BACONIAN METHOD AND NEWTONIAN SCIENCE

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IT is reasonable to raise the question of the influence, or lack thereof, which the thought of Francis Bacon (1561-1626) had upon that of Isaac Newton (1642-1727). Opinion on this question has traditionally been strongly divided, with some authorities, such as De Morgan and Randall, maintaining that Bacon's thinking had no influence whatever on Newton's activity,[1] and others, such as Maclaurin and Fulton, maintaining that Bacon's thinking provided the very methodology which made Newton's activity possible;[2] and whenever opinion lines up in this fashion, there are *prima facie* grounds for believing that both parties to the dispute are committed to half-truths. Moreover, the question is of philosophic and scientific as well as historic interest, for it not only concerns the relation between two major figures of intellectual history, but it also raises a concrete object-lesson in the relation between the sorts of things that philosophers are wont to say and the sorts of things that great scientists actually do.

In this essay, I shall argue that although there is little reason to suppose that Newton was influenced by Bacon directly (*i.e.*, through a firsthand study of him), certain passages as well as

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1 Florian Cajori, "The Baconian Method of Scientific Research," *Scientific Monthly*, xx (1925), p. 86, quoting De Morgan: "If Newton had taken Bacon for his master, not he, but somebody else, would have been Newton." John Herman Randall Jr., *The Career Philosophy* (New York, 1962), vol. I, p. 241: "Newton mentions Bacon but once, as author of the *History of Henry VII.* In the seventeenth century only Boyle seemed to take Bacon seriously as a theorist of science; and even his tribute lay in words rather than imitation."

2 Colin Maclaurin, *An Account of Sir Isaac Newton's Philosophical Discoveries* (facimilie edn. New York, 1968), p. 59: "Had the philosophers since Lord *Verulam's* time, adhered more closely to his plan, their success had been greater; and Sir Isaac Newton's philosophy had not found the learned so full of prejudice against it;" for according to Maclaurin (p. 56) Bacon is "Justly [to] be held amongst the restorers of true learning, but more especially the founder of *experimental philosophy.*" John F. Fulton, "The Rise of Experimental Method," *Yale Journal of Biology and Medicine*, III (1930-1931), pp. 316-317: Fulton refers to Newton's Principia as the "full fruition" of the Baconian theory.

certain historical facts strongly suggest that Newton was influenced by Bacon indirectly (*i.e.*, through others who were more or less enthusiastic about him). Newton can perhaps be called a "Baconian", but in a rather loose and somewhat Pickwickean sense.

I. BACON

Bacon was outspoken in his belief that the sciences had long been pursued in a fundamentally improper way. And although he is usually regarded as the critic of excessive rationalism, whereby the understanding "hurries on rapidly from the senses and particulars to the most general axioms, and from them as principles and their supposed indisputable truth derives and discovers the intermediate axioms" (*Nov. Org.* I. 19), it is clear that he wished also to be regarded as the critic of excessive empiricism, which, he believed, "produces dogmas of a more deformed and monstrous nature than the sophistic or theoretic school" (*Nov. Org.* I. 64). His search was for a "middle way" which would allow the natural philosopher to avoid the errors of the mindless fact-collectors as well as the rationalistic theorizers:

> Those who have treated of the sciences have been either empirics or dogmatical. The former like ants heap up and use their store, the latter like spiders spin out their own webs. The bee, a mean between both, extracts matter from the flowers of the garden and the fields, but works and fashions it by its own efforts. The true labor of philosophy resembles hers, for it neither relies entirely nor principally upon the powers of the mind, nor yet lays up in the memory the matter afforded by the experiments of natural history and mechanics in its raw state, but changes and works it in the understanding. We have good reason, therefore, to derive hope from a closer and purer alliance of these faculties (the experimental and the rational) than has yet been attempted [*Nov. Org.* I. 95].

The harmonious alliance of the experimental and the rational is, for Bacon, the *sine qua non* for progress in the sciences, and this alliance can be consummated through, and only through, adherence to the proper method — *viz.*, the method of induction. The method of induction is the only "instrument" (*organon*) with which the natural philosopher can intelligently and profitably approach the phenomena of the senses and thus become the true scientific empiricist (*cf. Nov. Org.* I. 100).

Of course, there had been induction, and talk of induction, before Bacon came on the scene, but pre-Baconian induction proceeded, according to Bacon, merely by the collection of "positive instances;" it is called "induction by enumeration" or "enumerative induction". Baconiàn induction is purported to be something more sophisticated; it is called "induction by elimination" or "eliminative induction". Bacon's procedure for eliminative induction is fairly well known, but it will prove useful to my purposes to give a brief exposition of it here. It runs as follows: First, a Natural History of the phenomenon under investigation is to be compiled. Next, the facts contained in this Natural History are to be arranged in a series of three "Tables". The Tables are to be constructed in such a way that the investigator can then run through them and, by a process of elimination, discover the causes, laws, or "forms" connected with the phenomenon in question. Suppose the following simple case: We have a phenomenon, *P*, before us and we wish to discover its cause. We first compile a Natural History of *P* and *P*-related phenomena. We next arrange this Natural History in a series of three Tables. The first Table, which Bacon calls the "Table of existence and presence" (*Nov. Org.* II. 11), lists cases in which *P* is observed. It might look like this:

TABLE I:

1. *P* is observed when A, B, and C are observed.
2. *P* is observed when A, B, C, D, and E are observed.
3. *P* is observed when A, B, D, and E are observed.

The second Table, which Bacon calls the "Table of deviation or absence in proximity" (*Nov. Org.* II. 12), lists cases in which *P* is not observed — but of course not *all* cases in which *P* is not observed, for if we were to examine all cases in which *P* is not observed, "our labor would be infinite", says Bacon. "Negatives, therefore, must be classed under affirmatives" (*Nov. Org.* II. 12), which is to say that we are interested only in cases in which the phenomena A through E are observed but *P* is not observed. The second table might look like this:

TABLE II:

1. B is observed when *P* is not observed.
2. C is observed when *P* is not observed.
3. D and E are observed when *P* is not observed.

The third Table, which Bacon calls the "Table of degrees or comparative instances" (*Nov. Org.* II. 13), lists cases in which P is observed with varying degrees of intensity, magnitude, or whatever (as the case may be). It might look like this:

TABLE III:

1. P_1 is observed when A_1, B_1, and C_1 are observed.
2. P_2 is observed when A_2, B_1, C_2, and D_1 are observed.
3. P_3 is observed when A_3, B_4, D_3, and E_1 are observed.

With these Tables before us, we then proceed to make the appropriate "exclusions and rejections" and arrive at the answer: We see, from *Table I*, that every time P is observed both A and B are observed; and this tells us that both A and B are possible causes of P. But we also see, from *Table II*, that B is observed when P is *not* observed; and this tells us that B is *not* a possible cause of P. Consequently, A is the only possible cause of P; and this is confirmed by the fact that we see, from *Table III*, that variations in P correspond to variations in A, and only variations in A.

The first impression left by all this is that it is simply too good to be true; and indeed it is. The most obvious objection to it is that it rests upon an assumption which is certainly *not* true. As Butterfield says of Bacon: "If we look for the root of the error that was in him — the cause that was perhaps behind the other causes — it lay in his assumption that the number of phenomena, the number even of possible experiments, was limited, so that the scientific revolution could be expected to take place in a decade or two."[1] With reference to the simplified example given above, for instance, three very embarrassing questions can be raised: (*i*) Even assuming that A is indeed the cause of P, what is there to guarantee that our Natural History will provide all the data needed to perform the exclusions and rejections necessary for arriving at A? Conversely, (*ii*) assuming that our Natural History provides the data needed to arrive at A, what is there to guarantee that the real cause of P is not the *conjunction* of A with some *unobserved* (or *unobservable*) Z, which is not included in the history? Again, (*iii*) what is there to guarantee that some further observation

[1] Herbert Butterfield, *The Origins of Modern Science* (revised edn. New York, 1965), p. 116.

will not turn up either a case in which *P* is observed when A is not observed, or a case in which A is observed when *P* is not observed? Such difficulties are well known and there is little to be gained laboring them, for the point is clear. As Pap put it: "Generalizations face the danger of being overthrown by contrary instances no matter whether they were reached by elimitative induction or by enumerative induction."[1] This is especially grave for a thinker like Bacon, for bearing in mind his claims for the conclusive certainty of his results, this shows his method of induction to be either a faulty form of deduction, or an extremely handicapped form of "finit induction".

Butterfield, however, goes too far. It is not the case that all of Bacon's errors can be traced to this assumption concerning the *quantity* of the raw phenomena. Bacon also makes a false assumption concerning their *quality — viz.*, the assumption that the phenomena will be presented to us already sorted out, with labels, as it were, indicating which elements are relevant to what problems. Any example of, say, Russell's famous "factory-hooters" type is sufficient to illustrate the dangers here, for not only the 4:00-PM sirens in Manchester, but many other equally irrelevant phenomena as well, occur in constant conjunction with the departure of the workmen from the London factories; and if we were to employ Bacon's methods in seeking the cause of this departure we should have no grounds for not including them in our Natural History, and no way of rejecting them through our examination of the Tables. Of course, Bacon was not so naive as to suppose that cases of this kind would never arise. He was, however, sufficiently naive to suppose that such cases would be exceptional — which is clearly false, since the features of experience which are relevant to the solution of any given problem are invariably accompanied by irrelevant ones; and he believed that such problems could be easily handled by means of "crucial experiments", or as he called them, crucial "instances" (*instantiae*):

> When in investigating any nature the understanding is, as it were, balanced, and uncertain to which of two or more natures the cause of the required nature should be assigned, on account of the frequent and usual concurrence of several natures, the instances of the cross (*Instantiae Crucis*) show

1 Arthur Pap, *An Introduction to the Philosophy of Science* (New York, 196?), p. 154.

> that the union of one nature is firm and indissoluble, whilst that of the other is unsteady and separable; by which means the question is decided, and the first is received as the cause, whilst the other is dismissed and rejected [*Nov. Org.* II. 36].

But to rely upon this technique in all such cases would be both unnecessarily laborious and ineffective as a general procedure. Quite apart from the dubious status of so-called crucial experiments in science,[1] their feasibility is severely limited to cases in which the phenomena can be readily manipulated and tightly controlled.

The main point, however, is this, that in any inference of discovery we not only do, but we also must rely upon something that neither the eliminative induction method, nor any other recipe-method can provide. In mundane, factory-hooter type cases we call it "common sense." In the more complicated and recondite cases dealt with by the physicist, the astronomer, and the biologist, we call it something else—namely, "insight", "scientific imagination" or perhaps "genius". If there is some one "root cause" of Bacon's errors, it is not that he miscalculated the number of available phenomena, but simply that he was utterly blind to the value, not to mention the necessity, of insight and genius in scientific discovery: he believed quite firmly that his method was "such as to leave little to the acuteness and strength of wit and indeed to level wit and intellect" (*Nov. Org.* I. 61). But the pretences of his method to mechanical rigor are illusory and its aim—"to level wit and intellect"—basically misguided. As Broad put it: "Like other philosophers of his time, Bacon made the mistake of thinking that, because a good method is necessary in order to accomplish anything, it is sufficient to accomplish everything."[2] Moreover, we should not expect a scientist who indeed has an "acuteness and strength of wit" consciously to submit himself either to the spirit or to the letter of Bacon's directions. Newton, of course, was just such a scientist.

[1] For the classical critique of "crucial experiments," see Pierre Duhem, *The Aim and Structure of Physical Theory* (Atheneum ed., trans. Philip P. Wiener. New York, 1962), pp. 188–190.

[2] C. D. Broad, "Francis Bacon and Scientific Method," *Nature*, CXVIII (1926), p. 487.

II. NEWTON

Newton's best-known, though perhaps not his most informative, statement on scientific method is his "rejection of hypotheses" which occurs towards the end of the General Scholium of the *Principia* (2nd edn., 1713):

> . . . .I frame no hypotheses *(hypotheses non fingo)*; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterward rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation were discovered [*Principia*, p. 547 (Motte-Cajori)].

There is much to be said for airing this passage early in a discussion of Newton's method. Its chief value, however, does not lie in what it says about "hypotheses" *per se* (the difficulties of interpreting "*hypotheses non fingo*" are well known).[1] Its chief value for my purposes lies, rather, in its general avowal of empiricism,[2] which, when coupled with Newton's actual mathematico-deductive procedures (especially in the *Principia* itself), produces what some regard as an anomally of the first magnitude. Randall, for example, says: "Newton's actual mathematical procedure made it necessary for him to assume much that his empiricism could not justify; and in his ideas of "the real world" his scientific procedure and his empirical theory [of scientific methodology] collide violently."[3] I believe that this is largely correct, but I also believe that Randall

[1] For a sobre and illuminating discussion of this passage and of Newton's treatment of "hypotheses" in general, see Alexandre Koyre', "Newton's Scientific Thought," in *Newtonian Studies* (London, 1965), pp. 25-52.

[2] E. W. Strong contends that the "rejection" is not a general avowal of empiricism, but is only Newton's "judgment upon the pertinence of the theological discussion which immediately precedes it" ("Newton's mathematical Way," *Journal of the History of Ideas*, XII (1951), p. 102). But with all respect, this seems hardly defensible in view of the wide range of subject-matters which the rejection is, by Newton himself, said to cover.

[3] John Herman Randall, Jr., *op. cit.*. vol. I, p. 585.

overstates his case and that he fails to explain the historical circumstances which account for the degree of truth which his thesis does indeed possess. In the present section I should like to qualify Randall's thesis by examining the relations between Newton's "empiricism" and his "mathematical rationalism" more sympathetically. In Section III I shall suggest an explanation for the conflict which nevertheless remains. It is worth noting that while the above quotation from Randall is cited from his *Career of Philosophy* (1962), the same remark appears, *verbatim*, in an essay of his which appeared twenty years earlier.[1] In the meantime (1951), however, there appeared an important paper by E. W. Strong, in which Strong argued that Randall was mistaken.[2] In the course of my discussion I shall suggest that Randall should not have ignored Strong so completely.

A look at some of Newton's other methodological statements will provide a starting point for assessing this case. The principal passages are Newton's Preface to the first edition of the *Principia*, Cotes's Preface to the second edition, the Rules of Reasoning in Philosophy from Book III of the *Principia*, and the concluding paragraphs of *Quaery 31* of the *Opticks*. I shall consider especially the latter. In *Quaery 31* Newton describes his method as consisting in two "methods"—"the method of analysis," and "the method of synthesis" (or "composition"). The method of analysis consists in "making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no exceptions against the conclusions but such as are taken from experiments, or other certain truths." The method of synthesis consists in "assuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and providing the explanations." The basic point which Newton urges is that "the investigation of difficult things by the method of analysis ought ever to precede the method of composition." The Rules of Reasoning add little substance to this, but they emphasize the importance of "analysis" by providing another statement of the principle of admitting only empirically grounded objections against the conclusions of induction (Rule IV).

[1] John Herman Randall. Jr., "Newton's Natural Philosophy: Its Problems and Consequences," in Clarke and Nahm (ed.), *Philosophical Essays in Honor of Edgar Arthur Singer* (Philadelphia, 1942).

[2] E. W. Strong, *op. cit.*

The key to Newton's views on empiricism and induction lies in this method of analysis; for clearly, this is the empirical phase of Newton's overall method, and it is in this that propositions are to be "deduced from the phenomena" and "rendered general by induction," after the teaching of the "rejection of hypotheses" in the General Scholium. Newton's own description of this method, however, is far from clear. Maclaurin explains it as follows: "that we should begin with the phenomena, or effects, and from them investigate the powers or causes that operate in nature; that from particular causes, we should proceed to the more general ones, till the argument ends in the most general: this is the method of analysis."[1] Although this taken just by itself is scarcely more illuminating than even the general Scholium, it becomes informative when considered in conjunction with an objection which has been made against it.

In his introduction to the facsimile edition of Maclaurin's book, Laudan criticises Maclaurin by saying that "although he uses the analysis-synthesis language, Maclaurin does not claim that theories are deducible from observations, nor does he adopt the Newtonian-Baconian doctrine of induction. Analysis is not for Maclaurin, as it was for Newton, a mechanical method of deducing or inducing theories from facts. As Maclaurin explains it, analysis is little more than grounding science on an experimental foundation."[2] But in defense of Maclaurin, where does Newton say that analysis is a "mechanical method of deducing or inducing theories from facts"? For that matter, where does Newton say that analysis is such a great deal more than "grounding science on an experimental foundation"? Laudan, like many other writers, is evidently misled, probably by phrases like "deduction from the phenomena," into beleiving that Newton regards theories as deducible (in the logician's sense of that term) from particular facts. Newton, however, explicitly rejects this view when he denies that analysis yields demonstrative conclusions: "the arguing from experiments by induction...[is] no demonstration of general conclusions" (*Opticks, Qu 31*, p. 404). The phrase "deduction from the phenomena" does not mean logical demonstration, and in supposing that it does, Laudan is in fact insisting that Newton adopt that Baconian demand for mechanical rigor which, as I noted earlier, it would be unreasonable to expect in Newton's work.

1 Colin Maclaurin, *op. cit.*, pp. 8-9.
2 *ibid.*, p. xvi.

For Bacon, induction *is* demonstrative in the sense that, granted Bacon's assumptions, the conclusions are purportedly reached by a strict process of elimination and are therefore purportedly necessary. For Newton, however, induction is merely "the best way of arguing which the nature of things admits of" (*Opticks*, *Qu. 31*, p. 404). It should be remembered, too, that Newton's principle of analysis is to admit "no objections against the conclusions but such as are taken from experiments or other certain truths" (*Opticks*, *Qu. 31*, p. 404)—to "look upon propositions inferred by general induction from phenomena as true *or very nearly true* till such time as other phenomena occur, by which they may either be made more accurate, or *liable to exceptions*" (*Principia*, p. 400, emphasis added). He does not say that we should admit no objections or entertain no exceptions at all, which is in fact what he would be obliged to say if Laudan's interpretation of his intentions were correct. Reflection will show that it makes no sense to speak of "objections" or "exceptions" to conclusions supposedly reached by the "mechanical method" of Bacon. Thus Maclaurin's looser construction of the method is after all the more nearly correct one.

With this established, Strong's discoveries become especially valuable. Strong argues that there is no wholesale clash between Newton's professions of empiricism and his use of deductive, mathematical procedures: Deduction (*i. e.*, the "method of synthesis") for Newton is always from "principles," and the results of deduction are never to be regarded as more certain than the principles upon which they are based.[1] Moreover, these principles are for the most part discovered empirically, that is, "inferred from the phenomena and afterward rendered general by induction" (*i. e.*, the "method of analysis"). In other words, there is at least a sense in which Newton's empiricism and his mathematicism go hand-in-hand and, so far from being at loggerheads with one another, in fact depend upon one another *methodologically*: empirical analysis provides the grist for the synthetic (mathemetico-deductive) mill. There are at least three reasons why this is not commonly recognized. First is the widely received, if somewhat vague, notion that Newton's induction is itself "rationalistic" (*i. e.*, *quasi*-mathematical and demonstrative). Second is the widely received, and equally vague,

[1] *Cf*. Letter from Newton to Oldenburg, June 11, 1672, in H. W. Turnbull (ed.), *The Correspondence of Isaac Newton* (Cambridge, England, 1959), vol. I, pp. 187–188.

philosophical prejudice that "empiricism" and "rationalism" are, on the one hand, adequate descriptive labels for actual scientific methods and, on the other hand, both conjointly exhaustive and mutually exclusive. Third is the widely received view that Newton's methodological precepts can be adequately understood and criticised without attending to how they might be exemplified in his actual scientific work. My critique of Laudan was addressed to the first. Strong's study is addressed to the second and third.

Working through concrete examples in the *Opticks* and the *Principia*,[1] Strong shows the central methodological importance for Newton of "measures"—*i. e.*, (numerical) ratios or proportions derived (or "inferred") from empirical measurements on experimental phenomena (*cf. Opticks*, II. 1, obss. 1-7, which culminates in a table of "measures" in this sense). It is on the basis of such measures that Newton formulates his mathematicised optical or mechanical "principles": this is the method of analysis. With these principles established and received with the appropriate degree of certainty, Newton proceeds to demonstrate mathematically "the phenomena proceeding from them" (*Opticks, Qu. 31*): this is the method of synthesis. Newton, then, can be called both an "empiricist" and a "rationalist"—the former insofar as he does employ empirical investigation to arrive at his principles, the latter insofar as his principles tend to follow upon measures and form the basis of mathematical demonstration. Strong emphasises the priority of the "empiricist" label by saying: "There are no laws of mechanics [or of optics] which are supplied solely by reasoning in mathematics. Demonstration, of course, is a procedure of mathematical reasoning, but such reasoning, as Newton states, is from "the laws and *measures* of gravity and other forces."[2] It cannot be denied that there are case histories in Newton's work which fit Strong's description. The universal applicability of this description is another question, however, and will be raised in the next section.

It is fitting to close this section by calling attention to two points which bear upon the Bacon/Newton question. First, from the point of view of someone actively engaged in the pursuit of science, a method is not an abstract entity valued for its own sake; a method is a means of achieving something. It is clear,

1 E. W. Strong, *op. cit*, pp. 96-101.

2 *Ibid.*, p. 96. Newton's remark is from his Preface to the First Edition of the *Principia* (1686); the emphasis is added by Strong.

moreover, that what Newton wished to achieve was a mathematico-deductive system of the empirical world: "and therefore I offer this work as the mathematical principles of natural philosophy, for the whole burden of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena..." (*Principia*, Preface (1st edn.)). This fact alone seems sufficient both to disassociate Newton from the main-stream of any genuinely Baconian tradition and to locate him squarely in the tradition of Galileo[1] and Descartes.[2] Of course, Descartes is less empirical-minded than Galileo,[3] and less empirical-minded still than Newton but his positive historical importance as a theorist and methodologist of science cannot be ignored; and as A. R. Hall points out, there was in fact little in the *Discourse on the Method* which ran counter to either Galileo or Newton: it was Descartes's actual system, as set forth in the *Principles of Philosophy* and the

1 "Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters, are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth" ("The Assayer" (1623), in Stillman Drake (trans.), *Discoveries and Opinions of Galileo* (Garden City, New York, 1957), pp. 237-238).

2 "Those long chains of reasoning, simple and easy as they are, of which geometricians make use in order to arrive at the most difficult demonstrations, had caused me to imagine that all those things which fall under the cognizance of man might very likely be mutually related in the same fashion; and that, provided only that we abstain from receiving anything as true which is not so, and always retain the order which is necessary in order to deduce the one conclusion from the other, there can be nothing so remote that we cannot reach it, nor so recondite that we cannot discover it" (*Discourse on the Method* (trans. E. S. Haldane and G. R. T. Ross. Cambridge, England, 1911), vol. I, p. 92).

3 The pendulum of opinion on the question of Galileo's empiricism has swung from one extreme to another, the older image of Galileo the empirical positivist having been replaced by the image of Galileo the mathematicising arch-*apriorist* (*cf.* Alexandre Koyre', *Etudes Galile'ennes* (1935-1989. Paris, 1966), *passim;* E. A. Burtt, *The Metaphysical Foundations of Modern Science* (Anchor edn. Garden City, 1954), *espec.* pp. 74 ff.). In a more recent study, Dominique Dubarle argues, successfully I think, that although Galileo indeed fails to conform to the older positivist image, there is an important empirical side to him nevertheless ("Galileo's Methodology of Natural Science," in Ernan McMullin (ed.), *Galileo: Man of Science*

Traite' du Monde, to which both Galileo and Newton took exception.[1] Second, it is important to note the difference between a *measure*, in Newton's technical sense, and a measurement or a mere collection of measurements: a measure, quoting Strong again, represents "the *comprehension* of the scientist of the *relevance* of what he has measured.. In some cases, the arraying of measurements may be *intuitively grasped* by the scientist as exhibiting a...measure...One must know not only how to measure but *what to measure*."[2] This brings us once again to the importance of insight and genius, which Bacon had wished to do away with. Newton is an empiricist with an "acuteness and strength of wit."

III. LOOSE ENDS: NEWTON, BACON, AND BARROW

At this point it might appear that Newton was indeed not influenced by Bacon and that there is, after all, no conflict between his empiricist professions and his mathematical practices. These impressions need to be corrected, and in this section I wish to call attention to some evidence for Baconian influence on Newton and so raise again—in a more precise way—the question of non-empirical (*i. e.*, *un*empirical) elements in Newton's theories. These two topics might at first seem not to go together, but I believe that they do go together.

It is possible, I think, to specify a sense, or senses, in which Newton was influenced by Bacon. This is a difficult matter, however, for not only is it clear from the foregoing that he is far from being an orthodox Baconian, but also the rarity of his expressions of indebtedness to others tends to keep his sources of influence well hidden. There are, however, helpful suggestions

(New York, 1967), pp. 295-313, *espec.* 305 ff.). The famous methodological passage in Galileo's 1604 Letter to Sarpi (*Opere*, X, pp. 115-116), which is frequently quoted in connection with arch-*apriorist* interpretations (*cf.* Alexandre Koyre', "La loi de la chute des corpes," in *Etudes Galile' ennes*, p. 87; Marie Boas, *The Scientific Renaissance*, 1450-1630 (New York, 1962), p. 224), has been given an empirically-based explanation by Thomas B Settle ("Galileo's Use of Experiment as a Tool of Investigation," in McMullin, *op. cit*, pp. 318-319).

1 A. R. Hall, *From Galileo to Newton*, (London, 1963), pp. 113-114.

2 *Ibid.*, p. 97, emphasis added. *Cf. Opticks*, I, 1, Prop. 6, Th. 5, where Newton indicates that the "late writers in Opticks" had erred not in their measurements so much as in their inability to comprehend the significance of their measurements.

between the lines of his more "experimentalist" writings, and I should like to pursue some of these. Also, I believe that some light can be shed on this issue by calling attention to certain features of the historical circumstances in which Newton's work was carried out.

First, as regards the experimentalist writings, what I have in mind is not merely the impossibility of reading, *e. g.*, the "New Theory of Light and Colours"[1] without being impressed by Newton's skill as an experimenter, but also the impossibility of reading, *e. g.*, *Quaery 31* of the *Opticks* or the essay (unpublished) "On the Air and the Aether"[2] without being impressed by his diligence as a compiler of Natural Histories. *Quaery 31* covers roughly thirty pages (in the fourth and final edition), the first twenty of which are concerned with the existence and operation of attractive and repulsive forces between the particles of matter. The basic premisses of the investigation are that the compounding or mixing of two substances indicates the operation of an attractive force between the particles composing those substances, and that the diffusion of the particles of a given substance (a compound or mixture) indicates the operation of a repulsive force between the particles composing that substance. Newton is not concerned with the mechanism of these attractions and repulsions, moreover, but only with the laws according to which they take place: "For we must learn from the phenomena of Nature what bodies attract [or repel] one another, and what are the laws and properties of the attraction [or repulsion], before we enquire the cause by which the attraction [or repulsion] is performed." So says Newton in the opening paragraph; and what follows is, in the most straightforward sense, a Natural History, in which are listed approximately one hundred substances and their behaviors under various conditions. Of course, Newton never constructs Baconian "Tables"—I have already insisted that it would be unreasonable to expect him to do this; but even so, there are passages which might well remind us of such Tables. For example:

1 "A Letter of Mr. Isaac Newton, Professor of Mathematics in the University of Cambridge; Containing his New Theory of Light and Colours," *The Philosophical Transactions of the Royal Society of London*, VI (1671/2). This paper was completed and sent to the Editor on February 6, 1971/2. It is reprinted in H. W. Turnbull, *op. cit.*, vol. I, pp. 92–102. My references will follow the pagination in Turnbull.

2 In A. R. and M. B. Hall (edd.). *Unpublished Papers of Isaac Newton* Cambridge England, 1962), pp. 214–228 (Eng. trans., pp. 221–228).

> For when salt of tartar runs *per deliquium* is not this done by an attraction between the particles of the salt of tartar, and the particles of the water which floats in the air in the form of vapours? And why does not common salt, or saltpetre, or vitriol, run *per deliquium*, but for want of such an attraction? Or why does not salt of tartar draw more water out of the air than in a certain proportion to its quantity, but for want of an attractive force after it is satiated with water?

That is, (*a*) there is a natural attraction between water and salt of tartar. So, when salt of tartar runs *per deliquium* it is reasonable to suppose that this is due to a mutual attraction between the particles of salt of tartar and the particles of water in the air. And (*b*) there is not this attraction between water and *common* salt, *etc*. So, when common salt does not run *per deliquium* it is reasonable to suppose that this is due to a lack of mutual attraction between the particles of common salt and the particles of water in the air. Moreover, (*c*) when salt of tartar runs *per deliquium*, the quantity of water drawn out of the air is proportional to the quantity of salt of tartar present. It is hard to believe that the similarities between the logical roles played by the instances in (*a*), (*b*), and (*c*) and the logical roles played by the instances in Bacon's *Tables I, II,* and *III* are either imaginary or purely coincidental. To be sure, there is none of the "Baconian rigor" here, but this is hardly surprising; there *is* a general correspondence with the Baconian model, and any careful reading of *Quaery 31* will reveal many more like it. Such correspondences can also be found in certain earlier works of Newton, such as "On the Air and the Aether" (*ca.* 1673-1675).[1]

1 Robert Kargon has called this essay "One of the best examples of Newton's method" ("Newton, Barrow and the Hypothetical Physics," *Centaurus*, XI (1965); p. 52). Like the *Quaeries*, it involves a great deal of natural history, for example, but compared with the *Quaeries*, it is considerably rougher and less sophisticated (*e. g.*, it argues exclusively from "positive instances," as opposed to the *Quaeries*, which also argues from "negative instances" (*cf.* (*b*) in the example above) as well as from "concomitant quanta" (*cf.* (*c*) in the example above). In fact, "On the Air and the Aether" seems to be both incorporated into and superceded by *Quaery 31* (*e. g.*, compare "On the Air Aether," p. 221 with *Opticks*, pp. 391-392; pp. 222-223 with *Opticks*, pp. 396-397; pp. 223-226 with *Opticks*, pp. 387-389; pp. 226-227 with *Opticks*, p. 396).

The fact that Newton is definitely not above the practice of compiling such Natural Histories and using them as a basis for "tabular" investigation and argument is suggestive of some sort of Baconian influence, but it is by no means conclusive. Additional evidence needs to be culled from other quarters. Consider Newton's famous " New Theory of Light and Colours." In 1666, Newton says, he "procured a triangular glass prism, to try therewith the celebrated phaenomena of colours."

> It was at first a very pleasing diversion to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I was surprised to see them in an oblong form; which, according to the received laws of refraction, I expected would have been circular...
>
> Comparing the length of this coloured spectrum with its breadth, I found it about five times greater; a disproportion so extravagant, that it excited me to more than ordinary curiosity of examining from whence it might proceed [p. 92 *cf. supra*, n. 22)].

After satisfying himself that the discrepancy was due neither to a fault in his prism, nor a difference of incidence of rays coming from diverse parts of the sun, nor the prism causing the rays to move in curved lines (!), he proceeds:

> The gradual removal of the suspicions at length led me to the *experimentum crucis*, which was this: I took two boards, and placed one of them close behind the prism at the window, so that the light might pass through a small hole, made in it for the purpose, and fall on the other board, which I placed at about 12 feet distance, having first made a small hole in it also, for some of that incident light to pass through. Then I placed another prism behind this second board, so that the light, trajected through both the boards, might pass through that also, and be again refracted before it arrived at the wall. This done, I took the first prism in my hand, and turned it to and fro slowly about its axis, so much as to make the several parts of the image, cast on the second board, successively pass through the hole in it, that I might observe to what places on the wall the second prism would refract them.

> And I saw, by the variation of those places, that the light tending to that end of the image, towards which the refraction of the first prism was made [*i. e.*, the violet], did in the second prism suffer a refraction considerably greater than the light tending to the other end [*i. e.*, the red]. And so the true cause of the length of that image was detected to be no other, than that light consists of rays differently refrangible, which, without any respect to a difference in their incidence, were according to their degrees of refrangibility, transmitted towards diverse parts of the wall [pp. 94-95].

This experiment, of course, forms the basis for Newton's theory of light, and it is well known to represent one of the most revolutionary discoveries in the history of optics. What is of primary interest in the present connection is the simple fact that Newton calls it an *experimentum crucis.*

The term "*experimentum crucis*" has traditionally been regarded as a Baconian term; and indeed it is. As has been pointed out in a recent essay by J. A. Lohne, however, it is not *Bacon's* term. It was coined not by Bacon but by Robert Hooke, misquoting Bacon. He introduced it in commenting upon the experiment in which he discovered that colors can be produced by pressing together thin glass plates:

> This experiment therefore will prove such a one as our *thrice excellent Verulam* calls *Experimentum Crucis*... serving as a guide or Land-mark, by which to direct our course after the true cause of Colours. Affording us this particular negative Information, that for the production of Colours there is not necessarily either a great refraction, as in the Prisme; nor Secondly, a determination of Light and shadow, such as is both in the Prisme and Glass-ball [*Microgr.*, p. 54].

Bacon used the term "*Instantiae Crucis*" (*cf. supra*, p. 7). He also used the term "*Experimenta Lucifera*"; and it has been suggested that it was by confusing "*Instantiae Crucis*" and "*Experi-*

1 J. A. Lohne, "Experimentum Crucis," *Notes and Records of the Royal Society of London*, XXIII (1968), p. 174.

menta Lucifera" that Hooke came to write "*Experimentum Crucis.*"[1] More important than what particular confusion might have taken hold of Hooke's mind, however, are the following points: (*i*) Hooke passed the term off as Bacon's; (*ii*) he was mistaken; (*iii*) there is strong evidence to show that Hooke had studied Bacon (*cf. infra*), indisputable evidence to show that Newton had studied Hooke,[2] but no strong evidence to show that Newton had studied Bacon; (*iv*) neither Hooke nor Newton in fact uses the term in quite the same sense, and neither of them uses it in the sense in which Bacon had intended it: For Bacon an *instantia crucis* is a piece of evidence which conclusively decides against one and therefore in favor of another of two possible explanations. For Hooke it is not quite this, for an examination of the context from which the above quotation was taken reveals that his *experimentum crucis* is being used almost exclusively as a weapon against Descartes rather than as a tool for discovery. And for Newton the usage is looser still, for his *experimentum crucis* was not even undertaken until the alternative explanations had already been dismissed: his *experimentum crucis* was simply a very telling experiment.[3] The suggestion, of which the "*experimentum crucis*" provides a paradigm case, is that if Newton was influenced by Bacon, he was influenced at second hand.[4] This suggestion fits well with Newton's Natural Histories and "tabular" analyses in *Quaery 31.*

It is true that Newton never refers to Bacon as one of his mentors, but the question of whether one thinker might have had significant influence on another cannot be given a negative answer *ex silentio*. The two pieces of evidence just adduced show that Newton was somehow in the Baconian swing of things, and they

1 *Ibid.*

2 Newton's notes in Hooke's *Micrographia*, which date from well before 1672, are printed in A. R. and M. B. Hall, *op. cit*, pp. 400–413.

3 Hooke and Newton had some considerable disagreement over this, Hooke denying that Newton's experiment was an *experimentum crucis* properly so called, Newton reaffirming that it was, and Hooke remaining unconvinced (see H. W. Turnbull, *op. cit*, vol. I, Letters 44, 67, and 71). A comparison of Newton's paper with Hooke's *Micogr.* shows that Hooke was correct in denying that the experiment was an *experimentum crucis* in his (Hooke's) sense.

4 As for the question of Newton's study of Bacon, I know of nothing which contradicts Randall's remark that "Newton mentions Bacon but once, as an author of the *History of Henry VII*" (*cf. supra*, n. 1). Lohne (*loc. cit.*) mentions the possibility that Newton may have had only a second-hand knowledge of Bacon.

provide *prima facie* grounds for believing that Newton might have felt some sort of hommage toward Bacon. The second in particular calls attention to something so obvious that it often goes ignored. When it is a question of whether So-and-so could have been influenced by So-and-so, it is a mistake to confine attention merely to the works and sayings of So-and-so and So-and-so. The simple fact is that Bacon was an extremely popular and revered figure among the intellectuals of Newton's time—especially among the members of the Royal Society. (Newton was elected Fellow of the Society in 1672, ten years after the granting of the first charter.) Randall's remark that "In the seventeenth century only Boyle seemed to take Bacon seriously as a theorist of science" (*cf. supra*, n. 1) is quite incorrect.[1] The very first History of the Royal Society (1667),[2] for example, has Bacon depicted in the frontspiece along with the royal founder and the first President of the Society. And it is perhaps more interesting still that even prior to this, in 1660 —just one year before Newton went to Cambridge and six years before he "procured his prism"—there was published a *Continuation* of the *New Atlantis,* which Bacon had left unfinished at his death in 1626. This *Continuation* was written by a certain Mr. R. H. Esquire, whom most scholars agree in identifying as none other than the eminent Dr. Robert Hooke.[3]

I have emphasized several times that the orthodox Baconian method is not a workable method for the practicing scientist. Bacon, however, was something of a legendary figure in the seven-

1 In addition to the references indicated *infra, cf.* Thomaso Campanella, *Realis philosophiae epilogisticae* (Frankfurt, 1623), p. 16; Thomas Stanley, *The History of Philosophy* (London, 1655); C. Barksdale, *Memoirs of Worthy Persons* (London, 1661), pp. 175-187; A. Crowley, *Proposition for the Advancement of Experimental Philosophy* (London, 1661), p. 28; Thomas Hobbes, *Problemata physica* (London, 1662), p. 26; Henry Power, *Experimental Philosophy* (London' 1664), *passim*; N. Malebranche, *De la recherche de la verite'* (Paris, 1674), p. 200; D. Abercromby, *Acadamia scientiarum* (London, 1687), p. 156; T. P. Blount, *Censura celebriorum authorum* (London, 1690), pp. 634-636; A, Baillet, *Vie de Descartes* (Paris, 1691), *passim.* Also, Henry Oldenburg makes frequent references to Bacon in early volumes of the *Transactions*: see prefaces to 1670, 1672, 1677.

2 Thomas Spratt, *The History of the Royal-Society of London* (London, 1667).

3 *Cf* Edmund Freeman, "A Proposal for an English Academy in 1660," *Modern Language Review*, XIX (1924), pp. 291-300; Geoffrey Keynes, *A Bibliography of Dr. Robert Hooke* (Oxford, 1960), pp. 2-4; Frank Manuel, *A Portrait of Isaac Newton* (Cambridge, Massachusetts, 1968), pp. 134-135. Also, see Hooke's *Posthumous Works* (London, 1705), pp. 1-70.

teenth century scientific world: it was thought that he had said and done great things for the advancement of science, if (perhaps) only because certain people were too busy praising him to attend carefully and critically to his actual work. He was indeed, to use Whewell's description, the "Legislator of Science," and it is hard to doubt that some form of Baconianism, however vague, was broadcast under the title of "*the* scientific method." Viewing the question historically, it would seem quite unthinkable that a respected natural philosopher, and Englishman, of the seventeenth century should not have been heavily influenced by Baconian ideas and incantations. The general empirical bias of Newton's statements on scientific method and much of his actual scientific work are precisely what we should expect from a brilliant man who is working in such an atmosphere and who has not devoted much, if any, personal effort to the first-hand study of Bacon's writings.

But Newton's "second-hand Baconianism," if I may call it that (construing "second-hand" strictly, and "Baconianism" somewhat loosely), is only one aspect of a larger historical picture, for although it is true that Bacon's influence was heavy in the air which Newton breathed, something else, just as important, was also heavy in the air. This something else was the idea of a mathematical physics.

Of course, the idea of a mathematical physics can be traced back to Plato and the Pythagoreans in antiquity and to Descartes and the Copernicans in the modern era: this story has been told many times. But in Newton's day the need for a new mathematical physics was being sounded from various august sources in England—for example, Christopher Wren in his *Parentalia*, and Isaac Barrow in his lectures.[1] In an especially important series of lectures delivered in 1664-1665 Barrow put forth this idea as the only same alternative to the extravagances of the Cartesians, who could not successfully make the separation of physics and metaphysics, and produced not science but endless disputation. "Mathematicians," said Barrow, "only meddle [*sic*] with such Things as are certain, passing by those that are doubtful and unknown. They profess not to know all things. What they know to be true, and can make good by invincible Arguments, that they publish.[2] That is, physics is not only a mathematical science, but also a science of solvable

[1] Isaac Barrow, *Mathematical Lectures Read in the Publick Schools* (London, 1734).
[2] *Ibid.*, p. 64.

problems. It is especially significant that Barrow should have been one of these new "Legislators of Science," for Barrow's iufluence on Newton is well known;[1] and it is remarkable that Barrow should have said what he did say in these lectures of 1664-1665, for it is certain that Newton attended these lectures.[2] Newton's Preface to the First Edition of the *Principia* shows that he took this sort of thing very seriously, and this is confirmed, if confirmation is needed, by the *Principia* itself. All this, oddly enough, brings us back both to the Bacon/Newton question and to the question of unempirical elements in Newton's theories.

On the basis of considerations of the kind just adduced, Kargon describes Newton's method as "a quantitative version of Baconian requirements"—*i. e.*, "mathematical Baconianism."[3] This idea is suggestive, but it is too accommodating, and it fails to meet the issues squarely. On behalf of the "mathematical Baconian" idea, the following can be said: Newton compiles and uses Natural Histories, *etc.*, in an effort to arrive at "measures" and quantifiable laws—laws which he can translate into mathematical notation and subject to mathematical transformations. Once he establishes such laws, he frequently strikes the Natural Histories from his exposition and presents his material in a more economic and logically-connected manner. The paradigm case of this would be the *Principia* in its final form,[4] but it is likely that the description also fits the "New Theory of Light and Colours:" it is hard to believe that Newton is telling the entire story in this paper, for if he were, then we should have to believe that he "procured his prism" and in scarcely any time at all made one of the most revolutionary discoveries in the history of optics—which sounds rather too easy.[5] And this accounts for—or tends to account for—the fact that Newton never published anything (except in the form of *Quaeries*)

1 *Cf.* Lewis Trenchard More, *Is ac Newton* (London, 1934), pp. 199-200.

2 Newton indicates that he attended the lectures, but that he was somewhat Vague about their content (Cambridge University Library, Add. MS. 3968 5, fol. 21r.; *cf.* Frank Manuel, *op. cit.*, p. 97). Newton's "vagueness," however, was perhaps due less to a lack of interest than to the largely programmatic nature of Barrow's lectures.

3 Robert Kargon, *op. cit.*, p. 54.

4 *Cf.* John Herivel, *The Background to Newton's Principia: A Study of Newton's Dynamical Researches in the Years 1664-1684* (Oxford, 1965)

5 J. A. Lohne (*op. cit.*, p. 169) mentions that from 1666 onwards Newton had performed a great variety of optical experiments, whereas only three are recorded in the famous paper of 1672/3.

on the "true" composition of matter and the causes of the forces which are so central to his physics; for he was evidently never able to satisfy himself that he had reduced the relevant phenomena to quantifiable laws. Essentially, this thesis in indistinguishable from Strong's.

However, it leaves some crucial pieces out of the picture, and Randall's contention that "Newton's actual mathematical procedure made it necessary for him to assume much that his empiricism could not justify" cannot, after all, be dismissed. Recall the "rejection of hypotheses". Here Newton states that hypotheses have "no place in experimental philosophy," that in this philosophy "particular propositions are inferred from the phenomena and rendered general by induction," *and* that it was in this way that "the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation were discovered." But surely, reflection will show that there is something suspect in the claim that the impenetrability and the impulsive force of bodies are things discoverable by inductive generalization, and there is something equally suspect in saying this about the laws of motion. An "hypothesis", in the pejorative sense, is for Newton an assumed cause or explanation of phenomena which is not itself empirically verified, which seems most clearly to imply that many of these examples of things reported as "inferred by experimental philosophy" ought rather to have been rejected as 'hypotheses'! This goes to the very heart of Newton's physics. Of course, it is probably the case that from *Newton's point of view* the statement of Law I, for example, involved chiefly the rejection of older models and metaphysical assumptions,[1] but with all respect, the rejection of one set of metaphysical assumptions frequently means the adoption of another; and it is very difficult to see what collection, or possible collection, of empirical observations could have assured Newton of the truth of Law I. Law II, of course, seems somewhat closer to being empirically confirmable, but Newton's statement of it involves the concept of "motive force", which later, more hard-nosed, empiricists tried to eschew through mathematical reduction.

1 It should not be forgotten, however, that Newton's Rule I is virtually indistinguishable from Descartes's First and Second Laws of Nature (*Princ Philos.* II. 37 ff.), which, according to Descartes, were strictly deducible from certain metaphysical principles. John Herivel (*op. cit.*, pp. 44-45, 50-51) even argues, with some plausibility, that Newton actually modelled his formulation of Law I upon Deseurtes's First and Second Laws.

Randall's statement about the "violent collision" in Newton's method is followed by these words: "Newton's real world is . . . made up of absolute masses endowed with an absolute force of inertia, and perhaps with a force of "gravitation", in absolute space and time; while sense experience supplies no evidence for any of these concepts."[1] We can agree with Strong's thesis that there are in Newton "no *laws* of mechanics which are supplied solely by reasoning in mathematics," and nevertheless insist with Randall that there are many *fundamental concepts* of mechanics which are, evidently, supplied solely by a mathematical disposition of mind. Consequently, there are strong objections to the idea that Newton should be regarded as a "mathematical Baconian", for this suggests that his actual method exhibits a harmonious synthesis of the two major methodological viewpoints of his time — to wit, Baconian empiricism and Barrovian mathematicism — while in fact it exhibits strong tension between them.

Newton cannot be called a "mathematical Baconian". He can perhaps be called a "second-hand Baconian", and to call him this is to call him a man of his time. But if we wish to understand him as a man of his time we must also see him as a great mathematician who is deeply enamoured of the idea of a mathematical physics and the methods appropriate to this idea. These two methodological strains are each historical phenomena as much as they are philosophical theories, and that they come together in Newton is an historical fact which cannot be legislated away by philosophical prejudice. Finally, a third "methodological strain" can be found in Newton's insight and genius — his ability to see the importance of what he was observing and the significance of what he was calculating — and to call him a genius is, of course, to call him very much his own man. No philosopher's general formula for "*the* scientific method" (least of all Bacon's) manages to incorporate and articulate the roles of all three of these strains, but there can be little doubt that it was the confluence of all three which contributed to Newton's greatness.

[1] John Herman Randall, Jr., *op. cit.*, vol. I. p. 585.