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The Experimental Philosophy or Francis Bacon's Elenchus

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ABSTRACT

Critical rationalism faces difficulty in Karl Popper's Socratic formulation: "I may be wrong, and you may be right, and by an effort, we may find the truth." But the Socratic *elenchus*, using refutations, can only give us negative knowledge of general principles, which is not the wisdom we seek. Affirmatively, we can only find a collection of opinions to be coherent, which is one of many. Francis Bacon proposed an improved elenchus to find general truths. You must take up a limited topic to study, then cross-examine your evidence for and against its apparent nature. Experiments contrary to evidence and presumed knowledge are entered as self-contradictions in tables of opposition recorded in an "experimental and natural history." Such an account highlights a challenging puzzle if the account is to be made coherent. With enough problematized evidence, a coherent reading, or a solution of the puzzle, will be unique. Being both coherent and unique, it will be the truth about that limited reality being investigated. Unlike the method of hypothesis ("Anticipating Nature"), deciphering a coherent model is "Interpreting Nature," allowing us to find a general truth on a limited topic. Isaac Newton achieved great success using Robert Boyle's mechanistic version of this method. Using the "experimental philosophy," he discovered general principles of optics and astronomy.

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I mean by an *elenchus*, a technique of refutations to produce affirmative knowledge of a principle.[1] In arithmetic, for instance, one can prove that there is no smallest rational number or largest prime number by showing that if we suppose a smallest rational or largest prime, each of these numbers would have impossible properties. The principles affirmed as negations or refutations are useful for studying arithmetic. In most cases outside mathematics, denying a principle does not yield knowledge of another useful principle. For instance, a negation of Plato's principle that heavenly bodies move in uniform circles does not generate a principle for astronomical models.

Socrates, however, claimed to have such a method (Vlastos, 1994) – an *elenchus*, we may call it, which he used, by way of refutations, to discern affirmative knowledge of ethical principles. Modestly enough, according to his admirer, Plato, he claimed he was barren of wisdom. But his cross-questioning of principles would give birth to knowledge among his interlocutors. He called this *elenchus* his art of (intellectual) midwifery.

Francis Bacon (Bacon, 2000/1620) proposed a new method of gaining knowledge. Like Socratic midwifery, he had no new science to bequeath to posterity. He bequeathed the method itself, a *new organon*, which later philosophers could use to bring new science to fruition. His *elenchus*, however, was not offered to find new principles of ethics, which were the objects of the Socratic *elenchus*. The Abrahamic religions reserve ethics to be determined by divine command, following the story of Adam's expulsion from paradise. Francis Bacon was a devout Christian and a member of the Church of England. He sought only wisdom in natural philosophy, which he hoped was far enough removed from ethical and other divine matters to keep them safe from skeptical inquiry. In natural philosophy, he proposed a skeptical method that can give us affirmative knowledge of principles.

Others have used his *elenchus* in unappreciated ways in modern natural science, which I hope to describe. Francis Bacon showed how to find new and better principles affirmatively by refutation. His method allowed modern science, with some improvements by Robert Boyle, to make breakthrough discoveries. These improvements were to introduce working hypotheses in the employment of the Baconian method, which was not foreshadowed in Francis Bacon's writing. The new method is hidden in the practice of science. We can no longer enunciate Baconian descriptions of natural science because when we try, we run afoul of some common assumptions we make about how we know whatever we know. But investigators in the natural sciences regularly find hidden knowledge of ordinary things. They do not usually state a general methodology, but they can find such new knowledge by tacitly following a version of Francis Bacon's *elenchus*.

I

Do the experts know what they confidently tell us? Could they merely seem to be knowledgeable because, being scientists, they have high social standing as experts?

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[1] An elenchus is often understood to mean a method of refutation, which was the original meaning. Here I have restricted its meaning to include an affirmative method of refutation.

Twenty-some years ago, there was a battle of words in print and online about this issue, called "science wars." Some scientists insisted they could find some objective truths about real things. Most of us would allow it, though we would be happier if they could describe how they acquire objective knowledge of any hidden reality.

But not everyone has agreed that scientists have such knowledge. Their critics denied that they or anyone else could know what they claimed. Invoking, among others, some well-known philosophers and historians of science such as Karl Popper, Paul Feyerabend, and Thomas Kuhn, some critics pointed out that the most general claims of science are hypotheses. They are better described as speculations, opinions, conjectures, paradigms, and even myths rather than knowledge, no matter how well they are researched. Science and Technology Studies (STS) adopted Thomas Kuhn's *Structure* (Kuhn, 1962) as the canonical description of science. However, they claimed that the practices of science and technology had yet to be filled in by their ongoing Kuhnian social research. They took Thomas Kuhn's somewhat Romantic conception of normal science (Hattiangadi, 2021) as a model for their professional organization in which dissent is barely tolerated and not encouraged.

The three influential philosophers mentioned above considered themselves friends of science, and everyone would grant that the description applies at least to Karl Popper. He avowed realism in his understanding of science, which is to say that he thought that some theories of science that try describe hidden reality might get it right. However, scientific realism is one option. Some other commentators in the "science wars" debate were belittlers of science. In STS, for instance, several practitioners ignored the objectivity of the reality that scientists invoke. They chose instead to study epistemological practices without making any realistic commitment themselves or denying it altogether. In reviewing the epistemological practices of science, they can ignore the reality of which the scientists write. However, the science and technology student may note that scientists imply or speak of truth and reality. Any such referred reality may be treated as an epistemic social construct, in which case the realistic scientist is duped.

Although it did not motivate my research, I will use the science-wars debate to frame the issues of this paper. If realistic scientists are correct, they bring us closer to reality than even their friends and admirers allow, e.g., Karl Popper. Scientists claim to learn about some truth that lies beyond their epistemological practices. How do natural scientists manage this feat? I want to address this issue to frame my paper so that it is relevant to current topics in the Philosophers of Science and to those STSers who have not pledged their troth.

Karl Popper, in 1961, described science as an ongoing critical examination of promising hypotheses. Critical rationalism is a very beguiling account of how science grows. A scientific theory may be an opinion, i.e., a conjecture. But if we subject it to severe and honest critical scrutiny in the form of empirical tests of its truth, we may find the truth we seek through Socratic dialogue. This simple account of science is an application of critical rationalism.

The form of critical rationalism he proposed, and the theory of science he advocated, do not show how affirmative knowledge of abstract principles is possible. Though Karl Popper was a staunch supporter of science, his inability to show how affirmative knowledge is possible, in my diagnosis, is a vital part of why the STS folk went off the deep end about any objective reality. Science for critical rationalists consists of endless debate, as social theories in STS might say, which implies that there is no internal closure of any discussion. For closure, we must go beyond arguments to a social fiat if critical rationalism is to be reconciled with scientific practice. I suspect we can improve upon Karl Popper's account of critical rationalism for some cases in science, filling this cavity of reason at the center of STS.

Critical rationalists face difficulty. We can begin with Karl Popper's Socratic formulation that criticism makes humans rational. But the aim of science, he says (Popper 1961 Ch. 3.), is the truth, which is fallibly known. Plato would have concurred. However, a "Popperian *elenchus*" does not work if he has critical thinking as his only tool. The truth is not just a pile of hypotheses discarded as false because they are refuted[1]. How can empirical tests perform the trick of giving us affirmative knowledge? It cannot. Every form of criticism that Karl Popper has described, including empirical falsification, can only yield negations of abstract knowledge.

Let us generalize this discussion a bit. Should my opinion contradict known facts, we can review and revise some of my adduced facts or revise my opinion. Suitably amended, my new and revised thoughts become internally coherent. That is the most critical rationalism can do for me. But it is not enough to be internally coherent[2].

Many alternative belief systems are each internally coherent; if not, we can make them coherent by adjusting them. There are too many alternatives in natural philosophy, each internally logically consistent. Testing each variant of each type is beyond our capability. Though each is adjusted to be internally coherent, any two belief systems will contradict one another. (If not, the coherent two can be combined into one coherent whole.) At most, one alternative can be true of many coherent collections of beliefs we may have. We have no means of finding which whole collection is true or even if we have formulated it. That would be a depressing fact of epistemology if we were seeking any wisdom.

Socrates faced such a problem, and Plato (in the *Gorgias*) showed that Socrates did not have the resources to solve it. Gregory Vlastos (1994) wrote a beautiful paper showing how Plato exhibits the Socratic difficulty in his dialogue. In later dialogues, Plato abandons further discussion of the problem of *elenctic* knowledge, having decided perhaps that he could not solve it. Beginning in *Meno* (Plato 1961), he went on to explore a new method - the method of hypothesis. Mathematicians, he wrote, state a hypothesis and, assuming it to be clear and true, use it to shed light on what follows from it without inquiring into whether it is true. They postulate its truth. Plato

[1] To be discarded is not the same thing as to be refuted, of course. Paul Feyerabend (1975) makes a strong case for retaining refuted theories as part of pluralism in science.

[2] By interpreting science as philosophers seeking coherence in knowledge I do not endorsre a coherence theory of truth or even coherentism as an epistemology. We need to think in terms of general coherence whenever facts and hypotheses may be both in question, given the theory ladenness of facts. I am, however, assuming a version of Tarskian truth, and endorsing fallibilism about knowledge.

remained faithful to Socrates in one respect. Of all the varied principles assumed in hypothetical science, the choice of one principle as true must be made by a Socratic dialectic, wrote Plato (1961) in the *Republic*. However, he did not show how that feat was accomplished by using the Socratic dialectical *elenchus*.

If we interpret Plato's problem in modern terms, it is faced by all coherence theories of knowledge. Showing that a principle is part of our coherent web of thought is not enough to argue that it constitutes knowledge. We must also show that nothing else is coherent with all the known facts that do not entail the principle itself. If we are studying a model of reality, we need to show that the model is uniquely coherent to claim that it is true of reality. Every other construal of reality must be incoherent. It is a tall order to make that case.

Pyrrhonian skeptics (Sextus, 2000) introduced the argument of the standard of truth or the criterion. We need a criterion for true belief, for many knowledge claims exist. Proponents of any claim can formulate their standards to prefer their claims over others. We need a criterion to choose the right criterion among many. But we face a dilemma. We argue in a vicious circle if we fall back on our knowledge claims or criteria to argue for its criterion. We face infinite regress if we need to find new criteria to choose each criterion we adopt. We must do one or the other to acquire a criterion of truth[1]. This way of putting it is beneficial. Recognizing that we have no criterion for truth has a benefit: we notice that we do have a criterion for falsehood.

A logical contradiction among statements is a criterion for false opinion. Critical rationalism can use logical contradictions to weed out falsehoods. The trick is to find a clever new way of using that criterion of falsehood to yield a derived criterion for truth.

Francis Bacon solved the difficulty of the Socratic *elenchus*, which he reformulated to apply to natural philosophy. Baconian induction is an *elenchus* that proceeds slightly differently: you begin small - studying the nature of a limited kind of thing. Like a skeptic, you undermine earlier hypotheses about the thing and discard them from consideration. Then you take your skepticism just a bit further.

You undermine the facts themselves - what you think you know about the nature of the thing under study. You create *experimental* difficulties for the appearances (*observations*) of the nature of the item[2]. Problematizing evidence is the main task of the new experimental method. You find experiments that undermine ascertained observations and other evidence of its nature. Then you

[1] The Pyrrhonists of course do not assent either that there is or there is not a standard for truth (Empiricus, 2000 Bk. II.) The criterion of knowledge was widely discussed in France when Francis Bacon was in Paris as a young cryoptographer employed by the diplomat, Amyas Paulet who represented Elizabeth I. It has seemed to me a natural way for Francis Bacon to alight upon his *elenchus*, which he seemed to have grasped in outline before his return to England. However, I have not found evidence of this argument pattern in any of his writings.

[2] In 1998, Brian Baigirie, returning from a research trip to study Isaac Newton's papers in Cambridge, said that he saw so many bins of papers entitled "Observations and Expriments" that he wondered why they mentioned both together so often in that period. I was at the time wondering why Albert Einstein kept deferring to Niels Bohr's interpretation of quantum mechanics as the best account of the *experiments*. This did not make sense, for different interpretations of the same equations should be regarded as empirically equivalent, what was it about experiments, I wondered, that is missing in observation? Brian, thus, got me started on a renewed study of Isaac Newton, Robert Boyle, Francis Bacon, and Plato's Socrates, as I went backwards in time.

write up your evidence and its difficulties – it is now all evidence – in a daily record or a journal. It would be best to record all the facts that have been ascertained and problematized. Robert Boyle updated the Baconian method and called his version the "experimental philosophy."

The important Baconian proviso is this: your experiments should be designed to yield results contrary to your current evidence and (apparent) knowledge. The experimental results must be surprising to be notable. You jot down these evidential discrepancies in a journal just as they are found - that is to say, without trying to resolve, clarify, or improve each discrepancy right away. When an account is written up in this manner (Bacon, 2000/1620), he calls it an "experimental and natural history." It is how a thing's nature appears to you after cross-examination by experiment. Experimentally interrogating existing evidence is like Socratic questioning. The learned journal is a record, like the notes taken by a court clerk of the deposition, oral testimony, and cross-examination of witnesses.

Francis Bacon's natural history was of a peculiar kind. Collecting *errors* seems strange for a philosopher interested in some *truth* about what is *real*. Those italicized words seem not to belong together. But the histories of our assembled errors confer us a benefit. The assembled problems create a complex puzzle about reality. However, an experimental history can do this only after you have collected enough problematic evidence. Only after the puzzle has become almost unmanageably complex should you begin to ask yourself: "what can be the case about the thing in focus that appears in all these puzzling ways exactly as it does?"

If you are lucky, you can then construct a schematism or a model in which the errors are dissipated. Your model must answer your question, "what model of the thing studied fits all the discrepancies of this experimental and natural history?" Any answer is a bit of metaphysics - a small part of a future account of reality. Asking the question thus is much more restrictive than some older metaphysical questions like, "What is reality, ultimately? Is it one or many? Is it mathematical or qualitative?" and so on. Instead, you try to solve a puzzle that is modest in scope. Even with that modesty, it isn't easy to recognize and solve an assembled puzzle in modern science. When you succeed, other scientists honor you as outstanding among peers. It takes dedication, imagination, skill, and a hefty dose of luck.

The puzzle you set yourself does not all have to be your work. If you are lucky, others may have prepared your experimental and natural history for you. Modern science has become more communal, where one group can sometimes produce all the experimental examination of a thing. In contrast, another group exclusively concentrates on theory, given the state of experiments. Of course, such dual groups may have some common members, but they need not have any. Baconian science is unlike the older philosophies of past millennia when each philosopher contemplated reality in solitude, like Auguste Rodin's *Thinker*.

A fully developed puzzle, whoever produces it, can barely be solved. If it is easily solved, particularly if it is easily solved in many ways, then we have not been diligent enough in assembling the experimental and natural history. The task of theorizing is premature at this stage. Your solution will be difficult to find if you have enough problematized evidence. Its difficulty is tied to its uniqueness. No other model of the thing under study will fit the quirky features of its natural history. Remember, you have cross-examined nature skeptically. When your model is coherent and unique, you have knowledge that can withstand intense skeptical scrutiny. That is why it is reasonable to call it "skeptical knowledge." Such knowledge is stable when a unique solution is found because it withstands all current skeptical scrutiny.

Skepticism about facts generates the strange errors reported in an experimental and natural history, using which we can discover the underlying nature of the thing you and I were seeking. Your new model is a true depiction of the underlying reality because A) it is coherent and B) it is a unique solution to a skeptically produced puzzle. Here is the criterion we sought. It is a modern version of the Socratic *elenchus*, which solves Plato's problem with its uniqueness, which he addressed but did not solve (Vlastos, 1994) in the *Gorgias.*

A uniquely coherent system of belief constitutes knowledge. Its criterion is the criterion of falsity, namely internal incoherence, applied in a new but skeptical way to generate truth.

The task of skeptical science is to get beyond appearances to a deeper level of reality, to the underlying or latent cause of things. Such a skeptical technique transports you beyond or behind appearances precisely because you have problematized the phenomena. Appearances cannot be left in their problematized state when we solve the puzzle: they must be made coherent by minor revisions. They must be normalized or modified to become coherent while remaining factual. You must go beyond the evidence as formerly understood to normalize it. There is no other way to do this than to break through the appearances to a deeper reality.

When you find a cause of whatever you are investigating, that cause will constitute a breakthrough. Francis Bacon (Bacon, 2000/1620) provided the theory of how to break through to the causes of things in his *New Organon*. But he did not apply his skeptical method to produce any breakthrough knowledge or science that his *elenchus* promised. Isaac Newton displayed the craft of skeptical ability in all its facets. He also made some contributions of his own to the craft. For that reason, it is appropriately called the "Newtonian Method" or "The Scientific Method."

II

Soon after its founding in 1662, the Royal Society of London began encouraging and recording skeptical or experimental natural histories. Robert Boyle updated Francis Bacon's recommendations and undertook its exposition and application. There were many experimental philosophers among the early members. Robert Boyle was particularly prominent as a supporter and expositor of the modified Baconian method. He provided funds for constructing expensive instruments and recorded and published detailed experimental histories in different parts of natural philosophy. The two most remarkable applications of the experimental method, which led to breakthroughs, were proposed by Isaac Newton a bit later. Before his contributions, many intriguing experimental natural histories were produced, but they seemed to lack direction. He first applied the new method of the "interpretation of nature" to the accumulated puzzle of optical phenomena in 1672 and fourteen years later to those of astronomy (though the latter claim, which he made, is rarely supported. and needs showing.)

I have briefly sketched Isaac Newton's theory of light and colors of 1672 in a chapter of an unpublished book manuscript, but even that brief sketch is too long to repeat here. If I may be allowed to introduce a sketch of that sketch, I can show how he "interpreted nature" (Bacon, 2000/1620, Aphorism XXVI)) to solve a puzzle about light. It is helpful for a critical rationalist to see an *elenchus* in operation if I may take myself as an example of someone who benefited from this help.

In his letter to Henry Oldenburg (Newton, 1672), he reported that light passing through a prism projected an image with an impossible appearance. He arranged for sunlight to pass through a small circular hole in a screen that blocked all other light to his room. The image of light from a circular hole should also be circular when the prism and screen are appropriately arranged. But the image appeared oblong on the screen though the experiment was carefully performed. That impossible elongated image is what he investigated and reported in his letter about colors. I emphasize that he was not, to begin with, studying colors. He was examining an error in our apparent knowledge. The theory of colors is his breakthrough that solves the puzzle about colors in which he found recorded many other discrepancies.

An oblong image from a circular source of light contradicts the laws of geometry when combined with the sine law of refraction, the rectilinear propagation of light (and perhaps a few other assumptions.) What causes the anomaly?

He considered the possibility that sunlight is not uniform. Light from different parts of the sun could vary, causing a distorted image. By rotating the prism around its long axis slightly and slowly and comparing the images produced at each slight turn, he ascertained that the sunlight was uniform – there was no change in the form of the image. He considered the possibility that the prism was flawed, causing the distortion. But when he took a second identical prism and placed it against the first, but upside down, he found that the image produced was a circular beam of sunlight. No flaw in the prismatic lens was distorting the image. He considered many hypotheses and ingeniously showed that none of them could explain the discrepancy. For instance, could the light travel in a curve after leaving the prism? By varying the screen's distance intercepting light out of the prism, he found by tracking the size and shape of the image that the light beam is traveling straight.

He measured the length and breadth of the image on the screen as accurately as he could. He calculated that the sine law of refraction predicted the image's width. But the height of the image was several times too large. Why was there an elongation in that one dimension? Moreover, the height was peculiar because its top and bottom were semicircular and not distorted by elongation. The elongation also displayed the colors of the rainbow. Isaac Newton began to suspect that perhaps colors may be implicated here.

This thought brought him good results. When he held up a screen with a hole isolating a color in the image in the light beam's path, he found each pure, color had a circular image. Moreover, a prism in the path of the color did not elongate its image. Each color was refracted at the prism as predicted by the sine law. A circular hole projected a xircular image. But each color refracted at a slightly different angle. The color images were superimposed partially on one another in the elongated image to produce its strange shape.

Until Isaac Newton's letter was published, theorists supposed that color was a modification of white sunlight by media. Sunlight was new and pure light, a general assumption in optics. René Descartes had proposed a model with this tacit but commonly made assumption. He had compared sunlight to a rubber ball, which would acquire a twist when it hits a hard surface like the ground. The resistance offered to one of the ball's surfaces would cause its twist: the surface impeded by the ground would slow down, while all other surfaces would move freely forward. The ball would acquire top spin. When the retina feels the twist (i.e., felt by the optic nerve attached to its rear), it registers as a color. He used this model of color, the sine law of refraction, and the measured refractive indices of air and water, to give a very detailed and impressive theory of the rainbow. It is an ingenious account with a magnificent explanation of the rainbow we see in the sky. Isaac Newton admired Cartesian optics, which he had studied with Isaac Barrow with great interest.

Isaac Newton knew in 1672 that colored light is not modified white light. His discovery was, in this sense, a breakthrough. White light is a mixture of colors, simple as sunlight appears to us everywhere, while colors are original (or intrinsic) properties of light. Note the breakthrough he made about the complex reality beyond the appearance of simple sunlight. It appears white, but it is a mixture of colors. The prismatic glass lens does not manufacture colors like a foundry; it merely sorts and dispatches each color to its proper address, like a post office. The discovery of a hidden cause is a characteristic feature of the Baconian method of true induction. It is designed to find the "true" (faithfully represented) measure of reality rather than imposing a measure of our mind (a hypothesis) upon reality.

Light of different colors refracts at slightly different angles, creating the strange image he was studying. As a Baconian, Isaac Newton knew he had to show how this knowledge gave him "power over nature," in Francis Bacon's words (2000/1620), which is a "pledge of its truth." Isaac Newton painstakingly measured the relative proportions of the different colors in the spectrum. He constructed a circular disc with that same proportion of colors arranged radially on it. Pure white light emerged by spinning the multicolored disc on a wheel[1]. He could show experimentally that the great René Descartes was wrong - color is an original property of light, white light is a mixture of the original colors, and colors vary in their refrangibility.

I have summarized this bit of Newtoniana, perhaps none too well, to make a particular point. If you look carefully at my account of his method, however poor it may be, it is very much the way Karl Popper described science. Isaac Newton took up a problem, namely, the impossible oblong image. Then he entertained one hypothesis after another to solve the problem until he found one that did. Each hypothesis is refuted by an ascertained fact leading Isaac Newton to seek another

[1] In the interest of brevity, I have neglected this important feature of Baconian method. It should be noted that Isaac Newton had produced and donated to the Royal Society of London a reflecting telescope. His explanation of colors.

hypothesis. Is this not the method of conjecture and refutation? Isaac Newton's ingenuity shines through in the hypotheses he considers and, even more, in the ingenuity of his tests. For all that, his method seems to be just an application of the method of hypothesis. When he solves the main problem, he does so (eventually) with a bold conjecture. Is this not critical rationalism at its best, and exactly how it applies to empirical science?

Indeed, it is. Isaac Newton's experimental method is an excellent example of critical rationalism, of that special critical rationalism that Karl Popper called "Science: Conjectures and Refutations." (Popper, 1961 Ch. 1) But it is also something else. It is an *elenchus*: it provides an affirmative theory of colors by relying on a process of refutation.

When a hypothesis is refuted, it is not enough to honestly accept its failure, though, of course, Karl Popper was right – you must not explain it away, for then you may have only your prejudice left to admire. When we see the impossibly oblong image, we can conclude that the sine law of refraction is refuted. Or that geometry fails us. Or that light does not always travel in a straight line. Or that the instrument used was faulty. And so on. Given the postulates of geometry, the sine law of refraction, the rectilinear motion of light, and other similar assumptions, the image should have been circular. And it is not. Ergo, we stand refuted. We must replace an assumption, but we do not know which. We need bold conjectures.

We note that Isaac Newton's discovery that light is a mixture of colors does a lot. It dissolves the paradox of the impossible image. It also explains all the peculiar facts he recorded about how the image is formed. It also explains a whole host of other discrepancies about light that were reported earlier by Robert Hooke and Robert Boyle. Each of these problems is an apparent impossibility, ripe for resolution. Isaac Newton's letter on colors shows how all those strange phenomena can now be explained and normalized at once.

Take, for instance, Robert Hooke's two glass flasks of colored water. If light from a single aperture in a screen enters a room and we let it pass through a glass flask filled with an intense red liquid, the room will be infused with red light. If we use a flask filled with a blue liquid, we will see it infused with blue. But what color do we get if we put them in tandem to filter the light together? We expect the red light to be modified by a blue tinge to yield a purple color. But that is not what happens. The two flasks in tandem become opaque. Light ceases to enter a room with red and blue filtering.

If we take Robert Hooke's experiment by itself, we may be tempted to think that the colors blue and red are special to understanding light. What might their hidden connection be? Once we take all the problematic phenomena together, we see how Isaac Newton's principle of mixtures fits all the troubling facts, including this. In the case of Robert Hooke's flasks, a red liquid transmits red rays of light but filters out (reflects) all other colored rays intermixed in sunlight, including the blue. The blue liquid, however, filters out all other light rays except the blue. Hence, the red and blue filters in tandem block out all light. Isaac Newton's application of the *elenchus* resolved several such anomalies of colors. Not only could the model of white light explain strange color phenomena that Robert Hooke and Robert Boyle listed, but also his own additions to the experimental history. His model of white light could solve the puzzle of the anomalous color phenomena on record.

When we examine the experiments reported by Isaac Newton in his 1672 letter on color, we can certainly read them as a series of conjectures and refutations, which they are. But they are also his additional entries into light's experimental and natural history. In one way, this account illustrates that Isaac Newton would have been a critical rationalist - a philosopher who refuted his own serial conjectures with glee. But if we are too flushed with the pleasure of finding a fellow critical rationalist in Isaac Newton, we miss the point. He was secondly also an experimental philosopher. He was applying Francis Bacon's skeptical method of induction.

Three hundred and fifty years after his letter to the Royal Society of London, the Newtonian claim that white light is a mixture of colors is still accepted as knowledge. Much has changed, but this discovery remains. Why is it stable knowledge? I suggest that the answer is not found in critical rationalism. Hypotheses need not be stable knowledge. It is stable for a different reason: it is uniquely coherent. It is the only account that works of some particularly problematized facts.

Karl Popper missed something in his methodological remarks: we should not try to solve problems serially. We need to exercise patience. It is too easy to stray from reality if we address problems one by one. A single refutation is easy to evade. We can defend any theory from recalcitrant experience, come what may[1]. Therefore, the best method is not to proceed impatiently with conjectures and refutations. Instead, we collect refutations patiently in some small domain until they form an insoluble puzzle. Then we solve all the problems together, as one puzzle. No one is tempted to return to the assembled chaos of earlier problems. When skeptically assembled anomalies are solved at once, they stay solved. Opponents find it hard to go back even when they find that their pet theories have been abandoned.

Isaac Newton's induction was not prescient. He got some things wrong about white light, too. He had asserted that white light is a mixture of the colors of the rainbow in the same proportion as in sunlight. His demonstration of spinning the many-colored wheel had produced pure white light. Moreover, if one color on the wheel were blocked, then rotating it did not produce pure white light. So white light is a mixture of the colors of the rainbow in their natural proportion, which he had so carefully measured. But he was mistaken about that. Christiaan Huygens soon showed him that white light can also be generated by mixing only a few of the colors, provided they are correctly balanced. Isaac Newton had made a subtle error. He had assumed that because the colors of the rainbow produced white light when mixed in due proportion and not when that ratio is varied, that white light is essentially that proportion by mixture.

Nor is the experimental method itself infallible. His model of gravity, proposed a decade later, also solved a large set of problems, though the problems were developed by disputes and

[1] I am referring here to Willard Van Quine, who claimed, "Any statement can be held true, come what may, if we are willing to make drastic enough adjustments elsewhere in the system." (Quine, 1952). I am particularly pleased that I have found out why science does not bear out his highly regarded claim.

disagreements and not by any Baconian intent. But that celebrated solved puzzle was later corrected by general relativity.

III

Isaac Newton did not follow the experimental method thoughtlessly. He was a skillful intellectual craftsman who extended the method of experimental philosophy beyond its earlier formulation. In the Preface to the first edition of his *Mathematical Principles of Natural Philosophy* (the *Principia, 1934/1686)*, he reported his method in that writing as consisting of two steps. "…[B]y the propositions mathematically demonstrated in the former Books, in the third I derive from the celestial phenomena the forces of gravity with which bodies tend to the sun and the several planets. Then from these forces, by other forces which are also mathematical, I deduce the motion of the planets, the comets, the moon and the sea." In other places, he called these two steps a special induction and a general induction. The division applies to his letter on colors, too. I would redescribe the two steps as *model building* and *Baconian induction*.

It has not often been noted that the *Principia* is a biographic report of his astrophysical research. In many ways, it resembles his biographic report of his optical research in 1762, though it also differs in three respects, as I note below. His astrophysics is a much more formidable mathematical exercise. Commentators who expect his insights were only mathematical have been puzzled when he reminds us in the second edition of the *Principia* that his books are a species of experimental philosophy.

His model building based on a mathematical discrepancy is the first biographic step in each report. He builds a model of simple light as a hidden mixture in one case. In the other case, in Book III of the *Principia*, he reveals a hidden force of gravit. Some of Isaac Newton's remarks about his method are appropriate as they relate to his model, but not to what he calls "general induction," – that is to say, not to his application of the Baconian method. In the General Scholium added to the second edition he says that he "deduced" gravity from the phenomena. And he did. But he did not deduce gravity from the phenomena alone! He used the mathematical model's resources, including the laws of motion, to deduce gravity from the phenomena. There is nothing logically odd about that deduction.

His optics and astronomy are similar exercises in model building, though with three differences. First, there is this obvious difference. The first book of the *Principia* is a remarkable feat of mathematics, whereas the letter on colors is an example of a careful skeptical investigation of phenomena. It has tempted some to suppose that his secret of success in the *Principia* was his mathematics. But he also solved an intractable puzzle of astronomy in Book III, so it was not all in the mathematics.

Another difference is that his optical report begins with the central anomaly, the impossibly elongated image. The *Principia* is different because the anomaly comes last in his report: an impossible path for each of two comets. He seriously investigated the System of the World only after encountering the strange comets of 1680 and 1682. The comets exhibited impossible orbital paths in a Cartesian plenum. In a plenum, every motion must be in a closed curve. But if we trace the vortices that carry the planets, the comets were on impossible paths going across and against the established vortices. He was led to a model in which they move in empty space, propelled not by vortices but by a strange new force – an action at a distance.

In the *Principia*, however, he described the bizarre orbits of the comets only at the very end of his report in Book III. Although the order of presentation was reversed, his reports were similar in other respects. His letter of 1672 was a theory of an impossible image, which also unexpectedly solved the puzzle of the colors. His *Principia* is a theory of comets with impossible orbits. There was no theory of comets to speak of then. His new cometary model unexpectedly solved the great puzzle of 17th-century astronomy concerning all the planets, the sun, the sea, and the moon. For that last reason, we could allow him to call his *Principia* an exercise in experimental philosophy.

A third difference was his role in making up the puzzle. In his optics, he contributed some additional problems to the puzzle. The *Principia* was different. Comets apart, he found a complete puzzle ready-made. That great puzzle of astronomy, which he solved, was found in the contentious disputes about astronomy between the scholastics, and Tychonians, Keplerians, Galileans, Cartesians, Gassendians, Horrockians, and others. By questioning each other, they had unwittingly created the kind of puzzle that Francis Bacon had recommended. But the disputants had not done so knowingly by adopting a Baconian methodology. Skeptical knowledge may well predate Francis Bacon's recommendations. Skeptical inquiry into phenomena and puzzle-solving could have happened in the natural course of thought before the Royal Society of London adopted the experimental philosophy. The Royal Society therefore only accelerated the growth of knowledge and did not invent it.

Isaac Newton did make an outstanding contribution to methodology. He showed by example how to build a complex model of a bit of reality in experimental philosophy. No earlier Baconian had paid attention to this feature of the craft of a natural philosopher. Isaac Newton worked on his models' details very carefully and methodically. He paid attention to detailed facts, wallowing in the particulars where the discrepancies are often found. He reported them to the public only after his surprising model of the puzzling phenomenon solved many other problems of the experimental history. I suspect all the great breakthroughs in subsequent natural science have exhibited this feature of Newtonian craft. Emulating the great Isaac Newton, every breakthrough in later modern science has begun with a careful and systematic examination of details of experimental phenomena to yield a surprising model. Then the model conquers all or, usually, almost all.

IV

A difficulty in my account of science will occur to you straight away. I have claimed that a carefully built model and an elenchus were used to gain knowledge – to produce science – repeatedly for three hundred and fifty years. Moreover, I have argued (1) that Francis Bacon stated the method. (2) It was understood early in the Royal Society of London. (3) In particular, Robert Boyle understood it well enough to reform it. (4) He also illustrated it copiously with Baconian experimental and natural histories. (5) Isaac Newton inspired later philosophers with this new method. (6) I have also claimed that he used the new experimental philosophy to develop his theory of light and his theory of universal gravitation, which were widely admired and intensively studied.

Why does nobody detect or describe this method today? Why does no one see it in broad daylight?

When we try to find a star during daylight, we cannot see it. The bright sun drowns out a faint star in the sky. The Newtonian method was drowned out by the bright light of a sun that arose almost simultaneously – namely, John Locke's (1975/1688) essay on the limits of human understanding, in which knowledge claimed about a religious faith was off limits. Compared to that sun, the starlight of a poorly described method was drowned. That is my reading of events[1]. Newtonian science has been much appreciated and emulated all over the world. But his experimental method is invisible and all but forgotten. We are so dazzled by the doctrine of sensory empiricism that we have assumed that the method used by scientists must satisfy empiricist criteria. Skeptical examinations of empirical facts cannot be stated coherently within empiricist limits.

Empiricism was the critical doctrine underpinning John Locke's recipe for liberal democracy in England. He proposed that secular knowledge, founded securely in sense experience and reason, can be used by a magistrate in dispensing justice for all. Differences in our religious faith, however, must be respected. They are most important to a faithful person, but they cannot be known to be true or false in secular science. Hence liberal democracy should not outlaw any genuine faith. A democracy that allowed freedom of religion, such as the Glorious Revolution of 1688, brought a measure of doctrinal peace between rival faiths. In retrospect, we may see flaws in that revolution, for it did not allow Roman Catholics or atheists to hold public office. But it was still a beacon of hope for peace in beleaguered Europe. Later versions inspired by John Locke's arguments had a benign effect wherever similar liberalism was adopted, whatever their defects. But the promise of peace was a great lure, though it was had at a cost. We had to accept a theory of knowledge that claimed that sensory inputs (i.e., any observations) and rational thought (i.e., logic and mathematics, including probability) generated secular and universal knowledge, such as science. Its universal acknowledgment as science demarcated it from a sectarian faith, which cannot be derived from sensory experience and reason.

No foundational sense experience can be riddled with skeptically generated paradoxes. Paradoxes are at the heart of the Baconian method. Consequently, it could not be stated coherently within empiricism. Methodology, however, is a small cost to pay for doctrinal peace. John Locke's empiricism replaced Francis Bacon's skeptical method. However, flawed the Liberal sun may have been, it shone so brightly that the Baconian star was invisible. Being an early empiricist and

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[1] *The Pax Empirica, and the Hidden Method*, is a second unpublished book of mine, describing mostly the *Principia* as an exercise of an affirmative *elenchus*, or experimental philosophy. I also touch upon John Locke's doctrinal peace based on the limits of understanding, which co-opted Newtonian method within empiricism, making it impossible to state coherently.

securing the *Pax Empirica* became a great honor to confer upon any modern philosopher. As a consolation prize, we have conferred honorary empiricism upon a reluctant Francis Bacon[1].

David Hume (1977/1748) showed, on empiricist grounds, that we cannot know any hidden causes of events precisely because they are hidden and not given in sense experience. All that we can observe are constant conjunctions of sensible events. Hidden causes cannot be derived from them logically because one would not contradict oneself by denying them. Moreover, we can have no predictive science or inductive knowledge by reasoning only from sense impressions using logic. Our record of experience is of the past. We cannot deduce from the past that the future will be like the past. Empiricism supports a different kind of skepticism that David Hume advocated – a doubt concerning reasons.

The obvious response to David Hume's arguments is to abandon John Locke's empiricism. It is not a necessary condition of knowledge. We should not be tempted to conclude on empiricist grounds that we have made no scientific breakthroughs, or that skepticism is alien to collecting evidence. There is too much evidence to the contrary. But few have so decided and gone on to study the Baconian *elenchus*.

The long hegemony of empiricist thought has influenced the scientific method. A form of foundational and empiricist thinking is also valuable for modern science. I need to fill in a bit of history to describe that benign influence. Skeptical knowledge flourished in nineteenth-century physics and chemistry, though clandestinely hidden among the many useful and influential hypotheses of significance in that period. Many overlapping and applicable new scientific models of ordinary things around us also arose. Each model would become increasingly better as time went on by recalibration from within - by better measurement, improved instrumentation, and better mutual support of internal theories. The models could also help calibrate each other wherever there was overlap. An extraordinary density of knowledge came to exist. The increasing density of knowledge was widely noticed because the models were also valuable in their practical application to ordinary things around us. Nature became more habitable. The world was becoming more artificial by the decade. A similar density of knowledge was achieved in the medical sciences later in the nineteenth century and decades after. Life expectancy improved.

Applying new knowledge suitably and safely in engineering or medicine became important. To avoid regret, we examine our foundations and cleanse them of error, as best we can, using a quasi-empiricist method. We cannot, of course, ask everything to be reduced to sense impressions. That is not feasible. Suppose we find an acceptable level of social trust in some form of observed data. We can then refine such quasi-empirical data to minimize error and use it as a basis to examine (quasi-empirically) the suitability and safety of applying new science. We have here the reverse of

[1] Francis Bacon (2000/1620) explicitly criticizes empiricism as inadequate in many different aphorisms, such as Book I, LXIV and XCV, but he is not consistently anti-empiricist. In Aphorism I Book I he seems to suggest that when he wrote it, he had a generally empiricist perspective, much like Aristotle. "Man is Nature's agent and interpreter; he does and understands only as much as he has observed of the order of nature in fact or by inference; he does not know and cannot do more." I suspect that early on he began as an empiricist, following Aristotle, and as he developed his thought as an experimental philsopher, he found he had to abandon many early assumptions, empiricism being one.

the *elenchus*, where we amplified errors to discover puzzles. One example will show how the two methods compare – the skeptical and the empiricist.

In the recent Covid-19 pandemic, it was determined that the infection was a virus. A couple of weeks later, it was announced that it was an RNA virus with characteristic proteins. It was fully decoded. A vaccine was very soon developed because new skeptical knowledge can give us new power over nature. New M-RNA vaccines could not be given to healthy people without checking whether the vaccine would be effective or had unacceptably harmful side effects. Consequently, we had to wait for trials on large numbers of volunteers before the vaccines were available.

The virus's decoding and the vaccine's safety and efficacy employ different methods at work. Decoding the virus used a well-developed model of heredity. An RNA vaccine was formulated using a recent enhancement of the model, which combined many earlier breakthroughs. Elements of the model were learned skeptically – they could not have been learned empirically, as David Hume showed us: Viruses are hidden causes of infection.

But evaluating the efficacy and safety of A vaccine is a different matter. We ask: How many volunteers were tested at its trials? How varied was their demographic composition? How carefully were the tests conducted? And so on. These are all quasi-empiricist criteria in which we seek to avoid errors. We need secure knowledge of the efficacy and safety of the vaccine. We would not ask such questions about the causal analysis of the virus. (E.g., "how many thousand viruses have they studied since it was discovered, and how variable are they?" is not a relevant question.) Each method is useful to its task. We err if we demand that skeptical knowledge meet quasi-empiricist criteria, which it cannot. Nor should we deny any knowledge based on probability and a reasonable quasi-empiricist basis (e.g., human-engendered climate change) because it was not skeptically produced.

Some more difficulties for my claims arise from their underappreciation, but one is genuine. Empiricists, adopting a hypothetico-deductive interpretation of modern science, expect to find rules of method that determine theory choice. But such an expectation does not carry over to the Baconian way. If you collect enough problems on a small enough topic, you no longer need to adopt rules to choose a theory. Problems, being self-contradictory, demand solutions as they are, as opposed to empirical facts that demand nothing of us. Problems by themselves may provide a *methodology without adopting methodological rules*[1]. My phrase may have fallen by the wayside. Marco Giuinti (1988) was alone among my critics to appreciate the value of a fully problem-based methodology if developed. However, he pointed out correctly that my attempts were incomplete and flawed[2]. Francis Bacon's method escapes his criticism. While a particular problem may allow multiple solutions, each being adequate to its task, numerous problems taken together could refute one anothers' solution until only a single solution remains free of paradox. In a Baconian puzzle,

[1] In my (1983) "A Methodology without Methodological Rules," I did not appreciate the importance of waiting and not rushing to judgment. This allows me to claim for Francis Bacon's interpretations of nature unique coherence and therefore knowledge.

[2] Marco Giunti showed that my theory was incomplete, which I accept, but I did not see how to complete it until I began to understand Francis Bacon's experimental and natural history.

methodological rules need not be adopted but arise out of the puzzle. Let us look at a test case. How can a nest of problems obviate the need for adopting a methodological rule against *ad hoc* hypotheses?

When Isaac Newton died, he knew that he had solved the Baconian puzzle of post-Copernican astronomy, but for one blemish, to which we will soon turn. Every complete model of the system of the world before the *Principia* was challenged on all sides. Isaac Newton was able to solve the almost intractable puzzle created by disputes and disagreements among astronomers (e.g., Nicholaus Copernicus, Tycho Brahe, Johannes Kepler, Jeremiah Horrocks, Christopher Wren), and the mechanists (e.g., Galileo Galilei, Evangelista Torricelli, René Descartes, Pierre Gassendi, Christiaan Huygens.) None of them had taken up Francis Bacon's philosophy to follow, of course. Still, together they had inadvertently created the paradoxes that the experimental philosophy demands of us. Isaac Newton brought forth coherence with a complete solution. In doing so, he responded to an "experimental" or disputed reading of facts and analyses, and in this sense, his solution to the puzzle was a conclusion in experimental philosophy[1].

Isaac Newton had solved one by one more than half a dozen inequalities in the moon's orbit, or inexplicable but recurring deviations. He had also solved one by one many other inequalities: the precession of the equinoxes, the planetary orbits of Jupiter, the planetary orbits around the sun, the unruly tides of the sea, the strange shape of the earth, and the vagaries of pendulums at different latitudes on the earth. The coherence that smoothed out all the known paradoxes came from a single model developed to describe comets. No astronomical model before had enjoyed such success. But one inequality of the moon remained unsolved: The moon's orbit was retarded by a tiny bit each month, which retardation, in about eighteen years, would cycle back to a given orbit. Isaac Newton could explain some retardation and even the shape of the orbital inequality. But when calculated from gravity, the moon should complete the cycle in about nine years, not the observed eighteen. Isaac Newton struggled with this numeric defect in his model during the latter part of his life.

The inequality was solved twenty years after his death by combining two changes in his model. Daniel Bernouilli published a better Newtonian theory of tides based on superior tidal estimates, in which he revised the center of gravity of the earth-moon system. This change accounted for more than half the lunar inequality that had beset the Newtonian model. Alexis Clairaut, a brilliant young mathematician who understood the Newtonian model well, accounted for the rest of the shortfall by showing that Isaac Newton had made a mathematical error. He calculated the inequality by considering each of the three dimensions at a time and adding their effect. But he had not calculated the effect along one of the dimensions, judging it negligible. It was not.

Our interest is in what happened before the problem was solved. Alexis Clairaut first published a result showing that the inequality of the moon can be resolved if we modify the law of gravity. He published a modification of the force formula, which is inversely proportional to the square of

[1] In the General Scholium found in the second edition of the *Principia*, Isaac Newton uses the phrase to describe his effort, but most commentary on his method regards this claim as merely polemic excess on his part in response to critiques of his model. I interpret his work as an example of experimental philosophy in my *Pax Emprica*, op.cit. fn. viii.

the distance between two bodies, by adding to the inverse the fourth power of the distance. The revised force law explained the known retardation of the moon's orbit quite well. He did better than Isaac Newton on that inequality. George-Louis LeClerc, or Comte de Buffon (better known as a naturalist and forerunner of theories of the evolution of life), promptly criticized the amended law as inadequate. He pointed out that gravity was used in multiple explanations by Isaac Newton, and we should not change the gravitational formula for just this one case. In effect, he enunciated the principle of avoiding *ad hoc* explanations, which may be called *Buffon's rule for methodology*. Given an option of Buffon's rule and another of the rule named "Inference to the Best Explanation," which rule would you choose? Whichever you do, you discern the skeptical problem of the criterion lurking in the background. Fortunately, Alexis Clairault solved the issue of the criterion by avoiding it.

Isaac Newton showed that the moon's acceleration toward the earth, calculated from astronomical measurement, was almost exactly equal to the acceleration of a body falling near the earth, as measured by a second's pendulum. Gravitational force is proportionate to acceleration during fall. If we revise the gravitational force on the moon, as Alexis Clairaut had proposed, it would be incompatible with the known accelerations of bodies falling near the earth. We would have a new discrepancy. So, the revision he had suggested was not the solution he sought. But it failed because the puzzle remained unsolved – a paradox remained - and not because he adopted Buffon's rule. Alexis Clairaut decided that he had to go back to the drawing board to find out why Isaac Newton's force did not produce the desired result and found, to everyone's surprise, it did. But that was luck, you may say.

An apparently unique solution may not be really unique, after all. A series of *ad hoc* explanations of phenomena combined into an unrefuted *ad hoc* hypothesis may challenge a presumed uniquely coherent model. It is possible, but this does not save Buffon's rule. All we need is this: we continue experimenting until we find an *experimentum crucis* (Isaac Newton) or an *Instance of the Finger Post* (Francis Bacon) to rediscover uniqueness. The argument is simple. In uniqueness, we trust - for adopting a rule is skeptically challenged.

We become foundationalists if we demand infallible proof of uniqueness. We can assure ourselves that if unique, our model solution must be true, in which case we have some knowledge. But we cannot go beyond that to a certainty of that knowledge. We cannot know that we know unless we can prove that the unique solution contains no hidden error and that no other coherent solution is possible. If we can do that, we can have well-founded knowledge, which would be very, very nice to have until we wake up. Indeed, the evidence from the growth of science suggests that as more discrepancies are found, we may end up with a bigger puzzle – a bigger mess - which calls for a new and different solution. And when Albert Einstein eventually found general relativity, then we found, in turn, that Isaac Newton's brilliant solution to the puzzle that Copernicus generated was not unique. Is that not what happened? But we now have a new solution that is also unique, apparently, in a much larger experimental context.

A difference remains between the method of hypothesis, championed by Plato and Popper, and the method of an affirmative Socratic *elenchus*. When we abandon a hypothesis, we must begin our search for a better hypothesis anew. There is no residue of the old hypothesis. When we abandon a unique model solving an intractable puzzle, we can leave the model but not the puzzle. It remains to haunt us. To solve a larger puzzle later, we must also solve the little old knot in its past. The continuity of problems was a motivating factor for my thinking along these lines, but it seems that I got it, too, as a hint from Karl Popper. He was keenly aware of this unifying aspect of science. He analyzed it differently, using Tarskian formal semantics when he wrote about how scientific growth could yield more verisimilitude. Logic now seems unsuitable for his task. I hope that the development of problems captures what he sought in the unifying growth of science. But all this concerns only fallibilism in the specialized area of scientific research. What about our knowledge at large?

A serious difficulty concerns fallibilism itself[1]. Any scientific theory is fallible. But for similar reasons, so is any non-scientific theory or understanding. Then a question arises in the general case: what is the difference between fallible knowledge and a fallible opinion? There is a difference, it seems. To deny it is to make knowledge necessarily infallible, which is an unreasonable conclusion to reach. But I have nothing satisfactorily worked out on characterizing that epistemological difference in a general way.

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[1] I am grateful to Anandi Hattiangadi for raising this important question that I must address.

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