is essential is that he developed a much more sophisticated appreciation of them. A second factor, which I will briefly sketch later, is the failure of his early attempt to establish a new mathematical theory of color, which he eventually recognized had little relation to reality and experiment, thus making him much more sensitive to these issues.

A third factor is the criticism that he received, especially from Hooke, on the claims to certainty of his theory of color. In a critique of Newton’s first paper on light and color Hooke rejected Newton’s claim that his theory was as “certain as mathematical Demonstrations." Newton replied:

> I should take notice of a casuall expression wch intimates a greater certainty in these things then I ever promised, viz: The certainty of *Mathematicall Demonstrations.* I said indeed that the *Science of Colours was Mathematicall & as certain as any other part of Optiques;* but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the

> absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall:* Whence the Propositions themselves 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 Phaenomena 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*.[15]

While Newton has here for the first time recognized the contingency of experimental science, he really did not yield much ground. For another few years he continued to insist that experiments conclude “directly & wthout any suspicion of doubt” and that his conclusions are “infallibly true & genuine.”[16]

The final factor is the series of quantitative experiments Newton carried out in his investigation of the colors of thin films. In quantitative experiments it is very difficult to conclude that one has perfect agreement between theory and measurement, and, I believe, one would be rapidly disabused of the idea of certainty. I will devote the rest of my talk to this issue and we will see Newton encountering very good reasons to entertain “suspicions of doubt.”

Experiment has become a very popular area of study and the term "experiment" is freely bandied about, but it is important to recognize that Newton undertook many different kinds of experiment throughout his career and as historians and philosophers we have to treat them differently. I do not intend to enumerate the different kinds of experiments that Newton undertook, but we can start with the distinction between the qualitative and quantitative, though not all qualitative and quantitative experiments are of the same sort.

Newton's theory of the nature of white light and color is essentially based on qualitative experiment. In this theory Newton formulated a series of definitions which classified the sorts of entities (lights and colors) that exist in the world and a sophisticated sequence of principles on how light behaves when it is subjected to various operations or procedures with prisms and refraction. These principles also explained the physical cause of the appearance of colors, namely, that sunlight is a mixture of rays of different degrees of refrangibility and color. Newton's concepts of simple and compound colors are defined operationally in terms of whether they are separated when they are passed through a prism. This is a sharply defined criterion but it is strictly qualitative. The existence of simple rays that are immutable is the bedrock of Newton's theory, and his critics eventually demanded that he prove their existence. The method of preparing simple immutable colors depends on a complex arrangement of prisms, lenses, and apertures, or operations on light. The test is purely qualitative and is decided by the appearance of homogeneity to the eye. We have clearly entered the realm of experiment heavily intertwined with theory. They are not here easily separated. Our conception of Newton's optics as an experimental science is largely based on this theory, which forms Book I of the *Opticks*. It is, I believe, very easy to convince one self that these sorts of experiments transparently yield their meaning as a “rigid consequence,” “directly and without any suspicion of doubt” once one possesses—as Newton did—some rules for interpreting them.

Let me turn to another sort of experiment, which I call mathematical and quantitative. The conjunction—mathematical *and* quantitative—is important, for one can have just quantitative experiment, e.g. measurements of densities, or simply a mathematical description without any explicit quantitative predictions, something like 18th-century rational mechanics. And I will soon show how Newton tried to construct a rational science of color. It is mathematical and quantitative science that defines modern physical science. With this sort of experimental program, Newton formulated mathematical laws that described a range of phenomena, most notably, the colors of thin films and thick plates and diffraction, which together constitute Books II and III of the *Opticks*. The measurements that Newton presented to derive and justify these laws agreed with a high degree of precision with the predictions of his laws. As Sam Westfall wrote many years ago, Newton raised "quantitative science to a wholly new level of precision.…He boldly transported the precision of the heavens into mundane physics…."[17] In fact, I think that we can say that quantitative mathematical and experimental physics scarcely existed before Newton, and he played a major role in its creation. If we accept this claim, as I do, then a number of questions naturally arise, such as when did Newton first develop this goal, at the beginning of his career, or in the *Principia*? The answer, I will argue, lies in between, namely, Newton did not at the beginning of his career have the close quantitative agreement between mathematical theory and nature as one of the central aims of his science and that this concern developed in the early 1670s with his work on the colors of thin films.

To answer this question I will turn to Newton's first physical treatise, his *Optical Lectures*, so that we can see how little concern he initially had for measurement, let alone for agreement between theory and measurement. Newton's earliest optical investigation was his theory of white light and color, which was one of the fruits of the *anni mirabili* in 1665-66. When he was appointed Lucasian Professor at Cambridge in 1669, he chose this theory for his first series of lectures from 1670 to 1672. Newton had intended to publish his *Optical Lectures*, but he abandoned these plans because of the controversies following its publication in the *Philosophical Transactions* in early 1672.

Newton began the *Optical Lectures* with a demonstration of the central idea of his theory, that sunlight consists of rays of unequal refrangibility. The fundamental experiment in demonstrating that discovery is that of passing light through a prism placed symmetrically with respect to the incident and emerging beams and then projecting its image or spectrum onto a screen. Experiments with prismatic spectra were quite common in the 17th century, but Newton's arrangement of passing a narrow circular beam of light through a prism in the symmetric situation—or, as it is now called, at minimum deviation—was an inspired one. He demonstrated mathematically that in this situation the image would be nearly circular if all rays were equally refrangible as was then universally held. He then measured the spectrum and found that it was greatly elongated, about five times longer than broad. At this point in the *Optical Lectures* there are few hard numbers or elaborate measurements to establish this, perhaps because the elongation is so "extravagant" that it mattered little whether precise measurement showed that it was 3 or 4 or 5 times as long as a circle. He had to eliminate other possible causes for the elongation, of course, but once he had established that there could be no other cause than that sunlight consists of rays of unequal refrangibility, he had, or so it seemed, a mathematical measure, a metric, for color: the degree of refrangibility. On the one hand, this experiment provided Newton with the fundamental experimental and conceptual tool of his theory of color, his method of analysis or decomposition: separating rays of different colors from one another by their unequal refrangibility. On the other hand, it allowed the possibility of developing a mathematical theory of color. Newton attempted to develop both paths. The experimental theory is the one that we all know from the "New Theory" and *Opticks*, but Newton also attempted to create a new mathematical science of color.

Newton's plan to develop a mathematical science of color was never more than a program, and it must be reconstructed from his *Optical Lectures*. His aim was to construct a rational science of color, a formal theory in which the refractions of rays of every color in any substance could be deduced from a few mathematical principles and a bare minimum of measurements. One of its major weaknesses was that it was only loosely related to his experimental theory or, for that matter, to any experiment or observation. Its foundation was to be degree of refrangibility, or index of refraction. First he assumed that Snell's law was valid for each color apart, and though he proposed an experimental verification, he rejected it as unnecessary. Then, to describe how the index of refraction varied with color, that is, chromatic dispersion, he constructed a refraction model or law that his notes show is based on Descartes' refraction model in which corpuscles receive an impulse as they cross a refractive surface. In his *Optical Lectures*, however, Newton effaced all traces of its mechanical underpinnings and presented it solely as a mathematical law. When he set forth his refraction model in his *Optical Lectures*, he freely admitted that:

> *Although I have not yet derived the certainty of this proposition from experiments*, nevertheless I do not doubt that it will satisfy all of them which it is possible to do with respect to it … meanwhile [I am] content to assume it gratuitously.[18]

We should recognize how much this early approach varies from all of Newton's public claims that his science is founded on experiment and eschews hypotheses. It is difficult to doubt that he learned an important lesson on experimental evidence from his ultimate failure here.

While the exact form of the law need not concern us, we should note that once the parameters of this model are determined—and only three measurements in one substance are necessary—then knowledge of just a single value of the index of refraction in another substance allows one to determine all refractions in it. Newton was quite proud of this feature and even boasted that with this law one need not "bother anew with experiments."

Newton devotes much of the mathematical section of his lectures to trying to develop a mathematical theory of color. It contains some fancy mathematics, but virtually all of the propositions are erroneous because of his dispersion law, and he makes no attempt to test them. For instance, he proved mathematically that the minimum of angular dispersion (that is, the angle contained by the extreme red and violet refracted rays) occurs when the rays pass through the prism symmetrically. Not only is the position of the minimum mistaken, but with the large-angled prisms Newton used there is no minimum at all. Although it is somewhat difficult to distinguish variations of the deviation and those of the angular dispersion, Newton was an acute observer and performed this experiment many times; in an observation in the *Opticks* he did correct this error. Newton seems almost oblivious to relating his mathematical constructions to the real world by experimental tests. On only one occasion in these two long lectures does he suggest an experimental test, and then only to reject it as insufficiently sensitive.

Even with this brief description, I think that I have made my point. Newton's early mathematical theory of color was barely an experimental science, let alone a quantitative one based on measurement, and had little connection to the qualitative experiments of the experimental color theory.[19] Let me now turn to Newton's investigation of the colors of thin films, for this shows the command of quantitative methods and a quest for precision that we now identify with his science. It also shows why Newton may have been compelled to revise his ideas about certainty.

I do not want to retrace the history of the colors of thin films, but I must note that Newton learned about them from Hooke's account in the *Micrographia* of the colors seen in sheets of mica. Hooke had conjectured that the appearance of the colors were periodic, though he confessed he was unable to measure the thickness of such thin films to demonstrate this. Newton's key breakthrough was his insight that if he put a lens (which is really just a segment of a circle) on a flat plane, then by a principle from Euclidean geometry on tangents to circles he could readily determine the distance between them simply by measuring the circles' diameter. The material realization of a mathematical principle about circles and planes in a physical arrangement (since known as "Newton's rings") marks Newton's brilliance as an experimentalist. Newton seems to have had this insight while reading the *Micrographia* and quickly carried out a rough and ready test in 1666. His results, though quantitatively very crude, were enough to demonstrate—at least to his satisfaction—that the appearance of the colors was a periodic phenomena, and he succeeded in determining a measure of the periodicity.

Satisfied with this fundamental result and that his method worked, Newton set it aside and pursued other research until 1671, when he undertook a serious investigation of the colors of thin films. Over 30 years ago Westfall discovered and published Newton's record of this investigation, "Of ye coloured circles twixt two contiguous glasses."[20] Let me begin with the measurements. Newton tells us little about his experimental arrangement, other than that he had some sort of frame for the lenses, measured the diameters of the rings with a compass, and that he believed he could determine them to fractions of 1/100th of an inch. In the first table we have in column 2 his measurements of the first six dark rings at various angles of incidence; the second set of measurements being made with "The Glasses being not altogether so hard pressed together." These rings are in fact not well defined, and Newton had to overcome the obstacle of working with white light in which the rings rapidly overlap and mix to form a uniform white. With a simple spectral color he could observe more than 20 circles, but the intensity was too low to make any measurements. In column 3 we have the squares of the diameters, because it is these that should increase by a constant quantity if the appearance of the colors is periodic. In the 4th column, he forms the differences—which should be a constant equal to the first value—and in the next column he calculated the simple average. Note how much he measured and remeasured to get what he thought was the best possible measurements. In the last column which was inserted after the table was initially composed, he entered the difference between the first square and the average, for in theory these two quantities should be equal. Such a use of averages is almost unprecedented in the 17th century, and Newton would suppress this in subsequent publication. He was applying new mathematical techniques to analyze his data and seek out sources of error.

To be honest, Newton's data is not that great. For the first ring the difference with the average value varies from about 9% to 2%, decreasing as the angle of incidence increases; and if we calculate the largest difference between the average value and the difference of the squares, it varies from about 7% to 1%, but most range from the 1 to 2% . Newton suspected that something was wrong. His first guess was that pressing the lens to make it touch the flat surface slightly flattened the lens and made the first circle too big. By a series of experimental tests he convinced himself that this was responsible for some of the error, but not all. He wrote plaintively

> I am apt to suspect that there was some imperfection in the glasses t[o]o w^{ch} cheifly caused these varietys namely that the one glasse was not exactly plane nor the other exactly sphaerical & so y^{e} experiment must needs bee various w^{n} made at severall places of the glasses. [Now y^{e} least imperfection of this nature is sufficient to make considerable variation in y^{e} experiment…]

In the manuscript he then drew his symbol for something to note well, a fist with a pointing finger, and declared the resolution of the problem:

> ☛ I find at last that one side of the greate glasse is more convex then the other though both were ground on the same toole. … soe that one makes y^{e} circles about 45 parts greater then the other if applyed to y^{e} same part of the plane of the other smaller glasse, but that is not an exact plane neither…Yet many times they imposed upon mee.

Newton had confidently assumed that the lens was truly biconvex, since he had ground it himself, with a radius of 50 feet. He now concluded that one side had a radius of 51 feet (rounded off) and the other 49 feet.

Newton first wrote up the results of his investigation of the colors of thin films in the spring of 1672, but he did not send it to the Royal Society until 1675. If we turn to the key observation that establishes the periodicity of the colors of thin films we are in for a surprise, for not a single measurement is presented for one of the more significant results of 17th-century optics, and this passage appears virtually unchanged in the *Opticks*. The explanation, I believe, is that Newton had already developed his ideal of near perfect agreement between theory and mathematical law. Since the data was not neat, Newton simply suppressed it. It is not that the data did not support this law. It did to a per cent or two, and Newton had worked very hard to attain this, but it was not perfect. In the next observation he presented the value for the thickness of the film or “interval” required for the reappearance of a given color, the basic parameter for his theory. He presents one measurement for the diameter of the sixth bright ring at normal incidence, and then calculates that the thickness or “interval” was 1/80,000 inch "to use a round number" for yellow light. In "Of y^{e} coloured circles," he determined this value by an average of his measurements for all the rings, but such a procedure then had no legitimacy and was not the sort of strict agreement that Newton wished to present. An average could serve as a representation of the variation of the measurements and not of their stability. Newton's measurement, as best as it can be compared with modern values, is about 10% too large. Still, for the 17th century to be able to measure reliably such a minute distance was as remarkable an achievement as the determination of astronomical distances.

When Newton began to compose the *Opticks* in the early 1690s, he was sufficiently content with his account of the colors of thin films that he incorporated it with little change. As his work proceeded on completing the *Opticks* he wound up undertaking a major experimental investigation of the colors of thick plates, which is a neglected *tour de force*. When sunlight shined on a mirror from a reflecting telescope through a small hole in a screen at the center of the circle from which the mirror was formed, he noticed four or five colored circles surrounding the hole and that they appeared to be like those in Newton's rings except larger and fainter. He was able to give a precise mathematical account of them using the same physical model and parameters that he had earlier developed for thin films. Newton explained the rings by rays that were scattered from the lower surface of the mirror and were able to pass out of the upper surface according as they encountered a compression or rarefaction in the vibrating medium.

Newton had completed the *Opticks* sometime in 1691 or 1692 and had written a new part on the colors of thick plates. For a number of reasons he became dissatisfied with the book and decided to revise parts. If we look at the first completed state of the part on thick plates, we see that Newton had worked out an exact account of the rings and had calculated their diameters. When he compared his calculation to his measurements, he found that they differed by about 5 $^1/_2$%. He did not remain content with a difference of this magnitude. He set about uncovering the source of the error and apparently remeasured the index of refraction and the radius and thickness of the mirror, but they could not account for the discrepancy. Newton had no choice but to reopen his earlier determination of the "interval" which was already incorporated into the *Opticks*. He repeated the experiments with two new telescope lenses that had a much smaller radius of curvature, 7 feet 7 inches and 7 feet 9 inches. While the diameters of these rings were less than half the size of his original measurement, the focal length of about 7 feet could be much more accurately determined than the original one of over 45 feet, which I suspect is the principal source of his error in his earlier determination.

Having become very suspicious of his measurements, Newton also introduced corrections to the measurements that he had not taken into account earlier. One corrects for the measured size of the ring. Since the rings are viewed through a glass lens, the true size of the ring is actually larger than the measurement because of the refraction in the lens. This turns out to be quite significant, about 3.5%. Newton's use of corrections arising from the nature of the measuring apparatus itself is, I believe, unprecedented in early modern physics, though it was widely used in astronomy. With his new measurements and corrections he now had 1/89,000 for the "interval" instead of the old 1/80,000. This was a significant change of 11% that brought the value to close to its modern value. When it was used to recalculate the diameters of the rings in thick plates, it essentially resolved the discrepancy between measured and calculated values.

I fear that I have painted a picture of Newton's "experimental philosophy" that seems almost identical with our "experimental physics." We have learned from Ted that we cannot simply transfer Newton's concepts to our own day, but that we must be sensitive to his concerns. Newton's "experimental philosophy" was in fact much broader than our "experimental physics," just as the "natural philosophy" of the 17th-century cannot be transposed to our "physical science." From a draft for Query 31 for the third edition of the *Opticks* in 1717, we can see how different it was:

> Hypotheses are not to be regarded in Experimental Philosophy. Nor are we here to regard Metaphysical Principles unless so far as they are founded upon experience. For all Metaphysicks not founded upon experience is Hypothetical: And so far as Metaphysical Propositions are founded upon experience they are a part of experimental Philosophy. Even that celebrated Proposition *Ego cogito ergo sum* is known to us by experience. We know that we think by an inward sensation of o^r thoughts. And therefore from that Proposition we cannot conclude that any thing more is true then what we deduce from experience. And even in proving a Deity all arguments not taken from Phaenomena are little better then dreams.[21]

Thus, for Newton "experimental philosophy" included metaphysics and proof of the existence of God. But this is beyond my bailiwick, and I will leave it to Ted to elucidate, if he chooses.

1 A. E. Shapiro, ed., *The Optical Papers of Isaac Newton*, Cambridge University Press (1984), volume 1, page 439.

2 Newton, *Correspondence*, vol. 1, p. 96. Oldenburg deleted this passage in the paper published in *Phil. Trans.* Emphasis added.

3 Observations, ULC MS Add. 3970, f. 525r;emphasis added; see also f. 510v for the unchanged 1675 version..

4 Newton, *Mathematical Principles of Natural Philosophy*, trans., Motte-Cajori, p. 547.

5 Newton to Cotes, 28 March 1713, *Correspondence*, vol. 5, pp. 398-9. Further in the letter Newton elaborated, "Experimental philosophy argues only from phaenomena, draws general conclusions from the consent of phaenomena, & looks upon the conclusion as general when ye consent is general without exception, tho the generality cannot be demonstrated a priori."

6 ULC MS Add. 3970, f. 243r.

7 ULC MS Add. 3970, f. 480v. J. E. McGuire, "Newton's 'principles of philosophy': An intended preface for the 1704 *Opticks* and a related draft fragment." *British Journal for the History of Science* 5 (1970): 178-86.

8 The published version: "As in Mathematicks so in Natural Philosophy the investigation of difficult things by the method of Analysis ought ever to precede the method of Composition. This Analysis consists in making experiments & observations & in arguing by them from compositions to ingredients & from motions to the forces producing them & in general from effects to their causes & from particular causes to more general ones, till the Argument end in the most general: The Synthesis consists in assuming the causes discovered & established, as Principles; & by them explaining the Phaenomena proceeding from them, & proving the explanations." (ULC MS Add. 3970, f. 286r).

9 Newton, *Opticks* (Dover, 1952). p. 404.

10 Newton, *Mathematical Papers*, ed. D. T. Whiteside, vol. 8, p. 452, n. 34.

11 [Newton], "An account of the book entituled *Commercium epistolicum," Philosophical Transactions* 29 (1714/15): 173-224, on p. 222, 223.

12 Ibid. p. 224. I cannot resist including a draft in which Newton invoked his experimental prowess to further ridicule Leibniz: "And must Experimental Philosophy be rendred uncertain by filling it with Opinions not yet proved by any experiment? If M^{r} Leibnitz never found but a new experiment in all his life for proving anything; If M. Newton has by so great multitude of new Experiments discovered & proved many things about light & colours & setled a new Theory thereof never to be shaken" (ULC MS Add. 3968, f. 586v)

13 Charles Webster, "Henry Power's experimental philosophy." *Ambix* 14 (1967): 150-178, n. 66.

14 Newton, *The Principia: Mathematical Principles of Natural Philosophy*, trans. I. Bernard Cohen, and Anne Whitman, with the assistance of Julia Budenz (Berkeley, CA: University of California Press, 1999), p. 386; I have modified the translation of the first sentence.

15 Newton, *Correspondence*, vol. 1, p. 187. This passage too was deleted by Oldenburg in the reply published in *Phil. Trans.*

16 Only many years later when he was revising the latter passage for the *Opticks*, did he tone it down to the unexceptionable claim that "now that all these things follow from the Properties of Light by a mathematical way of reasoning, so the truth of them may be manifested by Experiments." (Newton, *Opticks*, p. 240.)

17 Richard S. Westfall, "Newton and the fudge factor," *Science* 179(1973):751-758, esp. pp. 751, 753.

18 Newton, *Optical Papers*, page 201.

19 I do not want to leave the mistaken impression that there was no quantitative mathematical experiment in the *Optical Lectures,* for it contained novel ways to measure indices of refraction, including the method of minimum deviation, which became the preferred method for measuring indices of refraction for centuries.

20 Richard S. Westfall, "Isaac Newton's coloured circles twixt two contiguous glasses," *Archive for History of Exact Sciences* 3(1965):183-96.

21 ULC MS Add. 3970, f. 621v.