

# Singletons and Multiples in Science

1961

## Bacon's Problematics of Scientific Discovery

Having taken all knowledge to be his province, Francis Bacon made room even for what was to become sociology. His luminous writings include a charter for the social sciences, a proposed division between their several types, a precept guiding the inclusion of problems that might otherwise be lost to the view of social scientists and, finally, an early, incomplete yet instructive formulation of the hypothesis to which the greater part of this paper is devoted.

With the seeming artlessness of the true artist, Bacon set down in his *Novum Organum* what amounts to a charter for the human sciences:

It may also be asked in the way of doubt rather than objection, whether I speak of natural philosophy only, or whether I mean that the other sciences, logic, ethics, and politics, should also be carried on by this method. Now I certainly mean what I have said to be understood of them all; and as the common logic, which governs by the syllogism, extends not only to natural but to all sciences; so does mine also, which proceeds by induction, embrace everything. For I form a history and tables of discovery for anger, fear, shame, and the like; for matters political; and again for the mental operations of memory, composition and division [this is probably Aristotle's "affirmation and negation," as Fowler makes plain], judgment and the rest; not less than for heat and cold, or light, or vegetation and the like.<sup>1</sup>

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1. *Novum Organum*, book 1, aphorism 127.

Not only is Bacon prepared to encompass the human sciences in his plan, but he is careful to distinguish among them. Almost as though he were among us today exploring the differences and connections between the psychological and social sciences, he describes what he calls "Human Philosophy or Humanity" as having "two parts: the one considereth man segregate or distributively; the other congregate, or in society."<sup>2</sup>

So much for Bacon's effort to legitimate social science at a time when its first glimmerings were evident to only a few and before its sporadic development during that century of genius. No economist, Bacon could in 1615 originate the term, if not the concept, of "balance of trade,"<sup>3</sup> in the same year in which Antoyne de Montchrétien christened "political economy" (in his *Traicté de l'Economie Politique*). No psychologist, he could by anticipation appreciate the efforts, in mid-century, of Hobbes, Descartes, and Spinoza to contemplate the human passions introspectively, attending to problems of perception, sensation, imagination, and the like. No great admirer of mathematics but cognizant of the value of quantification, he could write as he did generations before the extraordinary London haberdasher John Graunt, Sir William Petty, and Gregory King could among them fashion the new political arithmetic and so initiate the serious study of demography, urban sociology, and epidemiology.

Bacon did not, of course, foresee all this. Little in his time would allow him to describe social science, in the fashion Galileo described mechanics, as "the very new science dealing with a very ancient subject." But his announced philosophy of investigation allowed for such a conception. In taking note of this, we need not try to fix a particular date on which the birth of the social sciences was authoritatively registered. After all, Bacon had referred, with approving comment, to beginnings of social science before his time, reminding his contemporaries, for example, that "we are much beholden to Machiavel and others, that write what men do, and not what they ought to do," then adding, in that stately and incomparable

2. *Advancement of Learning*, in *The Works of Francis Bacon*, [hereafter cited as *Works*] collected and edited by James Spedding, Robert L. Ellis, and Douglas D. Heath (Boston, 1863), 6:236–37. The pressures of time on this occasion being what they unavoidably are, I resist the temptation to remind ourselves of what Bacon goes on to say about psychosomatics (if the anachronism is allowed), when he follows precedent in writing of "the knowledge concerning the sympathies and concordances between the mind and body, which being mixed cannot be properly assigned to the sciences of either" (*Ibid.*, pp. 154 ff).

3. In his *Letter of Advice to Sir George Villiers*, in *Works*. Bacon's usage was independent of earlier use of the phrase in Italy; see W. H. Price, "The Origin of the Phrase 'Balance of Trade,'" *Quarterly Journal of Economics* 20 (November 1905): 157, and the typically informed long footnote on the concept-and-term in Joseph A. Schumpeter, *History of Economic Analysis*, edited from manuscript by Elizabeth Boody Schumpeter (New York: Oxford University Press, 1954), pp. 345–46.

Elizabethan prose from which peak we have achieved a steady decline, "For it is not possible to join serpentine wisdom with the columbine innocency, except when men know exactly all the conditions of the serpent; his baseness and going upon his belly, his volubility and lubricity, his envy and stinge, and the rest; that is, all forms and natures of evil: for without this, virtue lieth open and unfenced."<sup>4</sup> And so, in what follows, but without reference to serpentine evil or columbine good, I shall try to obey the precept of Bacon, and before him of Machiavelli, by examining some of "what men [of science] do, not what they ought to do."

Having legitimized the social sciences, having divided them into distinct though connected disciplines, and having directed us to examine the actual and not to mistake it for the ideal, Bacon gives us counsel about the scope of inquiry, urging us to give up the "childish fastidiousness" that would have us examine only those things in nature and society that we find good or pleasant or otherwise attractive. You will recall this bit of advice, destined to be echoed or independently reaffirmed in the centuries since his day by many great men of science—by a Claude Bernard or a Pasteur, among the many:

And for things that are mean or even filthy,—things which (as Pliny says) must be introduced with an apology—such things, no less than the most splendid and costly, must be admitted into natural history. Nor is natural history polluted thereby; for the sun enters the sewer no less than the palace, yet takes no pollution. And for myself, I am not raising a capitol or pyramid to the pride of man, but laying a foundation for a holy temple after the model of the world. That model therefore I follow. For whatever deserves to exist deserves also to be known, for knowledge is the image of existence; and things mean and splendid exist alike. Moreover as from certain putrid substances—musk, for instance, and civet—the sweetest odours are sometimes generated, so too from mean and sordid instances there sometimes emanates excellent light and information. But enough and more than enough of this; such fastidiousness being merely childish and effeminate.<sup>5</sup>

When we consider the particular sense in which scientific discoveries can be said to come about without being dependent upon the undoubted genius of the *particular* scientists who are properly credited with these discoveries, or when we consider, here in passing, what I have considered elsewhere at some length, the sociological import of the frequent clashes over priority of discovery that have marked the history of science—when I examine

4. *Advancement of Learning, Works*, 6: 327.

5. *Novum Organum*, bk. 1, aphorism 120. This same theme was later taken up and amplified by the thoroughgoing Baconian, Robert Boyle, in the first essay of part 1 of *Some Considerations Touching the Usefulness of Experimental Naturall Philosophy, Propos'd in Familiar Discourses to a Friend, by Way of Invitation to the Study of It* (Oxford, 1663).

these and related matters, far from belittling the scientists of genius who have done so much to shape the development of science, I shall only be trying to fathom their distinctive and complex role in that development. Perhaps the precept of Bacon will help us find in these matters seemingly incidental to the work of scientists, "excellent light and information."

After having provided us with an attitude proper to a commemorative occasion such as this one by urging us to take up and develop the force of what the memorialized man has said rather than merely to repeat his words; after having given us a charter for the human sciences in general and having set out a useful though in the end temporary division between the primarily psychological sciences that center on "man segregate" and the primarily social sciences that center on "man congregate"; after having urged us to examine what men do and not merely what they ought to do; and after having warned us, at our peril, not to exclude the apparently mean or trivial from the scope of investigation—after he has done all this, as though it were still not enough, Francis Bacon makes my lot here an easy as well as a pleasant one by practically providing a composite text dealing with the particular subject I wish to examine: the import of a methodical investigation of singleton and multiple discoveries in science for our understanding of how science develops.

Instructed by the ideas that have been developed after Bacon's time, we can piece together from his fragmentary but instructive observations, the prime ingredients of a theory of the social processes making for discovery and invention. I say "piece together" because these ingredients are not to be found in any one place in Bacon's writings, neatly and coherently tied up in a single bundle. In part, my reconstruction is deliberate anachronism. But in part also, it is not so much reading into Bacon as reading him entire to gain a sense of how he conceived scientific discoveries to come about.

To begin with, Bacon wholly rejected the notion that in the new science, discoveries would typically appear at random, dropping down from heaven through the agency of star-touched genius. Instead, he declares that once the right path is followed, discoveries in limitless number will arise from the growing stock of knowledge: it is a process of once fitful and now steady increments in knowledge. This notion of what we should today describe as the accumulative cultural base on which science builds became one of the many Baconian ideas taken up in abundance by his sometimes overly enthusiastic disciples at midcentury. Consider only one of the more devoted of these, John Webster, who in 1654 could pleasurably refer to "our learned Country-man the Lord Bacon" as having made it clear that "every age and generation, proceeding in the same way, and upon the same principles, may dayly go on with the work, to the building up of a

well-grounded and lasting Fabrick, which indeed is the only true way for the instauration and advancement of learning and knowledge.”<sup>6</sup>

Second, Bacon holds that the individual man of science pursuing his daily labors entirely alone would at best produce small change. As he announces in the *Novum Organum*, “the path of science is not, like that of philosophy, such that only one man can tread it at a time.” Consider, he says, “what may be expected from men abounding in leisure”—it would be too much to ask Bacon to foresee the excessively busy life of so many present-day scientists—“and working in association with one another, generation after generation. . . . Men will begin to understand their own strength only when, instead of many of them doing the same things, one shall take charge of one thing and one of another.”<sup>7</sup> This theme, too, was repeatedly picked up in the seventeenth century, not least by the first historian of the Royal Society, “fat Tom Sprat,” who, happily echoing Bacon, could proclaim that “single labours” in science are not enough to advance science significantly; rather, that it requires the “joynt labours of many,” even to the extreme of “joyning them into *Committees* (if we may use that word in a Philosophical sence, and so in some measure purge it from the ill sound, which it formerly had).”<sup>8</sup> And still in the Baconian vein, Bishop Sprat notes that social interaction among men of science facilitates originality of conception; or as he puts it less austere, “In Assemblies, the *Wits* of most men are *sharper*, their *Apprehensions readier*, their *Thoughts fuller*, than in their Closets.”<sup>9</sup>

Having formulated two prerequisites for the advancement of science—the accumulating cultural base and the concerted efforts of men of science sharpening their ideas through social interaction—Bacon returns, time and again, to a third component in the social process of discovery. He tells how his proposed methods of scientific inquiry reduce the significance of the undeniably different capacities of men. You will recall the ringing passage in the *Novum Organum* to this effect:

. . . the course I propose for discovery of sciences is such as leaves but little to the acuteness and strength of wits, but places all wits and understandings

6. John Webster, *Academiarum Examen; or, the Examination of Academies . . . Offered to the Judgment of All Those that Love the Proficiencie of Arts and Science, and the Advancement of Learning* (London, 1654), p. 105. Appropriately enough, the book is dedicated to Bacon.

7. *Novum Organum*, bk. 1, aphorism 113. I take here the instructed translation by Benjamin Farrington, rather than that by Spedding, Ellis, and Heath, or even that by Fowler. See Farrington, *Francis Bacon* (New York: Henry Schuman, 1949), p. 112.

8. Thomas Sprat, *The History of the Royal-Society of London, for the Improving of Natural Knowledge*, [hereafter cited as *History*] (London, 1667), p. 85. The same point of science advancing through the “joynt force of many men” or the “united Labors of many” recurs throughout the *History*; e.g., pp. 39, 91, 102, 341.

9. *Ibid.*, p. 98.

nearly on a level. For as in the drawing of a straight line or a perfect circle, much depends on the steadiness and practice of the hand, if it be done by aim of hand only, but if with the aid or rule or compass, little or nothing; so it is exactly with my plan.<sup>10</sup>

Read out of its immediate context, and out of the numerous other contexts in which Bacon expresses the same thought, this can be easily if not perversely misconstrued. It can be taken to claim that all men of science are on the same plane of capacity. It has often been so mistaken. [As was the case, for example in one of my own observations back in 1938; see chapter 11 of this volume, p. 235.] What is more, it has often been held to affirm that all scientists are being *reduced* to the same level by the methods of science rather than being *raised* to a lofty level of competence.

But as we know from the rest of Bacon's writings, both before and after the *Novum Organum*, he meant nothing of the kind. Repeatedly, he recognizes that men have various capacities, and, in his scheme of things scientific, he provides a distinctive place for each kind. That dreamt-of research institute, Solomon's House, allows for all grades of ability and varieties of skills in a complex division of scientific labor. The institute includes "Merchants of Light," who keep up with the work going on in foreign countries (in the language of today, reporters of scientific intelligence); "Mystery-men" who gather up the earlier experiments in science and the mechanical arts (in today's terms, the men who arrange for retrieval of scientific information); "Pioneers" or "Miners" who "try new experiments, such as themselves think good" (the skilled and creative experimentalists); "Compilers," or the lesser theorists, who examine the accumulated materials to draw inferences from them; "Dowry-men" or "Benefactors" who seek to apply this knowledge (men engaged in what we now call "research and development"); the "Lamps," who "after divers meetings and consults of" the whole number, undertake to "direct new experiments, of a higher light, more penetrating into nature than the former" (the experimentalist directing a series of cumulative experiments); "Inoculators," the technicians who "execute the experiments so directed, and report them"; and finally, his "Interpreters of Nature," who "raise the former discoveries by experiments into greater observations, axioms, and aphorisms"—the pure

10. *Novum Organum*, bk. 1, aphorism 61; also 122. The strong-minded Macaulay made this the butt of attack in his—to some famous, to others notorious—essay on Bacon; and the even-tempered Baconian scholar, Fowler, was moved to say, "Bacon's promise never has been and never can be fulfilled." As, of course, it cannot, if it is read out of the context of the rest of Bacon's writings, so that he can be charged with gross exaggeration. But need we forget this context, better known to Fowler than to any of the rest of us? Farrington, above all others known to me, has recognized that only a misplaced and narrowly focused literalism can lead one to assume that Bacon left no place for the great variability in the talents of men engaged in scientific inquiry. See Farrington, *Francis Bacon*, pp. 116–18.

theorist. Solomon's House makes room also for the advanced students, the "novices and apprentices" in order that the "succession of the former employed men not fail."<sup>11</sup>

Evidently, then, Bacon does not put all men of science on a single plane, nor does he foolishly regard them as altogether interchangeable. Rather, he emphasizes his belief that methodical procedures make for greater reliability in the work of science. Once a scientific problem has been defined, profound individual differences among scientists will affect the likelihood of reaching a solution, but the scale of differences in outcome is reduced by the established procedures of scientific work. Only in this sense and to this degree, does the new science, in the Baconian image, place "all wits and understanding nearly on a level."

To the three components of his implicit social theory of discovery—the incremental accumulation of knowledge, the sustained social interaction between men of science and the methodical use of procedures of inquiry—Bacon adds a fourth and even more famous one. All innovations, social or scientific, "are the births of time."<sup>12</sup> "Time is the greatest innovator." He employs the same instructive metaphor to describe both his own work and that of others, as when he accounts his own part in advancing knowledge "a birth of time rather than of wit."<sup>13</sup> Once the needed antecedent conditions obtain, discoveries are offshoots of their time, rather than turning up altogether at random.

To say that discoveries occur when their time has come is to say that they occur only under identifiable requisite conditions. But, of course, these conditions do not always obtain. In the past, says Bacon, inventions and discoveries have made their appearance sporadically, almost accidentally. This is so because there did not then exist the conditions of cumulative knowledge, the association of men of science and the methodical, composite use of empirical and reasoned inquiry. With the new science, all this will change. There are secrets of nature

. . . lying entirely out of the beat of the imagination, which have not yet been found out. They too no doubt will some time or other, in the course and revolution of many ages, come to light of themselves just as the others did; only by the method of which we are now treating, they can be speedily and suddenly and simultaneously presented and anticipated.<sup>14</sup>

With the increment in this passage, Bacon almost but not quite achieves a sociological conception of the development of science.

To round this out, he need only add the further component that if

11. See *Solomon's House*, in *Works*.

12. *Essays*, in *Works*, 12: 160.

13. *Novum Organum*, bk. 1, aphorism 122.

14. *Novum Organum*, bk. 1, aphorism 109.

discoveries are “a birth of time,” they will be effected by more than one discoverer. Never saying this in so many words, Bacon nevertheless intimates it—and more than once. By paraphrasing his language, I anachronize his idea, yet without doing violence to it. What he all but says is that multiple independent discoveries do occur but not nearly so often as people suppose. The erroneous supposal is made both by those who mistakenly identify their own ideas as ancient ones and by others who claim to find in the actually new what is ostensibly old. This is how Bacon puts it:

That of those that have entered into search, some having fallen upon some conceits [i.e., notions] which they after consider to be the same which they have found in former authors, *have suddenly taken a persuasion that a man shall but with much labour incur and light upon the same inventions which he might with ease receive from others*; and that it is but a vanity and self-pleasing of the wit to go about again, as one that would rather have a flower of his own gathering, than much better gathered to his hand. That the *same humour of sloth and diffidence suggesteth that a man shall but revive some ancient opinion, which was long ago propounded, examined, and rejected*. And that it is easy to err in conceit [the view] *that a man's observation or notion is the same with a former opinion*, both because new conceits [notions] must of necessity be uttered in old words, and because upon true and erroneous ground men may meet in consequence or conclusion, as several lines or circles that cut in some one point.<sup>15</sup>

The vice of what we may call “adumbrationism”—the denigrating of new ideas by pretending to find them old—must not be permitted to blind us to the fact that rediscovery does sometimes occur. It does not follow, however, that all newly emerging knowledge is nothing but rediscovery. Plato was mistaken in saying “that all knowledge is but remembrance.”<sup>16</sup> In part the error comes from the recurrent practice, particularly in “intellectual matters,” of first finding the new idea strange, and then finding it exceedingly familiar.<sup>17</sup> In another part the error comes from the selective perceptions of the reader. “For almost all scholars have this—when anything is presented to them, they will find in it that which they know, not learn from it that which they know not.”<sup>18</sup> Yet apart from this common error of mistaking the new for the old in science, the fact remains that “men may meet in consequence or conclusion” despite their initial diver-

15. *Valerius Terminus of the Interpretation of Nature*, in *Works*, 6: 72–73. The emphases are mine.

16. *Essays; or Counsels Civil and Moral*, essay 58, “Of Vicissitude of Things,” in *Works*, 12: 273; cf. *Advancement of Learning*, *Works*, 6: 88.

17. *Advancement of Learning*, in *Works*, 6: 130: “In intellectual matters, it is much more common; as may be seen in most of the propositions of Euclid, which till they be demonstrate, they seem strange to our assent; but being demonstrate, our mind accepteth of them by a kind of relation (as the lawyers speak) as if we had known them before.”

18. *De Augmentis*, in *Works*, 9: 170.



gence of ideas. In effect, both adumbrationism and the full denial of rediscovery are faulty doctrines; the truth is, in this reconstructed judgment of Bacon, that rediscovery occurs but not as often as the adumbrationists suppose.

Now I am not saying, of course, that Bacon formulated a coherent sociological theory of the composite elements making for discovery in science. That would be adumbrationism with a vengeance. I recognize that I have pieced together his intimations of such a theory from observations scattered through the works he wrote over a span of two decades. But with the advantage of historical hindsight, and of the ideas that were formulated later, we can identify the *ingredients* of such a theory in Bacon. He himself did not see the connections between them. Or, if he saw them, he never recorded them in a form that has come down to us. What is of interest, rather, is that these ingredients should have appeared more than three centuries ago and that many men over a long period of time should have come upon them anew and that they should have begun to compose them into the beginnings of a sociological theory of scientific discovery.<sup>19</sup>

19. Bacon had much else to say that qualifies him as a harbinger of the sociology of science; I cannot deal with these matters here. But at least two sets of observations can be segregated here below to intimate the broad scope of his understanding. First, he notes the problem of the relations between the social structure and the character of knowledge: "Of the impediments which have been in the nature of society and the policies of state. That there is no composition of estate or society, nor order or quality of persons, which have not some point of contrariety towards true knowledge. That monarchies incline wits to profit and pleasure, and commonwealths to glory and vanity. That universities incline wits to sophistry and affectation, cloisters to fables and unprofitable subtilty, study at large to variety; and that it is hard to say, whether mixture of contemplations with an active life, or retiring wholly to contemplations, do disable and hinder the mind more" (*Valerius Terminus*, in *Works*, 6: 76). Thus we must acknowledge that he sees *the problem* of the relations between types of social structure and types of intellectual work, whatever we might think of his hypotheses. And second, he identifies all manner of social considerations that affect the ways in which men of science and learning ordinarily record what they have learned (with the intimation, perhaps, that this sorry variation will have to be sufficiently standardized if the institution of science is to advance knowledge, rather than to congeal it): ". . . as knowledges have hitherto been delivered, there is a kind of contract of error between the deliverer and the receiver; for he who delivers knowledge desires to deliver it in such form as may be best believed, and not as may be most conveniently examined; and he who receives knowledge desires present satisfaction, without waiting for due inquiry; and so rather not to doubt, than not to err; glory making the deliverer careful not to lay open his weakness, and sloth making the receiver unwilling to try his strength. But knowledge that is delivered to others as a thread to be spun on ought to be insinuated (if it were possible) in the same method wherein it was originally invented. And this indeed is possible in knowledge gained by induction; but in this same anticipated and premature knowledge (which is in use) a man cannot easily say how he came to the knowledge which he has obtained. Yet certainly it is possible for a man in a greater or less degree to revisit his own knowledge, and trace over again the footsteps both of his cognition and his consent; and by that means to transplant it into another mind just as it grew in his own" (*De Augmentis*, in *Works*, 9: 122-23; see also pp. 16-18; *Valerius Terminus*, in *Works*, 6: 70-71).

In all this, Bacon had taken hold of a salient truth: the course of scientific development cannot be understood as the work of man segregate. But he exaggerated when he went on to the claim, which remains extravagant even when construed as he evidently intended it, that the new method of science would “level men’s wits and leave but little to individual excellence.” In this gratuitous overstatement he is not alone. For in the centuries since Bacon, scores of observers have repeatedly stated the matter in much the same disjunctive terms: shall we regard the course of science and technology as a continuing process of cumulative growth, with discoveries tending to come in their due time, *or* as the work of men of genius who, with their ancillaries, bring about basic advances in science? In the ordinary way, these are put as alternatives: *either* the social theory of discovery *or* the “heroic” theory. What Bacon sensed, others glimpsed a little more fully, without questioning the assumed opposition of these theories of discovery. And so for more than three centuries, there has been an intermittent mock battle between the advocates of the heroic theory and the theory of the social determination of discovery in science. In this conflict, truth has often been the major casualty. For want of an alternative theory, we have been condemned to repeat the false disjunction between the heroic theory centered on men of genius and the sociological theory centered on the social determination of scientific discovery.

### The Self-Exemplifying Hypothesis of Multiples

At the root of a sociological theory of the development of science is the strategic fact of the multiple and independent appearance of the same scientific discovery—what I shall, for convenience, hereafter describe as a multiple. Ever since 1922 American sociologists have properly associated the theory with William F. Ogburn and Dorothy S. Thomas, who did so much to establish it in sociological thought.<sup>20</sup> On the basis of their compilation of some 150 cases of independent discovery and invention, they concluded that the innovations became virtually inevitable as certain kinds of knowledge accumulated in the cultural heritage and as social developments directed the attention of investigators to particular problems.

Appropriately enough, this is an hypothesis confirmed by its own history. (Almost, as we shall see, it is a Shakespearean play within a play.) For this idea of the sociological significance of multiple independent discoveries and inventions has been periodically rediscovered over a span of centuries. Today I shall not reach back of the nineteenth century for

20. W. F. Ogburn and D. S. Thomas, “Are Inventions Inevitable?” *Political Science Quarterly* 37 (March 1922): 83–98; W. F. Ogburn, *Social Change* (New York: Heusch, 1922), pp. 90–122. On the same point, see chapters 10 and 17 of this volume.

cases. Let us begin, then, with 1828, when Macaulay, in his essay on Dryden, observes that the independent invention of the calculus by Newton and Leibniz belongs to a larger class of instances in which the same invention or discovery had been made by scientists working apart from one another. For example, Macaulay tells us that

the doctrine of rent, now universally received by political economists, was propounded, at almost the same moment, by two writers unconnected with each other. Preceding speculators had long been blundering round about it; and it could not possibly have been missed much longer by the most heedless inquirer.

And then he concludes, in truly Macaulayan prose and with the unmistakable Macaulayan flair:

We are inclined to think that, with respect to every great addition which has been made to the stock of human knowledge, the case has been similar: that without Copernicus we should have been Copernicans—that without Columbus America would have been discovered—that without Locke we should have possessed a just theory of the origin of human ideas.<sup>21</sup>

This is not the time to examine in detail the many occasions on which the fact of multiples with its implications for a theory of scientific development has been noted; on the evidence, often independently noted and set down in print. Working scientists, historians and sociologists of science, biographers, inventors, lawyers, engineers, anthropologists, Marxists and anti-Marxists, Comteans and anti-Comteans have time and again, though with varying degrees of perceptiveness, called attention both to the fact of multiples and to some of its implications. But perhaps a partial listing will bring out the diversity of occasions on which the fact and associated hypothesis of independent multiples in science and technology were themselves independently set forth:

- In 1828—as I have said, there was Macaulay, notably in his essay on Dryden;
- 1835—Auguste Comte, in his *Positive Philosophy*;
- 1846, 1847, and 1848—the mathematician and logician, Augustus de Morgan;
- 1855—Sir David Brewster, the physicist, editor of the *Edinburgh Encyclopedia*, and warmly appreciative though not always discriminating biographer of Newton, who was himself involved in several multiples in dioptrics with Malus and Fresnel;
- 1862–1864—when there was printed an entire cluster of observations upon multiples, growing out of the then-current controversy in England over the patent system, such that the *London Times* ran

21. *Miscellaneous Works of Lord Macaulay*, ed. Lady Trevelyan (New York: Harper, 1880), 1: 110–11.

- repeated leaders on the subject, remarking the common notoriety of the fact “that the progress of mechanical discovery is constantly marked by the simultaneous revelation to many minds of the same method of overcoming some practical difficulty” (13th September 1865);
- 1864—Samuel Smiles, that immensely popular Victorian biographer and apostle of self-help, repeatedly touched upon the fact of multiples;
- 1869—François Arago, the astronomer, physicist, biographer, and permanent secretary of the Academy of Sciences, made much of multiples;
- 1869—Francis Galton who, in his *Hereditary Genius*, considered “it notorious that the same discovery is frequently made simultaneously and quite independently, by different persons” as attested by famous cases in point during the few years preceding, and who returned to the same subject in 1874, in his *English Men of Science*;
- 1885—by the now little-known American anthropologists, Babcock and Pierce;
- 1894—Friedrich Engels, in his letter to Heinz Starkenburg, wrote of his partner in ideas that “while Marx discovered the materialist conception of history, Thierry, Mignet, Guizot, and all the English historians up to 1850 are the proof that it was being striven for, and the discovery of the same conception by Morgan proves that the time was ripe for it and that indeed it *had* to be discovered”;
- 1904—François Mentré, the French social philosopher and historian, whose basic paper, “La simultanéité des découvertes,” *Revue scientifique*, supplies a list of some 50 cases;
- 1905—Albert Venn Dicey, English jurist and political scientist in his magisterial *Lectures on the Relation between Law and Public Opinion*;
- 1906–1913—Pierre Duhem, the physico-chemist and one of the fathers of the modern history of science, who examines the fact and implications of multiples in every one of his major works;
- 1906—the distinguished German physiologist, Emil Du Bois-Reymond;
- 1913—the man who was to become the dean of American historians of science, George Sarton;
- 1917—the dean of American anthropologists, A. L. Kroeber;
- 1921—by Einstein; and then, as we near the formulation best known in the United States, in
- 1922—the fact and associated hypothesis of multiples as stated by the historian of science Abel Rey in France; by the then leading exponent of Marxist theory in Russia, Nicolai Bukharin; by the authoritative political scientist and essayist, Viscount Morley in England; and, of course, by Ogburn and Thomas in the United States.

The limits of time have required me to confine this partial list to the nineteenth century and the early twentieth. But this self-set rule must be breached at least once. For on this occasion, we can scarcely exclude the observations on the subject made by the chief founder of both the American Philosophical Society and the University of Pennsylvania. Of Franklin's several versions of the matter, I select one that bears his unmistakable imprint. Writing to the Abbé de la Roche, he remarks:

I have often noted, in reading the works of M. Helvétius, that, though we were born and brought up in two countries so remote from each other, we have often hit upon the same thoughts; and it is a reflection very flattering to me that we have loved the same studies and, so far as we have known them, the same friends, and the same woman.<sup>22</sup>

Here, as elsewhere, Franklin takes the occurrence of multiples as a matter of course.

Just so do most of the others in the truncated list of multiple discoveries of the theory of multiple discoveries. That many, indeed most, of them came upon the idea independently is at least suggested by the form in which they present it, as something they have found worthy of note. Its independence is suggested also by the interest which each succeeding formulation of the idea excited among those readers who happened to comment on it in print, either in book reviews or articles. The fact is that the theory was most unevenly diffused among scholars and scientists. By the middle of the nineteenth century, it had become, for some, a commonplace and often deplored truth; for others, it represented an entirely new conception of how science advances through the uneven accumulation of knowledge and through immanently or socially induced foci of attention to particular problems by many scientists at about the same time.

Further evidence that the idea—which, in a sense, has been “in the air” for about three centuries—was being independently rediscovered is also inadvertently supplied by those critics who attacked it as thoroughly unsound or at least as ideologically suspicious. Down to the present day, some authors can bring themselves to describe the hypothesis as essentially Marxist and so, we are invited to suppose, as necessarily false. That Marx was a precocious boy of ten when Macaulay first set down his ideas on the subject and a high-spirited youth of eighteen or so when Comte asserted the same ideas—the same Comte destined to be the butt of Marx's ire—all this would appear unknown to those critics who describe the theory of multiples as entirely Marxist. What the early Victorian writers of leaders for the *London Times* would have said of this description of the

22. Albert Henry Smyth, *The Writings of Benjamin Franklin* (New York: Macmillan, 1905-07), 7: 434-35.

hypothesis they put in print can unfortunately only be conjectured. In short, despite the many distinct occasions on which the theory of multiples was published, it has periodically emerged as an idea new to many observers who worked it out for themselves.

Even so, the fact of multiple discoveries in science continues to be regarded by some, including minds of a high order, as something surpassing strange and almost unexplainable. Here is the great pathologist and historian of medicine, William Henry Welch, on the subject:

The circumstances that a long-awaited discovery or invention has been made by more than one investigator, independently and almost simultaneously, and with varying approach to completeness, is a curious and not always explicable phenomenon familiar in the history of discovery.<sup>23</sup>

Other scholars tacitly assume that the pattern of multiples is both curious and distinctive of their own field of inquiry, if not entirely confined to it. As one example, consider the observation by the notable historian of geometry, Julian Lowell Coolidge:

It is a curious fact in the history of mathematics that discoveries of the greatest importance were made simultaneously by different men of genius.<sup>24</sup>

And recently, the sociologist Talcott Parsons is recorded as having described the threefold, or possibly fivefold, discovery of “the internalization of values and culture as part of the personality” as “a very remarkable phenomenon because all of these people were independent of each other and their discovery is . . . fundamental.”<sup>25</sup>

In part, of course, observations of this kind are merely casual remarks, not to be taken literally. But I should like now to develop the hypothesis that, far from being odd or curious or remarkable, the pattern of independent multiple discoveries in science is in principle the dominant pattern rather than a subsidiary one. It is the singletons—discoveries made only once in the history of science—that are the residual cases, requiring special explanation. Put even more sharply, the hypothesis states that all scientific discoveries are in principle multiples, including those that on the surface appear to be singletons.

## Evidence on the Hypothesis of Multiples

Stated in this extreme form, the hypothesis must at first sound extravagant,

23. William Henry Welch, *Papers and Addresses* (Baltimore: Johns Hopkins Press, 1920), 3: 229.

24. Julian Lowell Coolidge, *A History of Geometrical Methods* (Oxford: Clarendon Press, 1940), p. 122.

25. Talcott Parsons, in *Alpha Kappa Deltan: A Sociological Journal* 29 (Winter 1959): 3–12, at 9–10.

not to say incorrigible, removed from any possible test of competent evidence. For if even historically established singletons are declared to be multiples-in-principle—potential multiples that happened to emerge as singletons—it would seem that this is a self-sealing hypothesis, immune to investigation. And yet, it may be that things are not really as bad as all that.

An incorrigible hypothesis is, of course, not an hypothesis at all, but only a dogma or perhaps an incantation. I suggest, however, that, far from being incorrigible and therefore outrageous, this hypothesis of multiples is actually held much of the time by working scientists. The evidence for this is ready to hand and once its pertinence is seen, it can be gathered in abundance. Here, then, are ten kinds of related evidence that bears upon the hypothesis that discoveries in science are in principle multiples, with the singletons being the exceptional type requiring special explanation.

*First* is the class of discoveries long regarded as singletons that turn out to be rediscoveries of previously unpublished work. Cases of this kind abound. But here, I allude only to two notable instances: Cavendish and Gauss. Much of Cavendish's vast store of unpublished experiments and theories became progressively known only after his death in 1810, as Harcourt published some of his work in chemistry in 1839; Clerk Maxwell, his work in electricity in 1879; and Thorpe, his complete chemical and dynamical researches in 1921.<sup>26</sup> But in the meanwhile, many of Cavendish's unpublished discoveries were made independently by contemporary and later investigators, among them, Black, Priestley, John Robison, Charles, Dalton, Gay-Lussac, Faraday, Boscovich, Larmor, Pickering, to cite only a few. And in most cases, the rediscoveries were regarded as singletons until Cavendish's records were belatedly published. The case of Gauss, as we know, is much the same. Loath to rush into print, Gauss crowded his notebooks with mathematical inventions and other discoveries that turned up independently in work by Abel, Jacobi, Laplace, Galois, Dedekind, Franz Neumann, Grassmann, Hamilton, and others.<sup>27</sup> Again, presumed singletons turned out to be multiples, as once unpublished work became

26. The detailed cases of rediscovery can be garnered from G. Wilson, *The Life of the Hon. Henry Cavendish*, 2 vols. (London, 1851); Henry Cavendish, *Scientific Papers*, ed. from the published papers and the Cavendish manuscripts (Cambridge: At the University Press, 1921), vol. 1, *The Electrical Researches*, ed. J. Clerk Maxwell, rev. Sir Joseph Larmor, vol. 2, *Chemical and Dynamical*, ed. Sir Edward Thorpe and others; A. J. Berry, *Henry Cavendish: His Life and Scientific Work* (London, Hutchinson, 1960).

27. A preliminary list of such rediscoveries of Gauss' unpublished work has been compiled from the details in his voluminous letters—e.g., *Briefwechsel zwischen Gauss und Bessel* (Leipzig: Wilhelm Engelmann, 1889); *Briefwechsel zwischen Carl Friedrich Gauss und Wolfgang Bolyai* (Leipzig: Teubner, 1899)—and in Waldo G. Dunnington, *Carl Friedrich Gauss* (New York: Exposition Press, 1955). I shall return to the further implications of such repeated involvement of the same scientists in multiple discoveries later in this paper, when I propose a sociological concept of scientific genius.

known. Far from being exceptions, Cavendish and Gauss are instances of a larger class.

What holds for unpublished work often holds also for work which, though published, proved relatively neglected or inaccessible, owing either to its being at odds with prevailing conceptions, or its difficulty of apprehension, or its having been printed in little-known journals, and so on. Here, again, singletons become redefined as multiples when the earlier work is belatedly identified. In this class of cases, to choose among the most familiar, we need only recall Mendel and Gibbs. The case of Mendel<sup>28</sup> is too well known to need review; that of Gibbs almost as familiar, since Ostwald, in his preface to the German edition of the *Studies in Thermodynamics*, remarked, in effect, that "it is easier to re-discover Gibbs than to read him."<sup>29</sup>

These are all cases of seeming singletons which then turn out to have been multiples or rediscoveries. Other, more compelling, classes of evidence bear upon the apparently incorrigible hypothesis that singletons, rather than multiples, are the exception requiring distinctive explanation and that discoveries in science are, in principle, potential multiples. These next classes of evidence are all types of forestalled multiples, discoveries that are historically identified as singletons only because the public report of the discovery forestalled others from making it independently. These are the cases of which it can be said: There, but for the grace of swift diffusion, goes a multiple.<sup>30</sup>

*Second*, then, and in every one of the sciences, including the social sciences, there are reports in print stating that a scientist has discontinued an inquiry, well along toward completion, because a new publication has anticipated both his hypothesis and the design of inquiry into the hypothesis. The frequency of such instances cannot be firmly estimated, of course, but I can report having located many.

*Third*, and closely akin to the foregoing type, are the cases in which the scientist, though he is forestalled, goes ahead to report his original, albeit anticipated, work. We can all call to mind those countless footnotes in the literature of science that announce with chagrin: "Since completing this experiment, I find that Woodworth (or Bell or Minot, as the case may be) had arrived at this conclusion last year and that Jones did so fully sixty years ago." No doubt many of us here today have experienced one or more

28. See Hugo Iltis, *Life of Mendel* (New York: W. W. Norton, 1932); Conway Zirkle, "Gregor Mendel and His Precursors," *Isis* 42 (June 1951): 97-104.

29. This is the entirely apt paraphrase by Muriel Rukeyser in *Willard Gibbs* (New York: Doubleday Doran, 1942), 4: 314.

30. It is only appropriate that the original saying—"There, but for the grace of God, . . ."—should itself be, with minor variations, a repeatedly reinvented expression.



of these episodes in which we find that our best and, strictly speaking, our most original inquiries have been anticipated. On this assumption, I single out only one case in point:

The experience of Lord Kelvin as an undergraduate of 18, when he was still the untitled William Thomson, who sent his first paper on mathematics to the *Cambridge Journal* only to find that he "had been anticipated by M. Chasles, the eminent French geometrician in two points . . . [and] when the paper appeared some months later, prefixed a reference to M. Chasles' memoirs, and to another similar memoir by M. Sturm. Still later, Thomson discovered that the same theorems had been also stated and proved by Gauss; and, after all this, he found that these theorems had been discovered and fully published more than ten years previously by Green, whose scarce work he never saw till 1845."<sup>31</sup>

Far from being rare, these voyages of subsequent and repeated discovery of an entire array of multiples are frequent enough to be routine.

*Fourth*, these publicly recorded instances of forestalled multiples do not, of course, begin to exhaust the presumably great, perhaps vast, number of unrecorded instances. Many scientists cannot bring themselves to report in print that they were forestalled. These cases are ordinarily known only to a limited circle, closely familiar with the work of the forestalled scientists. Interview studies of communication among scientists have begun to identify the frequency of such ordinarily unknown forestalling of multiples. Systematic field studies of this kind have turned up large proportions of what is often described as "unnecessary duplication" in research resulting from imperfections in the channels of communication between contemporary scientists. One such study<sup>32</sup> of American and Canadian mathematicians, for example, found 31 percent of the more productive mathematicians reporting that delayed publication of the work of others had resulted in such "needless duplication," that is, in multiples.

*Fifth*, we find seeming singletons repeatedly turning out to be multiples, as friends, enemies, co-workers, teachers, students, and casual scientific acquaintances have reluctantly or avidly performed the service of a candid friend by acquainting an elated scientist with the fact that his original finding or idea is not the singleton he had every reason to suppose it to

31. Silvanus P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs* (London: Macmillan, 1910), 1: 44-45.

32. See Herbert Menzel, *Review of Studies in the Flow of Information Among Scientists*, Columbia University Bureau of Applied Social Research, a report prepared for the National Science Foundation (January 1960), 1: 21, 2: 48. Much other apposite information summarized in the Menzel monograph cannot be crowded into this paper. It should be added, however, that these data were uncovered in studies that were not focused on the matter of multiple and singleton discoveries; judging from the personal reports of previously undisclosed multiples that spontaneously came my way after I had published another paper on this general subject, I should judge that these occur on a scale so large that it has scarcely begun to be appreciated.

be, but rather a doubleton or larger multiple, with the result that this latest independent version of the discovery never found its way into print. So, the young W. R. Hamilton hits upon and develops an idea in optics and as he plaintively describes the episode:

A fortnight ago I believed that no writer had ever treated of Optics on a similar plan. But within that period, my tutor, the Reverend Mr. Boyton, has shown me in the College Library a beautiful memoir of Malus on the subject. . . . With respect to those results which are common to both, it is proper to state that I have arrived at them in my own researches before I was aware of his.<sup>33</sup>

What his tutor did for Hamilton, others have done for innumerable scientists through the years. The diaries, letters, and memoirs of scientists are crowded with cases of this pattern (and with accounts of how they variously responded to these carriers of bad news).

*Sixth*, the pattern of forestalled multiples emerges as part of the oral tradition rather than the written one in still another form: as part of lectures. Here again, one instance must stand for many. Consider only the famous lectures of Kelvin at the Johns Hopkins where, it is recorded, he enjoyed "the surprise of finding [from members of his audience] that some of the things he was newly discovering for himself had already been discovered and published by others."<sup>34</sup>

A *seventh* type of pattern, tending to convert potential multiples into singletons, so far as the formal historical record goes, occurs when scientists have been diverted from a clearly developed program of investigation which, from all indications, was pointed in the direction successfully taken up by others. It is, of course, conjecture that the discoveries actually made by others would in fact have been made by the first but diverted investigator. But consider how such a scientist as Sir Ronald Ross, persuaded that his discoveries of the malarial parasite and the host mosquito were only the beginning, reports his conviction that, but for the interference with his plan by the authorities who employed him, he would have gone on to the discoveries made by others:

The great treasure-house had been opened, but I was dragged away before I could handle the treasures. Scores of beautiful researches now lay open to me. I should have followed the "vermicule" in the mosquito's stomach—that was left to Robert Koch. I intended to mix the "germinal threads" with birds'

33. Robert Perceval Graves, *Life of Sir William Rowan Hamilton*, 3 vols. (Dublin: Hodges, Figgis, 1882), 1: 177.

34. Thompson, *William Thomson*, 2: 815–16. Kelvin tells of one such episode, thus: "I was thinking about this three days ago, and said to myself, 'There must be bright lines of reflexion from bodies in which we have those molecules that can produce intense absorption.' Speaking about this to Lord Rayleigh at breakfast, he informed me of this paper of Stokes's, and I looked and saw that what I had thought of was there. It was perfectly well known, but the molecule first discovered it to me."

blood—that was left to Schaudinn. I wished to complete the cycle of the human parasites—that was left to the Italians and others.<sup>35</sup>

Conjectural, to be sure, but with some indications that extraneous circumstances terminated a program of research that would have resulted in some of these discoveries becoming multiples rather than remaining adventitious singletons.

These several patterns of forestalled multiples, however, provide us with only sketchy evidence bearing on the apparently incorrigible hypothesis that multiples, both potential and actual, are the rule in scientific discovery and singletons the exception requiring special explanation. I turn now to evidence of quite another sort, the behavior of scientists themselves and the assumptions underlying that behavior. And here I suggest that, far from being outrageous, the hypothesis is in fact commonly adopted as a working assumption by scientists themselves. I suggest that in actual practice, scientists, and perhaps especially the greatest among them, themselves assume that singleton discoveries are imminent multiples. Granted that it is a difficult and unsure task to infer beliefs from behavior; almost as difficult and unsure as to infer behavior from beliefs. But in this case, we shall see that the behavior of scientists clearly testifies to their underlying belief that discoveries in science are potential multiples.

After all, scientists have cause to know that many discoveries are made independently. They not only know it, but act on it.<sup>36</sup> Since the culture of science puts a premium not only on originality but on chronological firsts in discovery, this awareness of multiples understandably activates a rush to ensure priority. Numerous expedients have been developed to ensure not being forestalled: for example, letters detailing one's new ideas or findings are dispatched to a potential rival, thus disarming him; preliminary reports are circulated; personal records of research are meticulously dated (as by Abel or Kelvin).

The race to be first in reporting a discovery testifies to the assumption that if the one scientist does not soon make the discovery, another will. This, then, provides an *eighth* kind of evidence bearing on our hypothesis [evidence set out in chapter 14 of this volume]. The many instances detailed there are quite typical; Norbert Wiener is no more circumstantial and outspoken about his experience than were Wallis, Wren, Huygens, Newton, the Bernoullis, and an indefinitely large number of other scientists through the centuries whose diaries, autobiographies, letters, and notes testify to the same effect.

35. Ronald Ross, *Memoirs, with a Full Account of the Great Malaria Problem and Its Solution* (London: John Murray, 1923), p. 313.

36. The following paragraphs are based on "Priorities in Scientific Discovery," chapter 14 of this volume.

In all this, I exclude those cases in which scientists move to establish their priority only to ensure that their discoveries not be diffused in the community of scientists before their own creative role in them is made eminently visible or to ensure that they not be later accused of having derived their own ideas from fellow-scientists who have borrowed them or cases in which, like that of Priestley, scientists publish quickly in order to advance science rapidly by making their work available to others at once. In this class of cases pertinent to the hypothesis, I refer only to those in which the rush to establish priority is avowedly motivated by the concern not to be forestalled, for this alone is competent evidence that scientists in fact assume that their initial singletons are destined not to remain singletons for long; that, in short, a multiple is definitely in the making.

But *ninth*, not all scientists who see themselves involved in a potential multiple are prepared to be outspoken about the matter. In many cases of this sort, their scientific colleagues, or kin, are. We have only to remember the elder Bolyai, himself a mathematician of some consequence, prophetically warning his son that “no time be lost in making it [his non-Euclidean geometry] public, for two reasons:

first, because ideas pass easily from one to another, who can anticipate its publication, and secondly, there is some truth in this, that many things have an epoch, in which they are found at the same time in several places, just as the violets appear on every side in spring. . . . Thus we ought to conquer when we are able, for the advantage is always to the first comer.”<sup>37</sup>

Almost we hear in these words the echoed warning by other faithful colleagues of the imminent danger of being forestalled: his friend Robinson urging Oughtred to make his work on logarithms public;<sup>38</sup> Wallis and Halley warning Newton;<sup>39</sup> Halley warning Flamsteed;<sup>40</sup> Bache warning Joseph Henry that “no time be lost in publishing his remarks before the American Philosophical Society” now that word has come of Faraday’s work on self-induction;<sup>41</sup> Lyell warning Darwin (Edward Blyth notwith-

37. The letter is quoted in Roberto Bonola, *Non-Euclidean Geometry* 2d rev. ed. (La Salle, Ill.: Open Court Publishing Co., 1938), pp. 98–99. See chapter 14 of this volume.

38. Stephen Peter Rigaud, ed., *Correspondence of Scientific Men of the 17th Century*, 2 vols. (Oxford: Oxford Univ. Press, 1841), 1: 7, 2: 27.

39. Charles R. Weld, *A History of the Royal Society* (London: Parker, 1848), 1: 408–9.

40. Francis Baily, *An Account of the Revd. John Flamsteed, the First Astronomer-Royal, Compiled from His Own Manuscripts* (London, 1835), p. 161. This case has particular point since Halley and Flamsteed were of course devoted enemies, but Halley thought it important that no English scientist be forestalled by a foreign scientist.

41. Thomas Coulson, *Joseph Henry: His Life and Work* (Princeton, N.J.: Princeton Univ. Press, 1950), pp. 109–10, 47–48.

standing) that he must publish lest he be forestalled;<sup>42</sup> Bessel and Schumacher warning Gauss that he will be anticipated (as he was) on every side;<sup>43</sup> the elderly Legendre warning the young Karl Jacobi that the younger Niels Abel would overtake him in the race for discoveries in the theory of elliptic functions unless “you take possession of that which belongs to you by letting your book appear at the earliest possible date.”<sup>44</sup>

Between them, Gauss and Bessel supply a beautifully ironic instance of how apt it is for scientists to assume that their original discoveries will be duplicated by others if they do not put them into print soon. For years on end the faithful Bessel has been haranguing Gauss to publish his new discoveries on pain of being forestalled. At last, Gauss behaves as Bessel would have him behave. He publishes a treatise on dioptrics and sends a copy to Bessel who, after heroically congratulating him on the work, ruefully reports that it thoroughly anticipates Bessel's own current but still unpublished investigations.<sup>45</sup>

Gauss supplies us with another striking instance of the scientist's or mathematician's firm belief that a discovery or invention is not reserved to himself alone. In 1795, at the ripe age of eighteen, he works out the method of least squares. To him the method seems to flow so directly from antecedent work that he is persuaded others must already have hit upon it; he is willing to bet, for example, that Tobias Mayer must have known it.<sup>46</sup> In this he was, of course, mistaken, as he learned later; his invention of least squares had not been anticipated. Nevertheless, he was abundantly right in principle: the invention was bound to be a multiple. As things turned out, it proved to be a quadruplet, with Legendre inventing it independently in 1805 before Gauss had got around to publishing it, and with Daniel Huber in Basel and Robert Adrain in the United States coming up with it a little later.<sup>47</sup>

There is a final and perhaps most decisive kind of evidence that the community of scientists does in fact assume that discoveries are potential multiples. This evidence is provided by the institutional expedients de-

42. Francis Darwin, ed., *The Life and Letters of Charles Darwin* (New York: Appleton, 1925), 1: 426–27, 473.

43. Dunnington, *Gauss*, p. 216; C. A. F. Peters, ed., *Briefwechsel zwischen C. F. Gauss und H. C. Schumacher* (Altona: Gustav Esch, 1860), 2: 82–83, 299–300, 3: 69, 75, 6: 10–11, 55.

44. Ore, *Niels Henrik Abel*, p. 203.

45. *Briefwechsel zwischen Gauss und Bessel*, Herausgegeben auf Veranlassung der Königlichen Preussischen Akademie der Wissenschaften (Leipzig: Wilhelm Engelmann, 1880), pp. 531–32.

46. *Briefwechsel zwischen Gauss und Schumacher*, 3:387.

47. Dunnington, *Gauss*, p. 19. Adrain, the outstanding American mathematician of his day, was involved in several multiples. See J. L. Coolidge, “Robert Adrain and the Beginning of American Mathematics,” *American Mathematical Monthly* 33 (Feb. 1926): 61–76.

signed to protect the scientist's priority of conception. Since the seventeenth century, scientific academies and societies have established the practice of having sealed and dated manuscripts deposited with them in order to protect both priority and idea. As this was described in the early minutes of the Royal Society:

When any fellow should have a philosophical notion or invention, not yet made out, and desire that the same sealed up in a box might be deposited with one of the secretaries, till it could be perfected, and so brought to light, this might be allowed for the better securing inventions to their authors.<sup>48</sup>

From at least the sixteenth century and as late as the nineteenth, it will also be remembered, discoveries were often reported in the form of anagrams—as with Galileo's "triple star" of Saturn and Hooke's law of tension—for the double purpose of establishing priority of conception and yet of not putting rivals on to one's original ideas, until they had been worked out further.<sup>49</sup> From the time of Newton, scientists have printed short abstracts for the same purpose.<sup>50</sup> These and comparable expedients all testify that scientists, even those who manifestly subscribe to the contrary opinion, in practice assume that discoveries are potential multiples and will remain singletons only if prompt action forestalls the later independent discovery. It would appear, then, that what might first have seemed to be an incorrigible, perhaps outrageous, hypothesis about multiples in science is in fact widely assumed by scientists themselves.

A great variety of evidence—I have here set out only ten related kinds—testifies, then, to the hypothesis that, once science has become institutionalized, and significant numbers are at work on scientific investigation, the same discoveries will be made independently more than once and that singletons can be conceived of as forestalled multiples.

### Patterns of Multiple Discoveries

Before turning to the last part of this paper—the part dealing with a sociological conception of the role of genius in the advancement of science—I think it useful to report some findings from a methodical study of multiple discoveries. Of the multitude of multiples, Dr. Elinor Barber and I have undertaken to examine 264 intensively. The greatest part of these—179 of them—are doublets; 51, triplets; 17, quadruplets; 6, quintuplets;

48. Thomas Birch, *The History of the Royal Society of London* (London: A. Millar, 1756), 2:30. The French Academy of Sciences made extensive use of this arrangement; among the many documents deposited under seal was Lavoisier's on combustion; see Lavoisier *Oeuvres de Lavoisier. Correspondance*, ed., René Fric (Paris: Michel, 1957), fasc. 2, pp. 388–89.

49. See chapter 14 of this volume.

50. See Birch, *History of the Royal Society* 4:437.

8, sextuplets. This aggregate of multiples also includes one septuplet and two nonaries, in which most of the nine independent co-discoverers were presumably ready to entertain the hypothesis that if any one of them had not arrived at the discovery, it would probably have been made in any case.

Each of these 264 multiples has been variously classified, after a search of the monographic evidence dealing with it. It has been classified in the particular discipline in which it occurred; the historical period of the multiple; the interval of time elapsing between the repeated discoveries; the number of co-discoverers; whether or not it gave rise to a contest over priority; the nationality of the co-discoverers, distinguishing those who were fellow nationals from the rest; the ages of the co-discoverers; and so on. The information about each multiple obtained through historical inquiry has been coded and transferred to punchcards, in this way permitting detailed statistical analysis.

This is not the occasion to report the findings in hand; my purpose here is only to suggest that the intensive study of particular cases of multiple discovery can be instructively supplemented by methodical analysis of large numbers of cases. It may be of interest, for example, that 20 percent of the multiples under review occurred within an interval of one year; some of them on the same day or within the same week. Another 18 percent occurred within a two-year span and, to turn to the other end of the scale, 34 percent of them involved an interval of ten years or more. The shorter the interval between the several appearances of a multiple, the *less* often does it lead to a debate over independence or other aspects of priority: of those made within a year of each other, just about half were subject to a contest over priority; of those more than 20 years apart, four in every five were contested. Ethnocentrism notwithstanding, if the independent co-discoverers are from different nations, there is slightly *less*, rather than more, probability of a conflict over priority. And to allude to just one other preliminary finding—this one, on the whole, rather encouraging—there seems to be a secular decline in the frequency with which multiples are an occasion for priority conflicts between scientists. Of the 36 multiples before 1700 which we have examined, 92 percent were strenuously contested; this figure drops to 72 percent in the eighteenth century; remains at about the same level (74 percent) in the first half of the nineteenth century and declines notably to 59 percent in the latter half; and reaches the low of 33 percent in the first half of this century. It may be that scientists are becoming more fully aware that, with growing numbers of investigators at work in each special field, any particular discovery is apt to be made by others as well as by themselves.

In any case, this inquiry has been enough to persuade us that the statistical analysis of historical data bearing on discovery is a feasible and instructive next step in the sociology of science.

### Sociological Theory of Genius In Science

After this short interlude, I return to the last part of the sociological theory of scientific development, dealing with the role of genius in that development. As I have intimated, the hypothesis of multiples has long been tied to the companion hypothesis that the great men of science, the undeniable geniuses, are altogether dispensable, for had they not lived, things would have turned out pretty much as they actually did. For generations the debate has waxed hot and heavy on this point. Scientists, philosophers, men of letters, historians, sociologists, and psychologists have all at one time or another taken a polemical position in the debate. Emerson and Carlyle, Spencer and William James, Ostwald and de Candolle, Galton and Cooley—these are only a few among the many who have placed the social theory in opposition to the theory that provides ample space for the individual of scientific genius. That so many acute minds should have for so long regarded this as an authentic debate must not keep us from noticing how the issues have been falsely drawn, and that once the two theories are clearly stated, there is no necessary opposition between them. Instead, it is proposed that once scientific genius is conceived of sociologically, rather than, as the practice has commonly been, psychologically, the two ideas of the environmental determination of discovery can be consolidated into a single theory. Far from being incompatible, the two complement one another.

In this enlarged sociological conception, scientists of genius are precisely those whose work in the end would be eventually rediscovered. These rediscoveries would be made not by a single scientist but by an entire corps of scientists. On this view, the individual of scientific genius is the functional equivalent of a considerable array of other scientists of varying degrees of talent. On this hypothesis, the undeniably large stature of great scientists remains acknowledged. It is not cut down to size in order to fit a Procrustean theory of the environmental determination of scientific discovery. At the same time, this enlarged conception does not abandon the sociological theory of discovery in order to provide for the indisputable, great differences between scientists of large talent and of small; it does *not*, in the phrase of Bacon, "place all wits and understandings nearly on a level."

This enlarged sociological conception holds that great scientists will have been repeatedly involved in multiples. First, because the genius will have made many scientific discoveries altogether; and since each of these is, on the first part of the theory, a potential multiple, some will have become actual multiples. Second, this means that each scientist of genius will have contributed the functional equivalent to the advancement of science of what a considerable number of other scientists will have



contributed in the aggregate, some of these having been caught up in the repeated multiples in which the genius was actually involved.

In a word, the greatest men of science have been involved in a multiplicity of multiples. This is true for Galileo and Newton, for Faraday and Clerk Maxwell, for Hooke, Cavendish, and Stensen, for Gauss and Laplace, for Lavoisier, Priestley, and Scheele—in short, for all those whose place in the pantheon of science is beyond dispute, however much they may differ in the measure of their genius.

Once again, I can only allude to the pertinent evidence rather than report it in full. But consider the case of Kelvin, by way of illustration. After examining some 400 of his 661 scientific communications and addresses—the rest have still to be studied—Dr. Elinor Barber and I find him testifying to at least 32 multiple discoveries in which he eventually found that his independent discoveries had also been made by others. These 32 multiples involved an aggregate of 30 other scientists, some, like Stokes, Green, Helmholtz, Cavendish, Clausius, Poincaré, Rayleigh, themselves men of undeniable genius, others, like Hankel, Pfaff, Homer Lane, Varley and Lamé being men of talent, no doubt, but still not of the highest order. The great majority of these multiples of Kelvin were doublets, but some were triplets and a few, quadruplets. For the hypothesis that each of these discoveries was destined to find expression, even if the genius of Kelvin had not obtained, there is the best of traditional proof: each was in fact made by others. Yet Kelvin's stature as a scientist remains undiminished. For it required a considerable number of others to duplicate these thirty-two discoveries which Kelvin himself made.

Following out the logic of this kind of fact, we can set up a matrix of multiple discoveries, with the entries in the matrix indicating the particular scientists involved in each of the multiples. Some of these others are themselves men of genius, in turn often involved in still other multiples. Others in the matrix are the men of somewhat less talent who, on the average, are involved in fewer multiples. And toward the lower end of the scale of demonstrated scientific talent are the far more numerous men of science, who in the aggregate are indispensable to the advancement of science and whose one moment of prime achievement came when they found for themselves one of the many discoveries that the man of genius had made independently of them.

To continue for a moment with the specimen case of Kelvin, these 32 multiples are of course only a portion of the multiples in which he was eventually involved. For, as I have said, they are only the ones which Kelvin himself found to have been made by others. Beyond these are the discoveries by Kelvin which were only later made independently by others. Of these we do not yet have a firm estimate. And beyond these still are what I have described as the forestalled multiples: the discoveries of

Kelvin which were not, so far as the record shows, made independently by others but which, on our hypothesis, would have been made had it not been for the widespread circulation of Kelvin's prior findings. Yet, even on this incomplete showing, it would seem that this one man of scientific genius was, in a reasonably exact sense, functionally equivalent to a sizable number of other scientists. And still, by the same token, his individual accomplishments in science remain undiminished when we note that he was not individually indispensable for these discoveries (since they were in fact made by others). This is the sense in which an enlarged sociological theory can take account both of the environmental determination of discovery while still providing for great variability in the intellectual stature of individual scientists.

Just a few words about another like instance, in quite another field of science. Whatever else may be said about Sigmund Freud, he is undeniably the prime creator of psychoanalysis. And still, only a first examination of about a hundred of his publications finds him reporting that he was involved in an aggregate of more than thirty multiples, discoveries which he made all unknowing that they had been made by others. Once again, the pattern is much like that we found for Kelvin. Some of Freud's subsequently discovered anticipators were themselves minds of acknowledged highest order: Schiller, von Hartmann, Schopenhauer, Fechner. But many of the rest of his independent co-discoverers or anticipators are scarcely apt to be known to most of us as distinguished for the highest quality of scientific achievement; men such as Watkiss Lloyd, Kutschin, E. Hacker, Grasset, Neufeld, and so on and on. It required a Freud to achieve individually what a large number of others achieved severally; it required a Freud to focus the attention of many on ideas which might otherwise not have come to their notice; in these and kindred aspects lay his genius. But that he was not individually indispensable to the intellectual developments for which he, more than any other, was historically responsible is indicated by the many multiples in which he was in fact engaged and the many others which, presumably, he forestalled by his individually incomparable genius.

What has been found to hold for Kelvin and Freud is being found to hold for other scientists of the first rank who are now being examined in the light of the theory. They are all scientists of multiple multiples; their undeniable stature rests in doing individually what must otherwise be done and, as we have reason to infer, at a much slower pace, by a substantial number of other scientists, themselves of varying degrees of demonstrated talent. The sociological theory of scientific discovery has no need, therefore, to retain the false disjunction between the cumulative development of science and the distinctive role of the scientific genius.

There is perhaps time for a few needed and self-imposed caveats. For I cannot escape the uneasy sense that this short though, you will grant me, not entirely succinct, summary of masses of data on scientific discovery must lend itself to misunderstanding. This is so, if only because so much has unavoidably been left unsaid. As a preventive to such misunderstanding, may I conclude by listing some *seeming* implications which are anything but implicit in what I have managed to report?

First, in presenting this modified version of a three-century-old conception of the course of scientific discovery, I do not imply that all discoveries are inevitable in the sense that, come what may, they will be made, at the time and the place, if not by the individual(s) who in fact made them. Quite the contrary: there are, of course, cases of scientific discoveries which could have been made generations, even centuries, before they were actually made, in the sense that the principal ingredients of these discoveries were long present in the culture. This recurrent fact of long-delayed discovery raises distinctive problems for the theory advanced here, but these are not unsolvable problems.

Second, and perhaps contrary to the impression I have given, the theory rejects the pointless practice of what I have called "adumbrationism," that is, the practice of claiming to find dim anticipations of current scientific discoveries in older, and preferably ancient, work by the expedient of excessively liberal interpretations of what is being said now and of what was said then. The theory is not a twentieth-century version of the seventeenth- and eighteenth-century quarrel between the ancients and the moderns.

Third, the theory is not another version of Ecclesiastes, holding that "there is no new thing under the sun." The theory provides for the growth, differentiation, and development of science just as it allows for the fact that new increments in science are in principle or in fact repeated increments. It allows also for occasional mutations in scientific theory which are significantly new even though they are introduced by more than one scientist.

Fourth, the theory does not hold that to be truly independent, multiples must be chronologically simultaneous. This is only the limiting case. Even discoveries far removed from one another in calendrical time may be instructively construed as "simultaneous" or nearly so in social and cultural time, depending upon the accumulated state of knowledge in the several cultures and the structures of the several societies in which they appear.

Fifth, the theory allows for differences in the probability of actual, rather than potential multiples according to the character of the particular discovery. Discoveries in science are of course not all of a piece. Some flow

directly from antecedent knowledge in the sense that they are widely visible implications of what has gone just before. Other discoveries involve more of a leap from antecedent knowledge, and these are perhaps less apt to be actual multiples. But it is suggested that, in the end, these too manifest the same processes of scientific development as the others.

Sixth, and above all, the theory rejects the false disjunction between the social determination of scientific discovery and the role of the genius or "great man" in science. By conceiving scientific genius sociologically, as one who in his own person represents the functional equivalent of a number and variety of often lesser talents, the theory maintains that the genius plays a distinctive role in advancing science, often accelerating its rate of development and sometimes, by the excess of authority attributed to him, slowing further development.

Seventh and finally, the diverse implications of the theory are subject to methodical investigation. The basic materials for such study can be drawn from both historical evidence and from field inquiry into the experience of contemporary scientists. What Bacon obliquely noticed and many others recurrently examined can become a major focus in the contemporary sociology of science.