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A Definition of Science as an Antidote Against Either Mystifying or Demistifying Science

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A DEFINITION OF SCIENCE AS AN ANTIDOTE AGAINST EITHER MYSTIFYING OR DEMYSTIFYING SCIENCE*

CHAPTER I. SCIENCE: STANDARDS AND DEFINITION

I.1 *Defining Science and Theorizing Science History*

It seems only natural that a work that claims to identify epistemological uniformities in the historical formation of every branch of the exact science should have it own definition of science; the identified theoretical elements would conceivably enter into the limitations and/or the potential of science. Nevertheless, the logical connection between the two sides, the theory and the definition, was not inevitable. The formulation of an intelligible axiomatic theory of knowledge for science. I believe, could have been accomplished at this stage without defining science innovatively. The role of axiomatization in intellectual history has impressed me, with ever increasing distinctness, for almost thirty years, whereas all along I was under the vague impression that science had already been properly defined. Now with a complete confidence in

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* *Editor's note*: Since the author had submitted to "Organon" the manuscript of an essay with this title, he has substantially re-written it, and actually has cast it into the first two chapters of a forthcoming book *Science, It History: A History of Axiomatization.* As the author has decided not to publish his original paper, what appears in the following is an essay composed of two chapters, and some of the wording in them indicates that more chapters are to come—though not necessarily in "Organon".

Author's note: This work began as a brief paper that I had prepared for a presentation at the XVIIth International Congress of History of Science (Berkeley, August 1985). My attendance at the Congress was generously financed by the Rackham Graduate School of the University of Michigan. I am very grateful to this great University especially for the academic guidance I have received through its eminent professors. For this essay of mine, I must mention Professor Nicholas Steneck of History Department and Professors Larry Sklar and Donald Munro of Philosophy Department.

the idea that axiomatization has been the methodological guide as well as the epistemological goal in the development of scientific theories, I am constantly puzzled by the question: Why a recurrent and significant phenomenon—that every branch of physical science ended in an axiomatic system and attained this status by successive axiomatization—has not been dealt with seriously long ago? As soon as I thought about a definition, my theoretical conviction started to function, and finally left its imprint there.

I am aware thet the act of looking for a definition—such as trying to define science in the past, present, or future—is itself a temporal affair. Although definers all want to arrive at a "timeless" formulation, a definition today will be replaced tomorrow. The deficiencies or flaws of an outmoded definition will be disclosed, and the expose can be occassioned by a more exact and comprehensive theoretical understanding. But it can also be motivated otherwise. An essay that now serves as the basis of the first two chapters of this book was originally written for the defence of science[1]. As I am incorporating a definition with a set of theories, it is evident that the two will strengthen each other. The specified nature of the subject-matters on the side of the definition and the emphasis of measurements on the side of the theory are logically complementary. So are the criteria of science and axiomatization.

In the following section I will list four reasons that impels me to seek a new definition of science; but, as I have just indicated, these reasons themselves do not imply the necessity of a theory of axiomatization.

I.2 *Why is a New Definition of Science Needed Now?*

The first reason is that the unsettled relationship between science and technology needs to be formalized. Many scholars say that the two are conceptually different. Yet in myriad contexts they are mentioned together—as if nowadays we have a new term "science-and-technology". In a new definition of science, technology will be naturally incorporated. There is no reason to overemphasize the theoretical, explanatory aspect of science. Theoretical depth is always relative, whereas the methodology of trial-and-error is universal—in theoretical science as well as in technology. On the other hand, we should not let the definition of science be engulfed by the definition of technology, such as J. Bronowski once did. In one of his writings he defined science this way[2].

> Science is a great many things, and I have called it a great many names; but in the end they all return to this: science is the acceptance of what works and the rejection of what does not.

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[1] Thanks to my presentation at the XVIIth International Congress of History of Science (Berkeley, August 1985), Dr Bogdan Suchodolski, editor of *Organon*, a multilnigual yearbook published by the Polish Academy of Science, invited me to contribute a paper. The outcome was my essay "A Definition of Science as an Antidote: Against Either Mystifying Or Demystifying Science," which in turn became the first two chapters here.

[2] J. Bronowski, *The Common Sense of Science* (Harvad University Press, Cambridge, Mass., 1978), p. 148.

Technology cannot incorporate science; the latter encompasses more than the former.

The second reason is the question of "admitting" fields of study into science. For more than a century a number of disciplines—meteorology, tidology (J.S. Mill's term, the study of tides), psychology, economics, sociology, etc.—have been striving to attain, or have succeeded in attaining, the status of science. Has science accepted them? Or, should science accept them? If it has (or should), certainly admittance is not conditionless. Then what are the conditions?

Thirdly, are mathematics and logic science? About three decades ago, students in China were still told that they were not. It was a Western idea, a tradition that traces back to Plato, or even the Pythagoreans, when astronomy and "music" (or "harmony," the study of the scale) were "mathematics", but not science. As late as in the 1920s, N. Campbell would write[3]. "Today we distinguish many branches of learning—mathematics, science, philosophy, history, and so on". But since then, obviously mathematics has gradually stepped into the temple of science and securely occupied its lawful niche. An updated definition of science should explain this historical change.

Fourthly, since the 1950s there has been, I believe, an identifiable ideological movement, of which several components work in a number of ways (yet probably not purposively) to compromise the integrity of science. I admit that this issue was a major concern of mine. Although nowadays most working scientists, modern professional hermits, are hardly influenced by philosophical debates and trends, it is only fair that the movement that imports the message that "science is not all that scientific" be somehow counter-balanced.

In sum, the idea of science should be consolidated (reasons one and three), broadened (reasons one, two, and three), and defended (reason four). It would be a worthy experiment to see if any theoretical design could fulfill these purposes. Thus far my conclusion is that, with a new definition, the idea of science itself is at once "tightened" and "loosened". According to this definition, a set of clear-cut standards will be maintained for science; at the same time, since these standards—briefly, the collection of factual data and the finding of prescient rules—are approachable by degree, any "candidate" branch of learning is "encouraged" to work toward them. In other words, we are now equipped with a dichotomy: the clear-cut formulation of scientific criteria and an open and flexible "membership policy".

I.3 *A Current Ideological Movement that Compromises Science*

This section, including a number of subsections, discusses the fourth reason mentioned above, i.e., the ideological movement and my rebuttal of some of its components that I think are misleading. I use the word "ideological" to describe the current movement, because I believe that there exists a coherent

[3] N. Campbell, *What is Science?* (Dover, NY, 1921; 1952, 2nd edition), p. 7.

body of ideas that more or less reflects consistent socio-political aspirations. The movement is characterized by an intensive interest in various non--scientific, or externalist, aspects of science. Obviously, this intensive interest was originated and is sustained by the increasing importance and pervasiveness of science and technology in the post-World War II world. As a result, during the past three decades, a rich literature in the history, philosophy, and social study of science, has been accumulated. In studying this literature, focusing on the history of both Western and Chinese science, gradually I have come to the conjecture that a number of claims, theories, advocacies, warnings, re-evaluations, etc. that have been set forth in various non-scientific studies of science are not independent from each other. Whether they are interdependent or not, or whether any one could prove the interdependency or not, are actually questions beyond the scope of this work. One common theme that I have sensed in a considerable part of these works of historical and philosophical scholarship could be summarized in a simple statement that I have already mentioned: Science is not all that scientific.

I.3.a *Components of the Movement that Shares a Common Theme*

The following is a list of the items the I have identified as components of a hypothetical movement. Let me put them in a compendious format, without paying attention to their completeness. Except for the first two, I will elaborate my views or quote other authors to confront the rest of the items[4]—approximately in a reversed order. The components of the movement include: 1) concern about the misuse of science, 2) fear of a total or massive destruction through human conflict using science-and-technology, 3) prophecy of the convergence of science, religion, horoscopy, etc., 4) assumption of certain adventitiousness in the development of exact sciences, 5) mystification of modern physics by identifying its philosophy with so-called oriental mysticism, 6) "demystification" of science by pointing out the analogy or comparability between modern science and primitive thoughts as well as by other considerations, 7) exaggeration of the role of personality in scientific creations, 8) advocacy of a vaguely perceived unity between science and the humanities, 9) assertion of a glorius scientific past among non-Western civilizations, 10) denial of the universality of science, and so on.

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[4] While I was rewriting this chapter, early in 1987, my wife, Marilyn, showed me *Popper and After: Four Modern Irrationalists* (Pergamon Press, 1982) by David Stove. Let me quote a passage from Stove's Preface: "This book is about a recent tendency in the philosophy of science... These authors' philosophy of science is in substance irrationalist. They doubt, or deny outright, that there can be any reason to believe any scientific theory; and *a fortiori* they doubt or deny, for example, that there has been any accumulation of knowledge in recent centuries." On the other hand, books that could serve as an easy target for a critic like David Stove are still being produced. For instance, a 1986 publication, Richard Jones, *Science and Mysticism* (Associated University Press), aims at "a reconciliation of science and mysticism."

To some of these components, especially when they are developed to the extreme, I am critical. Certainly I do not oppose the movement indiscriminately. In the following subsections I will cite several authors in order to characterize the movement, hence to show the necessity of clarifying the idea of science. Historians and philosophers of science with whose views I agree will also be quoted as a contrast to those who I think are in error.

I.3.b *Flat Denial of the Universality of Science*

Many earlier authors and many definitions of science affirm the idea that science is universal. Here, let us have. A. Wolf's view, the first and second editions of whose *History of Science, Technology, and Philosophy* were published respectively in 1935 and 1950[5]:

> It was not a recoil from rationality, but a big stride towards a freer and fuller rationality, unrestrained by arbitrary barriers. That is why science is universal, whereas the Churches are not. Science imposes no arbitrary restrictions on the reasoning by which it is cultivated; but the Churches usually confine the scope of reason within arbitrary boundaries of their several creeds or dogmas.

Nowadays, however, we sometimes hear clamorous statements that assert the opposite. As far as I know, Nathan Sivin is a recent representative of this position. In 1982 Sivin published a paper which comprises two major theses, one being the flat denial of the universality of science and the other the assertion of a "scientific revolution" in 17th century China. In a space of about two pages, Sivin repeatedly states that although modern science has been internationally accepted, it is not universal[6]:

> Finally there is the assumption that, since modern science has so quickly and thoroughly become international, it transcends European historical and philosophic biases, and is as universal, objective, and value-free as the Nature that it seeks to understand and manipulate.
> What seems to be common sense in that last assumption [...] does not stand up to thoughtful examination. Modern science is still too marked by the special circumstances of its development in Europe to be considered universal.
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> ......
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> Science and technology have spread throughout the world, but that has not made them universal, in the sense of transcending European patterns of thought...
> I am arguing that the notion of a universal and value-free modern science, which has somehow become independent of its social and historical origins, is wishful thinking.

To argue that modern science lacks universality, Sivin is challenging a well-established and popular conviction—as it will be made clear when we

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[5] A. Wolf, *A History of Science, Technology, and Philosophy in the 16th and 17th Centuries* (Allen and Unwin, London, 1950), p. 3.

[6] N. Sivin, "Why the Scientific Revolution Did Not Take Place in China—Or Didn't It?", *Chinese Science* 5 (June 1982), pp. 52—53. This paper was first delivered as the Edward H. Hume Lecture at Yale University, and then published in Sivin's own *Chinese Science* which "is not a periodical. Issues are published only when enough excellent contributions become available". After that the same paper was published in a festshrift dedicated to J. Needham's eightieth birthday.

survey definitions of science in the past. How does he do it? He tells us that modern science bears a European mark. How does he arrive at this conclusion? Let us quote him fully again—in dealing with grave issues such as this one, we can never be overly careful[7].

> Chinese science got along without dichotomies between mind and body, objective and subjective, even wave and particle [...] In the West the first two dichotomies were entrenched in scientific thought by the time of Plato. Galileo, Descartes, and others carried them into modern times to mark off the realm of physical science from the province of the soul, which was decidedly off limits to secular innovators like themselves. These distinctions let scientists claim authority over the physical world on the ground that purely natural knowledge could not conflict with the authority of established religion.
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> Science and religion have long since learned to coexist, but we are still living with these distinctions. If they are European peculiarities, and perpetual sources of troubles at that, why hasn't modern science managed to rid itself of them? It is evidently not a simple matter to root them out. Until we do, there is something to be said for frankly admitting a certain parochialism in the foundations of science. The mathematical equations may be universal, but the allocation of human effort among the possibilities of natural knowledge is not.

In order to grasp his logic—for many, including me, it is hopeless to try to follow his contents—let me distill the above passage into these points: 1) Chinese science did not have those dichotomies; 2) Western science had them; 3) Galileo et al. used these dichotomies to assert the independence of science from religion; 4) In the West, for a long time, science existed alongside religion, with the awareness of these dichotomies; 5) These dichotomies are "perpetual sources of trouble"; 6) Until they are rooted out, Western science has to admit a certain parochialism; 7) "The mathematical equations may be universal..." Scientific universality is finally and hesitatingly acknowledged. But this act put Sivin in a dilemma. He "solves" it by coupling his reluctant confession with a reminder of a non-universal aspect of science (its developmental priority)—as if the reader should thus be persuaded of the non-universal character of science itself.

The truth of almost all these eight statements contained in the quotation from Sivin is questionable. The significance of these statements in the history of science is uncertain. The relevance of these statements to the issue whether science is universally true or not is definitely untenable. Being universally true means passing tests: empirical, logical, impersonal, and repeatable tests. Therefore, universal truth was an outcome of comparisons in terms of quantitative conformity, logical persuasiveness, physical plausibility, and axiomatic compactness and neatness. With this set of criteria, scientific universality is confirmed by a consensus of contemporary scientists, or a tendency toward consensus over successive generations of scientists, and among scientists of foreign cultural backgrounds.

One implied assumption in Sivin's reasoning is that "Chinese science" is a valid science: "Chinese science" did not have it; "Western science" had it; both

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[7] *Ibid.*, p. 52.

were valid "sciences"; therefore, none could claim universality. But the implied assumption is false. If traditional Chinese science is valid science, or were as equally, or nearly, valid as the "Western science" that brought forth the internationally accepted modern science (a fact Sivin does not deny), the whole modern world history would be completely different. (I call this simple and valid way of argument "macrohistorical[8].)

Finally, Sivin's statement that in the West science and religion have long since learned to coexist can be compared with A. Wolf's passage on the intellectual contradiction between science and religion. No one denies that religion is not universal. Wolf stresses the fundamental difference between religion and science, and asserts the universality of the latter. Sivin tells about the coexistence of religion and science in the West, to imply the lack of universality of Western science.

In the same context, Sivin also rejects the universality of technology:[9]

> [...] its [modern technology's] strength emerges in application to needs and expectations that do not exist until it generates them. True universality would require modern technology to coexist with and serve cultural diversity rather than consistently serving as a tool to standardize it out of existence.

I would not characterize electricity, running water, and numerous other things as needs and expectations that had not existed until modern technology created them. One of my observations of the history of science-and-technology, which will be discussed in detail in Chapter Two, is the "principle of quasi-deterministic development," which states that the paths of progress are severely restricted by the intrinsic logic of natural matters: development does not enjoy much freedom in making choices among alternatives. When people demand better lighting and clean and convenient water, electric bulbs and faucets are the necessary technical breakthroughs to satisfy the needs at appropriate stages. With regard to the issue of technology being universal or not, however, the distinctions between "natural" needs and created expectations are, again, irrelevant.

In the quoted passage we notice that Sivin's conditions for a truly universal modern technology are extremely demanding. This supposed modern technology should be welcomed by all types of cultures, and adopted by them for coexistence. If one replaces the other, either way, that modern technology is not universal. (It is too bad that he did not specify in what "proportion" should the two, traditional culture and modern technology, coexist.)

But, I believe, this criterion of a "true" universal technology does not make sense for the majority of the people. Technology, as one of the tools of economy, is in the first place viewed and judged with economic criteria. It is universal, not only because it follows natural laws in its operations, but also

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[8] Wen-yuan Qian, *The Great Inertia: Scientific Stagnation in Traditional China* (Croom-Helm, London, 1985).

[9] *Ibid.*, p. 53.

because its recognized economic efficiencies, which are numbers, as universal as possible. No one can deny that people generally—universally—prefer higher economic efficiencies. So, our conclusion is just the opposite as Sivin's. The more consistently and readily a modern technology standardize cultural diversity out of existence, the more (relative) universal advantage this modern technology must possess. Not being able to distinguish technology, which operates with natural agencies and functions as a tool of economy, and cultural preference and ethical judgment, which are in the final analysis individual, I believe, was the cause of Sivin's false logic and flimsy rhetoric. As a contrast to Sivin's idea, let me quote John K. Fairbank's remark which bears similar wording but opposite ideas:[10]

> The flow of scientific technology outward from Europe has now penetrated all national cultures. The forces of technology and culture today buffet all peoples. Technology, being international, acts upon culture, which is national, to shape each people's modern way of life...

In this context the word "international" is a synonym of "universal." The interaction between imported new technology and native culture is here succinctly delineated. All possibilities in consequence of the interaction are open, a situation that does not reactively (illogically) influence the universality of technology itself.

I.3.c *Logical Troubles caused by the "Chinese Scientific Revolution"*

Another thesis of Sivin's paper and its adjunct folly are reflected in the title of his paper, "Why the Scientific Revolution Did Not Take Place in China—Or Didn't It?" By the question, "Or Didn't It?" he implies in the title, and then states in the text, that there occurred a "scientific revolution in 17th-century China". In China questions such as "Why didn't Chinese beat Europeans to the Scientific Revolution?"[11] would only be raised by young pupils who more or less share the mentality of the 1900 Boxers. Yet Needham and Sivin take these – self-invented, in a large extent – questions seriously, and spin out fantastic discourses on the basis of them. Finally Sivin was carried away to the extent to make such an amazing statement. It is paradoxical: while denying the universality of Western science, he wants to import the idea of the Scientific Revolution, a newly identified period of Western history, into Chinese history. Moreover, according to Sivin, it is "By conventional intellectual criteria, China had its own scientific revolution in the 17th century."[12] What conventional criteria are these – Chinese or Western? Apparently he meant Western, because soon after this sentence he continues:

> Western mathematics and mathematical astronomy were introduced to China beginning around 1630—in a form that before long would be obsolete in those parts of Europe where readers

[10] J.K. Fairbank, "Preface" to *The Great Inertia*, note 8 above.
[11] N. Sivin, note 6 above, p. 62.
[12] *Ibid.*, p. 62.

were permitted access to current knowledge (in post-Galilean Italy they were not). Several Chinese scholars, among them Mei Wen-ting (1633—1721), Hsueh Feng-tso (ca. 1620—80), and Wang Hsi-shan (1628—82), quickly responded and began reshaping the way astronomy was done in China. They radically and permanently reoriented the sense of how one goes about comprehending the celestial motions. They changed the sense of which concepts, tools, and methods are centrally important, so that geometry and trigonometry largely replaced traditional numerical or algebraic procedures. Such issues as the absolute sense of rotation of a planet and its relative distance from the earth became important for the first time. Chinese astronomers came to believe for the first time that mathematical models can explain the phenomena as well as predict them. These changes amount to a conceptual revolution in astronomy.

So, this "Chinese astronomical revolution"—like the Copernican Revolution in the West, it occupies a pivotal position in the alleged "Chinese scientific revolution" (Sivin's another unavoidable, probably unconsicous, Western parallel)—was nothing other than some results of learning from limited Western sources ("the Jesuiits were obliged to conceal from the Chinese that development in Europe"[13]). Besides, the same paper also acknowledges that "the predictive superiority of the European techniques was acknowledged" in China.[14] If "predictive superiority" did not enjoy a universal appeal, why should 17th-century Chinese astronomers learn and respect it? The quoted paper is indeed a jumble of erroneous statements, empty arguments, conflicting views, and messy logic.

In order to assess the comparison between China and the West in terms of the possibility of generating modern exact sciences, besides familiarity with history of the two sides, one needs to be acquainted with the essentials of exact sciences. One should appreciate the long and arduous preparation the West had made in order to stage the Scientific Revolution; one also should recognize the simple fact that in China there persisted a relative scantiness in the interest of studying natural phenomena—for whatever externalist reasons. The Latin West, instead, had a briliant, though for a long time almost inaccessible, heritage in Greek and Hellenistic science and philosophy; a continuous, though once dwindling for centuries, interest in nature, logic, and mathematics; interactions with the neighboring Arabic civilization who was then awakened to and fascinated by the glory of the Greek heritage; the historical condition that sustained the herculean and enduring efforts to recover the heritage; the critical and dialectical ability that was nurtured in the historical environment; and the flowering of an irrepressible creativity that finally paved the irreversible course for the self-motivated modern exact science.

Apparently Sivin has also been hampered by his peculiar attitude toward comparative studies. In the same article which is now under my scathing review he states:[15]

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[13] *Ibid.*, p. 62.
[14] *Ibid.*, p. 63.
[15] *Ibid.*, p. 46.

It also seems to me that comparing all of the scientific and engineering activity of one civilization with all that of another conceals more than it reveals, since it is only in modern times that these various kinds of work became closely connected.

Again, here the argument is as unintelligible as the conclusion itself.

I.3.d *More Contrasting Views*

As a philosophical issue, the universality, or objectivity, of science has ist own history. We need not go back too far. By the turn of the century, two great mathematicians and philosophers of science, Henri Poincaré (1854—1912) and Bertrand Russell (1872—1970), respectively championed two contradistinctive views. I will quote one representative passage from each. In retrospect, it is surprising to see that Russell would have made an erroneous statement in such a comprehensive and absolute form. He was talking about projective geometry:[16]

It takes nothing from experience, and has, like arithmetic, a creature of the pure intellect for its object. It deals with an object whose properties are logically deduced from its definition, not empirically discovered from data.

We will come back to this when we discuss the question of mathematics and logic being science. Whereas with these sentences Russell has said that some branches of mathematics had no empirical roots at all and were created by mathematician's intellect, pure and simple, with many paragraphs such as the following quoted, Poincaré staunchly defended the objectivity of science:[17]

Some people have exaggerated the role of convention in science; they have even gone so far as to say that law, that scientific fact itself, was created by the scientist [...] No, scientific laws are not artificial creations; we have no reason to regard them as accidental...

Next I want to compare two writers whose views on the impartiality of scientific creation differ drastically from each other. James B. Conant published *On Understanding Science* in 1947, in which he conveys a number of historical insights. For instance, he is admirably explicit about the building up of scientific standard through history:[18]

But if I read the history of science in the 17th and 18th centuries rightly, it was only gradually that there evolved the idea that a scientific investigator must impose on himself a rigorous self-discipline the moment he enters his laboratory. As each new generation saw how the prejudice and vanity of their predecessors proved stumbling blocks to progress, standards of exactness and impartiality were raised.

As a result of this historical process, science began to impose on its pursuers

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[16] H. Poincaré, translator G. B. Halstad, *The Foundations of Science* (The Science Press, Lancaster, PA, 1946), p. 201.

[17] *Ibid.*, pp. 208—09.

[18] J. B. Conant, *On Understanding Science* (Yale University Press, New Haven, 1947), pp. 6—7.

an impersonal, indeed institutionalized, standard of truthfulness, standard that even exert corrective actions over personal idiosyncracies:[19]

> Would it be too much to say that in the natural sciences today the given social environment has made it very easy for even an emotionally unstable person to be exact and impartial in his laboratory? The tradition he inherits, his instruments, the high degree of specialization, the crowds of witnesses that surround him, so to speak (if he publishes his results)—these all expert pressures that make impartiality on matters of his science almost automatic. Let him deviate from the rigorous role of impartial experimenter or observer at his own peril; he knows all too well what a fool So-and-so made of himself by blindly sticking to a set of observations or a theory now clearly recognized to be in error. But once he closes the laboratory door behind him, he can indulge his fancy all he pleases.

Published about a dozen years later, J. Bronowski's writings give us a very different view on science and scientific creativity. There is no doubt that Bronowski was conversant in both science and the humanities. His ambition was to establish a natural philosophy for the 20th century. He is now best remembered as a popularizer. He knew the rigor of science; but it often seems that he was only forced to admit it. What fascinated him was the unity of science and the arts. Yet in stressing the unity he sacrificed both the clarity of his arguments and the objectivity of science. To illustrate this point let us use his pamphlet *The Abacus and the Rose: A New Dialogue on Two World Systems,* a pompous work that was claimed by its author as a modern version of Galileo's *Dialogue Concerning the Two Chief World Systems.*

The most obvious irony of the dialogue is the contrast between its many passages about Rutherford and what Rutherford himself said about scientific achievements as a collective creation, a profound and modest passage which apparently Bronowski was not aware of. First a quotation from Bronowski:[20]

> The finding, in physics as much as in painting, remains a personal illumination [...] Rutherford's model of the atom was not a fact simply concealed in nature and waiting for any Tom, Dick, or Harry to fish it up. Rutherford's absurd and wonderful model of the atom was an imaginative discovery, a highly personal way of seeing nature [...] To any one who knew the bluff colonial manner of Rutherford, the roughness and twinkle, and solemn sense of pulling his own leg, everything in his discovery of the structure of the atom is of a piece. The methaphors are as much a part of Rutherford's personality as the idea of the experiment...

The contrast between Conant and Bronowski is already clear. The former stresses the impersonality (impartiality) of science; the later says:[21] "Everything here [in Rutherford's scientific creation] is as individual and as human as Rembrandt." In another dimension, the "uniqueness" of the idea of the experiment and the interpretation of its result (i.e., the Rutherfordian model of the atom), Bronowski's idea contrasts Rutherford's own statement. Again, to be a careful critic, let me quote some more from Bronowski:[22]

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[19] *Ibid.*, p. 9.
[20] J. Bronowski, *Science and Human Values* (Harper and Row, NY, 1965), pp. 104—05.
[21] *Ibid.*, p. 104.
[22] *Ibid.*, p. 103.

[...] to see something in an original way, we must see it in a personal way [...] But the layman has still to learn, you both have still to learn, that what is true of painting is also true of physics—that physics also, I have to repeat it, is constructed by men, not machines—the moments of great discovery in physics, are flashes of vision when a single man sees a new link between different and apparently unrelated aspects of reality. In this visionary moment, the great scientist lay bare a new linking [...] And his vision is as imaginative, as much a creation, as the painter's vision.

Now a paragraph from Rutherford himself. He talked about physics, no mentioning of the arts; and he talked about physics as "the combined wisdom of thousands of men":[23]

It is not the nature of things for any one man to make a sudden violent discovery; science goes step by step, and every man depends on the work of his predecessors. When you hear of a sudden unexpected discovery—a bolt from the blue as it were—you can always be sure that it has grown up by the influence of one man on anothers, and it is the mutual influence which makes the enormous possibly of scientific advance. Scientists are not dependent on the ideas of a single man, but on the combined wisdom of thousands of men, all thinking of the same problem, and each doing his little bit to add to the great structure of knowledge which is gradually being erected.

I.3.e *The History of Rutherford's Model of Atom Explained*

Following one of the above-quoted passges, after declaring Rutherford's model as a highly personal way of seeing nature, Bronowski immediately added one proviso: "even though Rutherford was then able to persuade a thousand other physists to see nature in his way." How could a thousand physicists—characteristically stubborn independent thinkers—be persuaded by something "individual, human, visionary, and imaginative"? It was plain fact in the first place; now thanks to Bronowski's sophisticated comments, an explanation is needed.

Let us follow the natural order: first, the genesis of the idea of Rutherford's experiment. By the turn of the century J.J. Thomson discovered the electron and Becquerel and the Curies discovered radioactivity. For physics the study of the atomic and subatomic structures became the order of the day. The collision, or scattering, method, pioneered by Rutherford, was one of the few conceivable experiments for inquiring into the atomic structure. The crux of the method is in the counting and measuring of radioactive particles. We do not know a certain structure, but we can control (or measure) certain flying bullets—the structural parts and the bullets are of comparable sizes. We shoot at the structure with these bullets, and measure the scattering of the bullets from the structure. If we know (or we can assume) the dynamic laws that mediate the interaction between the flying bullets and the unknown structure, then we can use mathematical analysis to infer about the unknown structure from all the known factors: the conditions of the flying bullets before and after

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[23] J. B. Cohen, *Revolution in Science* (Harvard University Press, Cambridge, Mass., 1985), p. 553.

the interaction (collision, or scattering) and the law of interaction. Why was this one of the few conceivable types of methods? Let me use an analogy. How do we study an object? First we look at it. How do we carry out the action "looking"? We receive and check the light bouncing back from it. We would like to "look" at the atom the same way—if it is possible. But it is not. Visible light is useless here—it bounces back from the atom as a whole; it does not penetrate into the atom. Fast radioactive particles can do the job. Rutherford was great because he was one of the first who realized this possibility, and designed and carried through a corresponding experimental project. A little later Max von Laue realized that X-rays could penetrate and detect crystal structures, and because of this insight he, like Rutherford, was awarded the Nobel Prize. There was nothing personal in the essentials of these scientific discoveries. What Rutherford and von Laue achieved were analogous to the simple action of looking.

Moreover, when a scientific method, after its invention, rapidly interested fellow scientists and motivated them to apply, improve, or renovate it, the world was ripe for the birth of the method. If Tom had not invented it, Dick or Harry was about to break the path. Can't we see this spirit—no sudden discovery; not a oneman affair; progress through mutual influence—in Rutherford's passage quoted?

Next, let us try to understand the genesis of Rutherford's interpretation. Why in 1911 could Rutherford in principle persuade every rational being of his model? Because at that time that was the best to account for his experimental data, best in the sense of objective, numerical comparisons. In 1911 and the following few years, with Rutherford's experimental data, and with data from any lab that performed the scattering of alpha-particles by a gold foil, any theoretician could try to mentally construct a model of the atom of gold. But any model that strove to encompass the phenomena of the scattering had to fit an extremely concentrated nucleus into every gold atom. That was the essence of Rutherford's model. Prior to him, his teacher, J.J. Thomson, had conceived the "raisin bread model" of the atom: raisins are electrons; bread, evenly distributed positive charge. In a sense the step that Rutherford took was a well-established methodological procedure: to test, (some philosophers like to say, to "falsify"), a scientific hypothesis. According to the Thomsonian model, the collision between alpha-particles and a piece of gold foil would be like firing a swarm of artillery shells at a sheet of paper. While in experiments most of the shells did behave as expected from the Thomsonian model, a tiny portion of the shells surprisingly bounced back—that was Rutherford's wonderful finding. It was an easy inference that the unknown structure of the gold atom was not evenly distributed positive electricity studded with electrons. Rather, the structure was mostly empty space, with a heavy, concentrated positive core—many times heavier than the "artillery shells" themselves.

It was a straightforward episode in the history of modern physics. With Rutherford's or similar experimental equipment, any Tom, Dick, or Harry would acquire the same sets of data and with some unsophisticated mathematical analysis—Rutherford himself wrote that he was a simple man and that his working math never exceeded analytical geometry—many Toms, Dicks, or Harrys would have to reach the same Rutherfordian model.

Let me wind up my critique of Bronowski by quoting another passage from the same dialogue, because it will be useful later when we define science. Dr. Potts, Bronowski's Salviati,[24] declared:

> If I have to give you a single answer to the question "what makes a great scientist;" then I say that he is made as every creative man is made, painter or novelist, musician or poet.

Bad analogues. Science is rigidly factual. On this score, history is a much closer analogy. I agree that great scientists are creative. But their creativity is different from the creativity of all four types of artists listed by Bronowski. None of them are required to be factual in their creative work. In fact, they all could be as fantastic and rhapsodic as one could imagine.

I.3.f *Primitive Religious Thoughts and Scientific Theory*

I will continue to cite a few more recent writers with the critical view that some of their publications are components of a recent movement and that, with these publications, they do not do justice to the idea of science. Robin Horton is the author of a series of papers published in the 1960s, that led to his publication of "African Traditional Thought and Western Science" in 1970. His major thesis is that a profound similarity exists between African primitive religious thought and modern scientific theories. In the following I will quote from his 1970 paper. I may only make minimum comments. Since I have explained my basic stand, I believe I can get my message across by simply exhibiting his typical passages. Horton tells us that an African divinerdoctor[25]

> [...] who diagnose the intervention of a spiritual agency is also expected to give some acceptable account of what moved the agency in question to intervene. And this account very commonly involves reference to some event in the world of visible, tangible happenings.

With this, Horton thought he had stated a justifiable "reason" to compare an African diviner with "an American physicist." He found that the arguments of these two persons, the diviner and the physicist, are parallel: the substitution of one set of terminology for the other results in the replacement of the diniver's diagnosis by the physicist's explanation or vice versa. Horton concluded:[26]

> In both cases reference to theoretical entities is used to link events in the visible, tangible world (natural effects) to their antecedents in the same world (natural causes) [...] Both are making the same use of theory to transcend the limited vision of natural causes provided by common sense.

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[24] Salviati is one of the three characters in Galileo's two monumental dialogues, and evidently serves as Galileo's mouthpiece.

[25] Robin Horton, "African Traditional Thought and Western Science," [in:] Bryan Wilson, ed., *Rationality* (Harper and Row, Evanston and NY, 1970), p. 136.

[26] *Ibid.*, p. 136.

Horton characterizes typical African belief systems this way:[27]

In many (though by no means all) tradicional African belief systems, ideas about the spirits and actions based on such ideas are far more richly developed than ideas about the supreme being and actions based on them.

According to him, along with a hierarchy of spirits there is a system of "different levels of thinking." Furthermore, Horton identified a similarity between this system and science:[28] "It seems clear that they [these different levels of thinking] are related to one another in much the same way as are different levels of theoretical thinking in the sciences." Then the primitive though rapidly sheds off its religious and mystical garments:[29] "At this points the relation between the many spirits and the one God loses much of its aura of mystery. Indeed there turns out to be nothing peculiarly religious or 'mystic' about it." Finally the phrase "much the same way" transcends into "essentially the same": "For it is essentially the same as the relation between the homogeneous atoms and planetary systems of fundamental particles in the thinking of a chemist."

When evaluating his own work, Horton made the following statement:[30]

In treating traditional African religious systems as theoretical models akin to those of the sciences, I have really done little more than take them at their face value [...] Above all, it [Horton's approach] has cast doubt on most of the well-worn dichotomies used to conceptualize the difference between scientific and traditional religious thought. Intellectual *versus* emotional; rational *versus* mystical; reality-oriented *versus* fantasy-oriented; causally oriented *versus* super-natuarlly oriented; empirical *versus* nonempirical; abstract *versus* concrete; analytical *versus* non-analytical; all of these are shown to be more or less inappropriate.

I.3.g *"Eastern Mysticism" and 20th-Century Physics*

One of the cultural shocks that I experienced when I came from China was the realization that on the basis of a traditional pluralism, a wide spectrum of intellectual pursuit and ideological experimentation exists in the West, much wider than I had anticipated. While undoubtedly advanced in science-and--technology, unexpectedly—for me, several years ago—the West is also featured with many marketable authors whose mission is to blur the distinction between science and "belief systems." Horton thinks the shedding of "the aura of mystery" on the part of African traditional thought improves its status in machting modern science; Fritjof Capra celebrates "the parallels between modern physics and eastern mysticism," and states that "physics leads us today to a world view which is essentially mystical." To sum up, what was collected by Capra from Taoism, Buddhism, Hinduism, and Confucianism, and labelled

[27] *Ibid.*, p. 144.
[28] *Ibid.*, p. 144.
[29] *Ibid.*, p. 144.
[30] *Ibid.*, p. 152.

"eastern mysticism," are a host of "wise" instructions, cynical remarks, and illogical conundrums—essentially a group of maxims that preach intellectual irresponsibility, pessimism, nihilism, or defeatism. In order to identify modern physics and "eastern mysticism," Capra is most interested in jotting down what physicists had said in their bafflement. That relativistic and quantum physics had abandoned some of the "classical" manner in describing physical phenomena was for Capra a major opportunity and motivation. Since I do not intend to grapple with his nebulous parallelisms and grandiloquent claims, let me just make a few general observations: A) In facing up to any difficulty people are divided into optimists and pessimists. B) The progress of science was made possible by the confidence and efforts of intellectual optimists. C) No one denies that science is always tormented by numerous unsolved problems, a fact that is in the nature of science and was reflected in its many definitions. Admittedly the inchoate development of relativistic and quantum physics perplexed scientists; (Newton's universal gravity baffled a host of Cartesians of his time). The nature of difficulty in physics during the first few decades of the century, however, should be best judged by historical development. Physicists felt for a while unused to the new discoveries and perceptions, but ever since then conceptions and theories have been settled, and stabilized, in the sense that they, serving as the guide of all the experimental and theoretical activities, are constantly verifed, checked, improved, and renovated in the ever-expanding and most fertile research in the subatomic and ultra-cosmic worlds. Thus far, relativistic and quantum physics are not clouded with a impending crisis. When a revolution does come, some time in the future, a sense of mystery will inevitably recur in many perplexed minds, but neither that situation will imply another "parallelism" between "eastern," or any other, mysticism and our futuristic physics.

I.3.h *Overglorifying China's Scientific Past: One of the Consequences*

J. Needham was probably the first person to relate ancient Chinese proto-scientific thinking to the future development of science. In *Science and Civilization in China*, Vol. 2, published in 1956, Needham declared:[31]

> All that our conclusion need be is that Chinese bureaucratism and the organicism which sprang from it may turn out to have been as necessary an element in the formation of the perfected world view of science...

And:[32]

> The gigantic historical paradox remains that although Chinese civilization could not spontaneously produce "modern" natural science, natural science would not perfect itself without the characteristic philosophy of Chinese civilization.

[31] J. Needham, *Science and Civilization in China*, Vol. 2 (Cambridge University Press, 1956), p. 339.

[32] *Ibid.*, p. 340.

In other words, Chinese civilization, which could not produce modern natural science, will be fertile for a "futuristic" science. But I am not the only person to doubt whether there is any seed of truth in this "gigantic historical paradox." Nakayama wrote:[33]

> However, it is very doubtful that "organism" in its Chinese version could ever take the role of a promoter of modern science. Some of the characteristics of organism that Needham considered uniquely Chinese might be found in other pre-modern cultures too.

Needham's understanding of field physics, relativistic physics, and quantum physics was faulty.[34] Meanwhile misunderstanding strengthened his confidence in his above-mentioned buoyant hypothesis. In addition to this aspect of philosophy of science, Needham also offered problematic historical interpretations for China's scientific past in answering questions such as: How great were achievements there and then? How much was the potential? How should we evaluate and explain Chinese developmental patterns? While comparing Chinese and Western history of physics, apparently thanks to his boundless admiration of Chinese civilization, Needham put forth a hypothesis that attributes arbitrariness, or adventitiousness, to the formation of a branch of the exact science, oblivious of the internal logic of the sequential accumulation of scientific contents. It was the realization of this mistake of his that led me to the idea of the inexorable accumulative order of positive discoveries in exact sciences. We will discuss this principle of quasi-determinism in the next chapter.

I.3.i *"Convergence" of Science and Religion*

I by no means want to make a comprehensive survey of the literature that now forms a special field from my critical viewpoint. As the last instance of this section let me quote one 1978 book with a high-sounding title: *Toward One Science: the Convergence of Traditions.* By "toward one science" the author means to incorporate "extrasensorial" studies, horoscopy, astrology, etc, into "science", and to synthesize science and religion. He thinks that the long history of conflict between science and religion has come to an end in the present era. "In either physical science or theology, any claim that we have the final insight at a particular time must be dismissed as insufferable arrogance." The author tells us: "The important sense in which we have to say that the current theories of physical science are not ultimately true is the historical sense." His historical sense of physics should be best illustrated by his own paragraph:[35]

> What is fairly certain about such entities as quarks, gluons, and gravitons is that eventually they will be dropped from our theories, not because what is being said about them is false or

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[33] S. Nakayama, "Joseph Needham, Organic Philosoher," [in:] S. Nakayama and N. Sivin, eds., *Chinese Science* (The MIT Press, Cambridge, Mass., 1970), p. 39.

[34] Wen-yuan Qian note, 9 above, particularly Chapter III.

[35] Paul Snyder, *Toward One Science: the Convergence of Traditions* (St. Martin's Press, NY, 1978), p. 200.

nonsensical but because we will eventually "stop talking that way." [...] Statements about phlogiston and ether can no long be assessed for truth or falsity in the context of physics. We simply don't talk that way any more.

Any current scientific theory will be shown one day that it is not ultimately true. Science is, by its nature, self-critical, dynamic., and progressive. It is constantly subject to the test of a set of truthful criteria, which, in trun, advances in terms of experimental rigor and logical perfection. But no one can take advantage of this fact to try to deny the reliability of science—when reliability is understood historically. (When 17th-century scientists used phlogiston to explain combustion, they, with their level of experimental practice and theoretical maturity, found it reliable. When in the 18th century phlogiston became suspect, it was finally replaced by oxygen, which has been reliable ever since.) Apparently the true goal of the author of *Toward One Science* was to do away with physics altogether, because his "physics" cannot even assess truth or falsity with regard to phlogiston and ether, hence much less can it distinguish physical concepts that have justified their legitimacy by taking part in the explanation of a series of events and those *ad hoc* ones that existed shortly by accounting for a few isolated phenomena and have since been denied by respective crucial experiments. Nevertheless, a true historian would not even contemptuously discard concepts of the latter type. Because in the final analysis, there is no rigid division between the former and the latter. Most of these two types of concepts appeared to meet explanatory necessity and played significant historic roles. Some of them served as inevitable rungs in a ladder that leads to a later and better understanding—this is one of the themes some of the following chapters will evince.

It happens that we still talk the way a phlogistonist talked, only that with oxygen replacing phlogiston, our language about combustion, calcination, respiration, etc. makes more sense than G.E. Stahl's three hundred years ago. It also happens that we still talk the way an "etherist" did, because light, X-rays, and electromagnetic radiation in general have been scientifically confirmed as wave-motions, only that the medium that transmits the wave had peculiarities that we have not yet recognized. And it happens that we still talk the way an "electric fluidist," such as Benjamin Franklin, did. Doesn't any textbook of physics today still use the phrase "electric current"? Science always talks in its characteristic way, insofar as it is collecting data, devising concepts, inventing rules, and explaining phenomena—especially explaining them by way of predicting. That is why phlogiston, ether, caloric, etc. have their prominent positions in various chapters of the history of exact sciences, whereas horoscopy, astrology, and that ilk were peripheral even in the history of mediaeval and Renaissance science, and since the Scientific Revolution, these latter have been driven out of the history of science for good.

Several decades ago the declaration "we simply don't talk that way any more" might sound heroic, although it never applies to science at its epistemological level. Much less can it be used to jumble science with superstition and mysticism. Essentially science talks the same way since antiquity, because science has its unchanging core. With regard to "the way of talking," the component in the unchanging scientific core is that science always aims at "saving the appearances," meaning explaining the phenomena. More evident than talking, science has been working the same way since antiquity. From Thales of Miletus (as an early geometer) onward, scientists have been measuring physical objects, taking their results of measurments seriously, making commonsensical, down-to-earth judgments, and using their rational thinking. They are still, and will keep, working the same way.

I.4 *Possibility of and Efforts in Defining Science*[36]

I.4.a *No One Can Define Anything by Ignoring its Essential Factors*

J.D. Bernal published *The Social function of Science* in 1935. One of his critics, H. Dingle, pointed out that he should have defined science. Then in 1956 Bernal published another big book *Science in History*, in which he gave a detailed discussion of the "aspects of science." In the meantime, he still refused to define science:[37]

> My experience and knowledge have convinced me of the futility and emptiness of such a course. Science is so old, it has undergone so many changes in its history, it is so linked at every point with other social activities, that any attempted definition, and there have been many, can only express more or less inadequately one of the aspects, often a minor one, that it has had at some period of its development.

Among the aspects of science, Bernal listed science as an institution, a method, a cumulative tradition of knowledge, a major factor in production, a powerful influence molding beliefs and attitudes, and the interaction between science and society. Yet two aspects that I think to be crucial for science are missing in Bernal's discourse. These two aspects are the constant character, or repetitiveness, of the subject-matters of scientific pursuits and the criteria of truthfulness. From primitive science to modern science, methods of acquiring science have changed; science institutions came into being quite recently; many scientific topics have never found utility in production; primitive science did not influence the beliefs and attitudes of the primitive people as strongly as modern science does modern people... Yet it is easy to see that the two aspects of repetitive phenomena as the subject-matters and the necessity of verifying

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[36] This section is a general survey of definitions of science from Aristotle down to several modern authors. But neither my collection nor analysis is meant to be complete. At the 4th International Conference on the History of Chinese Science (Sydney, May 1986), I learned from fellow Chinese delegates that scholars at the Institute of the History of Natural Science and Technology In Beijing (Peking) have accomplished a collection of about 150 definitions of science.

[37] J. D. Bernal, *Science in History*, 3rd edition (The MIT Press, Cambridge, Mass., 1965), p. 30.

scientific assertions are shared by every stage of science in any civilization all through the history of *homo sapiens*. Exact or statistical constancy (repetitiveness) in phenomena provides the possibility of establishing prescient (predictive) knowledge; criteria of truthfulness, in which the basic item is empirical verification, guarantee in the first place the dependability of information, and then uplift the "scientific degree," hence predictive power, of science itself. Of these two, the character of the subject-matters is basic. If the phenomena are utterly capricious, no scientific criterion can turn the study of them into a system of predictive knowledge. On the other hand, because it takes tremendous theoretico-scientific efforts to turn "raw" phenomena into scientific knowledge, the basic aspect, certain specific character of the subject-matters of the study, can neither logically encompass the other aspect, a set of progressive, or hierarchical, truthful criteria.

A closer look at Bernal's view reveals the reason of his negligence of scientific subject-matters and scientific standards. In both his 1935 and 1956 books he stated that the tradition of science traces back to magicians, priests, and others. Magicians boast about and actually perform extraordinary feats; priests help people to go to heaven after death. So, these two types of specialists do not deal with repetitive or repeatable phenomena, and do not teach predictive rules verifiable impersonally and repeatably. They both are proud of being able to do things others cannot do. But the function of science is by nature populist. It was science that helped people to survive and prosper. Primitive people had good botanical science so that they could teach their children not to eat poisonous mushrooms. Grass-roots scientific knowledge has been derived from observing repetitive phenomena, and has been repeatedly tested by people in their daily life. There is no denying that science *interacted with* traditions represented by magicians and priests. Nevertheless, *science has its own independent sources*, more ancient and more respectable than those of magic and religion.

I.4.b *Definitions Representing Varied Levels of Emphasis*

Traditionally, in defining science scientists and philosophers paid attention to the character of knowledge or the method of acquiring knowledge. In the *Nichomachean Ethics* Aristotle declares: "To be acceptable as scientific knowledge a truth must be a deduction from other truths." This definition is consistent with his idea of scientific knowledge discussed in detail in the *Posterior Analytics* and other works. Some scholars argue convincingly the predominant influence of geometrical ideas upon Aristotle in this regard. We will introduce these themes in Chapter Three. In the 17th century, Thomas Hobbes, for whom Euclidean geometry and Galilean mechanics had been significant scientific enlightenment, states:[38]

[38] Thomas Hobbes, *Leviathan* (Collier Macmillan, NY, 1962), p. 48.

> Science is the knowledge of consequences, and dependence of one fact upon another: by which, out of that we can presently do, we know how to do something else when we will, or the like another time; because when we see how any thing comes about, upon what causes, and by what manner; when the like causes come into our power, we see how to make it produce the like effects.

In these propositions the internal coherence of knowledge is put forth as a requisite; they are consistent with a most popular definition of science that it is systematic (or organized) knowledge In the 19th century there appeared a crop of definitions. For instance, William Whewell (1794—1866), the scholar who coined the word "scientist," had this: Man is the interpreter of Nature, Science the right interpretation. J.S. Mill (1806—73) had this: The only proper sense of the term of science is inquiries into the course of nature. W. Stanley Jevon (1835—82):[39] "Science is the detection of identity"; or, "Science arises from the discovery of the identity admist diversity." Here, the first two definitions speak about the act and goal of science; to interpret or to inquire. Jevon's definition uses similar verbs, to detect and to discover; but it is also much richer in contents: it endeavors to mentally reduce plurality to simplicity. This definition, thus, on the one hand bears similarity with some ancient Pythagoreans' definition of beauty—uniformity within diversity—and on the other hand, heralds the definition of science by Ernst Mach (1836—1916)—the mental economizer—which, in turn, traces back to "the razor of Ockham (*ca*1300—*ca*49)," especially in its formulation by Henry of Langenstein (d. 1397): Hypotheses should not be multiplied without necessity.[40] Whewell also had a more formal definition for science:[41]

> When our conceptions are clear and distinct, when our facts are certain and sufficiently numerous, and when the conceptions, being suited to the nature of facts, are applied to them so as to produce an exact and universal accordance, we attain knowledge of a precise and comprehensive kind, which we may term science.

According to this definition, science is the kind of knowledge that is exact and precise, and *a fortiori* universal.

Henri Poincaré was an advocate of science for its own sake:[42] "The search for truth should be the goal of our activities; it is the sole end worthy of them." From *The Values of Science* we learn that he was debating with a certain Le Roy., who thought[43]

> [...] science is only a rule of action. We are powerless to know anything and yet we are launched, we must act, and at all hazards we have established rules. It is the aggregate of these rules that is called science.

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[39] W. Stanley Jevon, *The Principles of Science* (Dover, NY, 1958), pp. 673 and 1.

[40] Nicholas H. Steneck, *Science and Creation in the Middle Ages* (University of Notre Dame Press, Notre Dame, Indiana, 1976), p. 35.

[41] W. Whewell, editor Yehuda Elkana, *Selected Writings on the History of Science* (The University of Chicago Press, Chicago and London, 1984), p. 203.

[42] H. Poincaré, note 15 above, p. 205.

[43] *Ibid*., p. 323.

Poincaré defended the objectivity of science by pointing out that it is the best rule and it reflects our knowledge of events:[44]

> The rule of tric-trac is indeed a rule of action like science, but does any one think the comparison just and not see the difference? The rule of the game are arbitrary conventions and the contrary convention might have been adopted, *which would have been none the less good*. On the contrary, science is a rule of action which is successful, generally at least, and I add, while the contrary rule would not have succeeded...
>
> Science foresees, and it is because it foresees that in can be useful and serves as rule of action.

Although this is not Poincaré's formal definition of science, with this passage he covered both vital aspects of science: its basis and its function. Science is built upon empirical verifications. Its rules are not arbitrary, not conventions. Only those that pass tests are acknowledged as science. To fulfill its fundamental function, science aims at prediction, or foreseeing. To acquire predictive power is the goal of science. Science builds, strengthens, broadens itself in order to foresee better.

Several early definitions, including Aristotle's, bespeak the "theoretical," or "developed," levels of science, when science as a systematized body of knowledge is stressed, and as an intellectual "economizer," or "axiomatizer," is portrayed. Chapter Three will make it clear that axiomatic epistemology as a principle emerged early in ancient Greece, even earlier than the first branch of systematized empirical science, geometry, had reached its classical axiomatic format, Euclid's *Elements*. At the present stage, when we are concerned with defining science, all these definitions, with their respective emphases spreading through varied levels, could be useful for us. They remind us that a good, comprehensive definition may start with the core aspects, and build upon the core "theoretical" superstructures.

I.4.c *Defining by Degree*

All the 20th-century definitions of science, up to about 1950, that I have learned, strongly uphold the universality of science. Einstein said that science is the most objective thing known to man. Whitehead defines science as a passionate pursuit of the relation of general principles to irreducible and stubborn facts. D.D. Kosambi, an Indian scholar, has a definition of science, remarkably succinct: Science is the cognition of necessity. In *What is Science?* N. Campbell states:[45] "Science is the study of those judgments concerning which universal agreement can be obtained." In the early 1940s, J. Conant tentatively defined science this way:[46] "[...] perhaps science is after all only organized common sense, preferably derived from experiment and preferably

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[44] *Ibid.*, pp. 323–24.

[45] N. Campbell, note 3 above, p. 27.

[46] J. B. Conant, "The Advancement of Learning in the US in the Post-War world", *Science* (February 4, 1944), no. 99.

organized on quantitative basis." In 1946 when Conant was asked to give the Terry Lectures at Yale he found it was hard to answer the question: What is science? Following the Lecture, when he was preparing the manuscript for publication, he realized that he had ended the study with a definition of science, as he had "outlined the boundaries of one portion of accumulative knowledge in terms of historical development and called it science":[47]

> As a first approximation, we may say that science emerges from the other progressive activities, of man to the extent that new concepts arise from experiments and observations, and the new concepts in trun lead to further experiments and observations. The case history drawn from the last three hundred years show examples of fruitful and fruitless concepts. The texture of modern science is the result of the interweaving of the fruitful concepts. The test of a new idea is therefore not only its success in correlating the then-known facts but much more its success or failure in stimulating further experimentation or observation which in turn is fruitful.

This formulation contains the essentials of science: experimentation and observation as empirical basis as well as methodology of science, and science as a progressive human activity. Conant did mention the correlation of facts as a test of science, but theorization as an increasingly important methodology was not stressed.

In 1949, Max Black published "The Definition of Scientific Method." Black began his paper by telling us that to define scientific method is "nearly the same thing" as define science itself. Then he told us that he was not happy about the Baconian definition that science is an activity that yields power and mastery. He proposed that[48]

> We treat "scientific method" as a historical expression meaning, among other things, "those procedures which, as a matter of historical fact, have proved most fruitful in the acquisition of systematic and comprehensive knowledge."

So, with regard to the character of the body of information, his expression was "systematic and comprehensive knowledge." But it is problematic to represent science itself this way: even mythology, theology, and other fields of subjective intellectuality could be systematic and comprehensive. The concrescent and elastic set of scientific methods characterized by Black, however, would guarantee the production of reliable scientific knowledge:[49]

> Neither observation, nor generalization, nor the hypothetic-deductive use of assumption, nor measurement, nor the use of instruments, nor mathematical construction—not all of them together—can be regarded as essential to science. For branches of science can easily be found where any one of these criteria absent or has so little influence as to be negligible [...] The characters mentioned are neither necessary nor sufficient, but they may be present in higher or lower degree and they contribute to what we recognize as science.

Since Black sees the scientific method as composed of multiple components, interactive and flexible, he urges us "to think of science as a concrescence, a

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[47] J. B. Conant, note 18 above, p. 24.

[48] M. Black, "The Definition of Scientific Method", [in:] R. Stauffer, ed., *Science and Civilization* (University of Wisconsin Press, madison, 1949), p. 81.

[49] *Ibid.*, pp. 80–81.

growing together of variable, interacting mutually reinforcing factors contributing to a development organic in character."[50] With regard to the components of the scientific method, Black put forth an interesting couplet of statements: "Their diminution removes from an activity the features we apprehend as scientific; their joint presence in high degree creates conditions recognized as pre-eminently scientific." Therefore, according to him, there is not a solid demarcation line dividing science and non-science. Branches of scholarship vary in their "scientific degree." This is an interesting conclusion. By considering conformity to a hierarchical set of truthful criteria that a science has achieved, instead of the use of scientific methods, I myself arrived at a similar conclusion, which I will explain in the next section.

Also in 1949, Morris R. Cohen gave a definition of science, which affirms that science, on the one hand, adaptable and progressive as systems of knowledge, and on the other hand, traditional and continuous in terms of the "canons" that guide the test and acquisition of knowledge. In other words, its contents grow and become better; its criteria remain unchanged:[51]

> We may define science as a self-corrective system [...] science invites doubt. It can grow or make progress, not only because it is fragmentary, but also because no one proposition in it is in itself absolutely certain, and so the process of correction can operate when we find more adequate evidence. But note, however, that the doubt and the correction are always in accordance with the canons of the scientific method, so that the latter is the bond of continuity.

I have to admit that this is a requote from Black's work, and I could not locate Cohen's own essay. Thus I am not clear about his "canons of the scientific method." With conjectural understanding, if I may, I would translate his statements "science is fragmentary" to "science endeavors to become an axiomatic system," and his phrase "more adequate evidence" to "passing more stringent tests or covering new phenomena."

I.5 *A definition of Science*

I.5.a *The Unchanging Core of Science*

It is necessity, need and curiosity that motivate human beings to acquire and use factual and predictive knowledge about nature, human communities, and themselves. Human mentality has various aspects, which served as the originators of various types of intellectual creativities: visual arts, music, mythology, fiction, religion, and so on. It is the mentality that sought exactness that engendered science. Men and women survived, prospered, improved themselves and changed their environment, because they, in the first place, had and used science. I agree with the idea that our primitive ancestors had and applied science—in fact, as a necessity.

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[50] *Ibid.*, p. 94.

[51] *Ibid.*, pp. 86—87.

As I have explained in I.4.a, unlike Bernal, who was more or less daunted by the myriad changes experienced by science itself and though it would be futile to define such a protean creature, I looked for its unchanging core. When I saw that not only a certain scientific constancy exists, but it also serves well to distinguish science from non-scientific intellectual creativities, I realized that it would also lead to a good definition. The unchanging core of science is data and prescience—three words, including the word "and"—pithy enough to apply to the continuum from primitive science to 20th-century science—although for a long, long time the body of knowledge thus qualified was limited, either in its scope, or in its degree of exactness, or in its predictive power.

I have also explained before that science deals with repetitive phenomena and imposes upon itself a set of truthful criteria. What are the logical connections between these statements and the statement about what science is? The connections are straightforward. Because science deals with repetitive phenomena, it could accumulate scientific data and pursue prescient knowledge; and thanks to the application of truthful criteria, data and prescience could be tested, rejected, replaced, or upgraded. Therefore, we can say that, since, for instance, the time of Peking (Beijing) Man, i.e. about five hundred thousand years ago, science always deals repetitive phenomena, always imposes upon itself a set of advancing truthful criteria, and always comprises data and prescience. What does science do? In a sense, science does all sorts of things to bring to fruition the logical connections between what science is (data and prescience) and what science deals with (repetitive phenomena) and how (with a sharp truthful awareness). Science makes measurements; it accumulates and checks data; it invents, tests, and extends its predictive rules; it integrates, remodels and axiomatizes its logical systems.

Some scholars reminded me that Darwin and Wallace studied biological evolution in the last century, and nowadays some people are pondering the Big Bang (or the Steady State). These are neither repetitive nor repeatable phenomena. Shall we continue to call Darwin a scientist? Of course we will, and in doing so, we will not cause any conceptual confusion. Biological phenomena are statistically repetitive: the recurrence of life cycles; the identity, or repetition, of individuals of the same species, etc. Evolutions are longterm developmental processes realized by many life cycles; in the process each generation basically resembles its parental generation, with some unnoticeable systematic mutations. To the question of the Big Bang one possible answer is: the study is a theoretical exploration, in which all the natural laws used were themselves the consequence of the study of repetitive physical phenomena.

I.5.b *History is Not Science*

Being exact alone does not qualify knowledge as scientific. Exactness is a necessary confition, but not sufficient. Journalist reports and chronological records are ideally factual and exact; they are not science. Not even history, which differs from chronicles in that it endeavors to explain past events and evolution. In 1962, Carl Hempel published "Explanation in Science and History,"[52] in which he grouped "the natural sciences, psychology, and sociology as well as historical inquiry" together as "the various branches of empirical science." The paper argues for the conformity with science of "the historical mode of explanation." In addition, Hempel stated that "two concerns" have sustained scientific inquiry since time immemorial: one, to predict and control natural events; two, to satisfy intellectual curiosity. Then we have a logical problem if history is included in science. First, prediction and control never apply to history, which means studying happenings in the past. If we drop this terminological limit and include into history events to come, we still have difficulties. One, any prophecy about future socio-political development is at best a wise guess, which lacks even statistical exactness. Two, any one who attempts at controling human affairs usually does it in a very unscholarly manner. The reason that history is not a science is that its subject-matters are neither repetitive nor repeatable, not even statistically. Thus far scholars still have difficulty in identifying human motivation, attitude, behavior, moral, aspiration, endurance, etc. as statistically repetitive phenomena. To correlate human psychology to collective, community behaviour would be an even much harder task for the future.

I.5.c *Scientists and Craftsmen*

As I have explained before that I do not identify the roots of science with the chief occupations of magicians and priests. I cannot agree with Bernal even if he invokes magicians' "ordered speculations"[53] and priests' "rationalized mythology."[54] Because "unnatural" feats, miracles, afterlife "phenomena," etc. do not belong to the jurisdiction of science. On the other hand, Bernal's view that part of the scientific roots were in "the practical operation and traditional lore of the craftsman" and "ordered technique" does make sense for me. Craftsmen, or technologists, are scientists.

Considerations such as these precipitated me to try to define a series of vocations. The "definitions" that I arrived at may look different from what is stated in a dictionary, because these professions are defined with a special reference to the idea of science. Let me list my definitions: the first few types of proffesionals are closest to scientists; other than they are not arranged in a particular order. 1) Technologists study, realize, or try to realize, repeatable products or processes, either within the possibility of natural laws, or with an

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[52] C. Hempel, "Explanation in Science and History", [in:] R. G. Colodny, ed., *Frontiers of Science and Philosophy* (University of Pittsburgh Press, 1962).

[53] J. D. Bernal, *The Social Function of Science* (The MIT Press, Cambridge, Mass., 1967), p. 13.

[54] J. D. Bernal, note 37 above, p. 3.

attempt to take advantage of some then-unknown laws, or even impossible ones. 2) Alchemists study and try to realize certain repeatable phenomena, in the sense that either it is an impossible task (such as the making of "the philosopher's stone"), or it requires the discovery of new (futuristic) laws (such as those govern the general transmutation of matter). 3) Astrologers study, describe, and predict the alleged cause-effect connections between the movement and change of heavenly bodies on the one side and character and behavior of earthly individuals on the other side, connections which have never been empirically justified. 4) Artists produce artifacts, compose writings or music, or give performances that are endowed with such aesthetic excellence that they cannot be easily repeated. 5) Magicians claim or intend to perform feats which are contrary or exceptional to natural laws. (Jugglers are technologists, or athletes, not magicians.) 6) Miracle-expectans believe in the occurrence of phenomena which are beyond the jurisdiction of natural laws. 7) Priests are members of a missionary, hierarchical organization the intellectual core of which is an arbitrary body of spiritual doctrines, ethical assumptions, anecdotal traditions, and ritualistic conventions. 8) Theologians explain the world on the basis of the priestly intellectual core, "rationalized" by its combination with current, supportive "science" and "logic." 9) Mystics have faith in personal, miraculous experience which they claim to be spiritually satisfying, cannot verbally explain to others in full, and do not share with others in any empirically verifiable way.

If we compare the first two categories, we see certain similarity in them. Alchemists were confident that once they accomplished the transmutation, or produced the elixir, the operations would be repeatable under specified conditions. On the other hand, technologists might go overboard as well, attempting at repetitively realizing certain chains of events which, denied by science, cannot even be carried out once.

I.5.d *The First Two Levels of the Tiered Truthful Criteria*

M. Black's definition of science recognizes the varied degree of using scientific methods by various branches of science. Instead of relying on the elastic concrescence of scientific methods, my definition will use a set of tiered criteria of truthfulness. The set has three levels; it is a "vertical," or hierarchical system, not concrescent. Various branches of science match these criteria in varied degrees. The first level, also the most fundamental, is empiricism: science should stand impersonal, repeatable empirical verification. When referring to technology, practicality will be the suitable word to replace, or stand side by side with, verifiability.

The next level is logical rigor and mathematical accuracy. Here is the chance for us to come back to the question: Are mathematics and logic science? Evidently they both deal with repetitive phenomena, either physical or conventional, and they impose a high standard of truthfulness upon themselves. Therefore, they are science. Geometry is a branch of physics; all its theorems are verifiable by measuring lengths, angles, areas, etc.—in the final analysis, lengths. Owing to its antiquity, the origin of geometry—theorizing land measurement—appears more or less legendary. On the other hand, the empirical origin of projective geometry, thanks to its proximity to modern times, has been confirmed in developing linear perspective and optical instruments. Yet, taking advantage of the fact that projective geometry in its maturity had acquired a high degree of abstraction, B. Russell declared that "it takes nothing from experience" (I.2.d).

If geometrical theorems are only logical deductions, not empirical truth, then I can completely ignore them, as much as I ignore the rules of playing American football. As a matter of fact, they are physically objective. Consequently I can never ignore them. If I do, I will make mistaken judgments all the time, at many occasions fatally dangerous judgments. And so is logic. The rule "A belongs to B; B belongs to C; therefore A belongs to C" and others, are practical physics in countless context. While simple arithmetic was known to all ancient civilization, it took many centuries for mathematicians to realize sophisticated algebraic rules; but all are valid natural laws. If one calls them conventions, they are at present the best conventions—cf. Poincaré view, I.4.b.—and they are amenable for further improvements. In sum, the second level of the truthful criteria demands that only verifiable rules are allowed for use in argument and reliable technologies permitted in practice. Valid laws of mathematics and logic are widely applicable rules. There is no denying that in its development science often uses heuristic devices, especially analogies, which are not applications of rigorous logic. We should, however, notice two facts. One is that other than an axiomatic basis, science does not tolerate extra hypotheses. While unavoidably they would crop up in a developing science—like many famous conjectures in the history of mathematics and many more "phenomenological theories" in the history of physics—science always aims at incorporating them into its rigorous system, in which only valid mathematics and logic are used. Another fact is that any branch of exact science also works to improve its axiomatic basis (because it is itself a hypothetical theory). In the sense that science is not satisfied until its whole system is turned into a rigorous theory (built upon an axiomatic basis), we can say that science uses science only; (or, science does not allow, or like, mixing non-science into in system).

Shall we call games with strict rules "science"? Many people do. Nowadays we have all sorts of "sciences" in this regard—from the hard "science" of playing chess to the soft "science" of boxing. These man-made sciences are limited in the sense that their rules are all known, and subject to artificial changes. Their excitement is in the varied and competitive applications of the rules. Natural and social sciences—as a contrast—always have unknown and inexorable rules to be discovered. The applications of these rules are also exciting games for those who know the game.

I.5.e *"The Principle of Economy" enters into the Criteria*

The third level of the criteria is axiomatization. It should be desirable to discuss it here, though preliminarily. As I have mentioned at the beginning, I.1, it is possible that a definition of science and a theory about the history of science will strengthen each other. In the 4th century BC, closely following Plato's philosophical guide. Aristotle put forth formulations about an axiomatic criterion of science, especially in his *Posterior Analytics.* The short definition of science that we earlier quoted from the *Nichomachean Ethics* naturally reflects his standard and theory of a deductive knowledge. He states that science should be, in terms of inherent logic, a closely knitted body. In Greece it was the time when geometry moved, over successive generations, toward its axiomatization. Within a few decades following Aristotle's death, in Euclid's *Elements,* geometry reached its epitome. As it was a century-long process of mutual interaction, it is hard to decide if it was geometers who carried out the scientific epistemology of Plato and Aristotle or reversely, it was philosophers who saw and articulated an ongoing intellectual movement. We will come back to this and related interessing issues in Chapter Three. The salient feature of the monumental and undying Euclidean geometry is its unity of intellectual power (reliability) and beauty. Since its birth, it plays a most prominent role model in theoretical construction for other fields of physical sciences: the more successful a branch of physics is axiomatized, like the *Elements,* the more predictive power it acquires. This is where the "magic" power of modern exact sciences is. (Nothing is truly "magic" here, of course. It is quite an intelligible phenomenon. More later.) Several of the following chapters of this work will show that in a sense the Scientific Revolution of the 16th and 17th centuries was the awakening of the endeavor in one field after another to emulate the achievement of Euclidean geometry—by axiomatizing themselves, or by increasing their "degree of axiomatization."

Another amazing feature of axiomatization is that it is at once so old and so young; it is an ongoing affair as well as ancient history. Thus here we have another unchanging element of science. Or a quasi-constant element, because we cannot ascribe axiomatization to the science of Peking (Beijing) Man; it began with Plato, or a little earlier. Since the Scientific Revolution, scientists have been uniformly rendering their comprehensive presentations in physics and mathematics in an axiomatic format. It is heartening to see that recently an eminent textbook of biochemistry is systematized under a number of axioms.[55] All leading physicists trusted axiomatic epistemology wholeheartedly. Albert Einstein used glowing words to praise the first axiomatized branch of science:[56]

> We reverence ancient Greece as the cradle of Western science. Here for the first time the world witnessed the miracle of a logical system which proceeded from step to step with such precision

[55] Albert L. Lehninger, *Biochemistry,* 2nd edition (N. Y. Worth Publisher, NY, 1975).
[56] A. Einstein, *Essays in Science* (The Wisdom Library, NY, 1934), p. 13.

that every single one of its propositions was absolutely indubitable—I refer to Euclid's geometry. This admirable triumph of reasoning gave the human intellect the necessary confidence in itself for its subsequent achievements. If Euclid failed to kindle your youthful enthusiasm, then you were not born to be a scientific thinker.

It should be enlightening to contrast what Einstein said about Euclidean geometry with R. Horton's mixing up of science and belief systems, which we have learned about earlier. Doesn't such contrast caution us about the movement that works to sabotage our historically established scientific integrity?

By the turn of the century, to refute Tolstoy, who said that "science for its own sake" means taking randomly into account any fact, Poincaré wrote:[57]

I will not further insist, but these few words suffice to show that the scientist does not choose at random the facts he observes. He does not, as Tolstoi says, count the lady-bugs, because, however interesting lady-bugs may be, their number is subject to capricious variations. He seeks to condense much experience and much thought into a slender volume; and that is why a little book on physics contains so many past experience and a thousand time as many possible experiences whose result is known beforehand.

First, a correction of a minor point. The number of lady-bugs is subject not to "capricious variations," but to statistical fluctuations, which present topics of enormous scientific (and practical) interest. With popular parlance, Poincare disclosed the quintessence and power of scientific axiomatization. Yet intellectual power was not the whole story. Poincaré was a great admirer of the beauty of nature, and the beauty of science. Closely following the above--quoted, he continues:

The scientist does not study nature because it is useful, he studies it because he delights in it; and he delights in it because it is beautiful.

Later he balanced the two aspects, intellectual utility and beauty, and unified the two into a principle credited to Ernst Mach: Science economizes intellectual efforts, I.4.b. Poincaré's passage is:[58]

We see too that the longing for the beautiful leads us to the same choice as the longing for the useful. And so it is that this economy of thought, this economy of effort, which is, according to Mach, the constant tendency of science, is at the same time a source of beauty and a practical advantage.

I.5.e *A Definition that covers Technology and Taxonomy*

The idea of "axiomatic consistency"[59] itself smacks of pure intellectualism. As I have indicated at the very beginning, I.2, there is no reason to over-value

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[57] H. Poincaré, note 16 above, p. 366.

[58] *Ibid.*, p. 367.

[59] Wen-yuan Qian, "The Idea of the laws of Nature: as a Logical as well as a Historical Question", [in:] *Abstracts of Papers, the XVIIth International Congress of History of Science, Berkeley, 1985* (Office for History of Science and Technology, University of California, Berkeley, 1985). Section Qe.

the theoretical side of science; a comprehensive definition should be balanced between intellectuality and utility, or practicality. Engineering technology, in distinction from artistic creation and performance, aims at the realization of repeatable products or procedures. Moreover, just like nature, who does nothing in vain, technology intends to avoid waste. Thus, here we have a common philosophical ground for science (intellectual economy) and technology (utilitarian economy). There is also an evident parallelism between an axiomatized system of theorems and an integrated system of machines. Aware of these shared bedrocks, it will not be hard for us to arrive at a formulation that encompasses both sides. Anyhow, we will not say: "The scientist does not study nature because it is useful." Instead, we will explicity put utility into our definition of science. Unexpectedly, in so doing we have Plato's support. Despite all his sublime philosophy, Plato apparently cherished the idea that science includes technology. The following is from the *Theaetetus*, a passage of conversation between Socrates and Theaetetus:[60]

> *Socrates*: ...So tell me, in a generous spirit, what you think knowledge is.
> *Theaetetus*: Well, Socrates, I cannot refuse, since you and Theodorus ask me. Anyhow, if I do make a mistake, you will set me right.
> *Socrates*: By all means, if we can.
> *Theaetetus*: Then I think the things one can learn from Theodorus are knowledge—geometry and all the science you mentioned just now; and then there are crafts of the cobbler and other workmen. Each and all of these are knowledge and nothing else.

Following this, although Socrates used long discourse to remind Theaetetus that the answer he wanted was what knowledge in abstract is, he did not deny "a knowledge of shoemaking" or "a knowledge of how to make wooden furniture" the status of knowledge—as it was understood here, *scientific* knowledge. Also noticeable is the last quoted sentence, "[...] and nothing else." Scientific knowledge is certainly not all-inclusive; it is strictly limited. Here, finally, is our definition of science:

Science comprises branches of learning (or topics in them) that study exactly or statistically repetitive or repeatable phenomena, that aim at acquiring exact and predictive knowledge or realizing repeatable proceses, and that are tested and elaborated progressively by impersonal, repeatable verification or practicality, the exclusive use of rigorous logic and valid mathematics, and the accomplishment or having the potential of forming an integrated system conforming to the principle of intellectual or utilitarian economy.

A few scholars told me that this definition itself needs a number of definitions to explain the terms contained. I think all the phrases used are commonsensical, but let me try to make them more familiar and also make a few promises. In chapter Two the idea of "exactly or statistically repetitive or repeatable phenomena" will be further clarified, as it will be related to

[60] Plato, translator Francis M. Cornford, *Theory of Knowledge* (The Liberal Arts Press, Indianapolis, NY, 1957), p. 21.

measurements explicitly. "Intellectual economy" means theoretical reduction; it echoes what the famous 14th-century Ockham's razor has already said, I.4.b. Since E. Mach, the idea has become popular. We will have a number of opportunities to come back to it. "Exact knowledge," "predictive knowledge," "impersonal, repeatable verification or practicality," and "utilitarian economy" really do not need special explanations. There is a little difficulty with "rigorous logic and valid mathematics." Who is to judge the rigor and validity of logic and mathematics? Two points need to be made. One is empricism; the other, "historicism." They are historical, meaning changing, developing; but at any historical stage, people believed the existence of dependable logic and mathematics. Moreover, most of the logical and mathematical rules have been trusted as *the* truth, constant and universal. That is the reason why the phrase "rigorous logic and valid mathematics" does make sense for most of us.

I.6 "*Two Cultures or a Spectrum of Cultures*"?

In the *Confessions*, Books V and VI in particular, and *The City of God*, Chapters 2 and 16 of Book VIII in particular, St. Augustine (354—430) showed clearly his awareness of two types of intellectuality. They are approximately the equivalent of the modern dichotomy of science and non-science. He characterized science as fields of knowledge that admit direct demonstration and unquestionable reasoning; he understood that the touchstone of science is its capability to predict. On the other hand, as a former teacher of rhetoric, he could very well distinguish the empty eloquence of a fake, Manichean "scientist" from the "the real truth." While this Manichean bishop, Faustus, disappointed him in scientific matters, Augustine still recognized him as a good scholar in "literature." In *The City of God* Augustine shows two distinguishable attitudes, one toward scientific regularity, another miraculous unpredictability. For him both were legitimate. Moreover, as is well known, the issue of faith *versus* reason was one of St. Augustine's major concerns. The famous Galilean tract, *A Letter to the Grand Duchess* (1615), especially highlights the Latin Father's awereness of the necessity of reason and science, and his support of them.

This brief historical review may serve us as a remote background to C.P. Snow's contentions made in his 1959 Rede Lecture *The Two Cultures*. Snow showed worry about the lack of mutual understanding between science and non-science. Then a few years afterwards, J. Bronowski published *The Abacus and the Rose*, a dialogue that aims at showing the similarity between science and non-science, or the identity between the "Abacus" and the "Rose." As I have pointed out, in doing so, among other things, he compromised the integrity of science.

Nevertheless, science does share common characteristic with a number of disciplines which were traditionally considered as non-science. Besides, the term science covers many disciplines which themselves differ from each other in "scientific degree," and they have been changing, rather developing their "scientific degree." Max Black arrived at this conclusion by considering the flexible and concrescent use of scientific methods, I.4.c. I myself did the same by taking into account the fact that collecting data and discovering regularities can be achieved by degree. In fact, more than one hundred years ago, J.S. Mill had arrived at some interesting observations that presaged this dynamic picture. I would like to put his ideas into three perspective themes:

1) A number of disciplines, one following the other, strove to attain the status of science:[61]

> We learn to do a thing in difficult circumstances by attending to the manner in which we have spontaneously done the same thing in easier ones.
>
> This truth is exemplified by the history of the various branches of knowledge which have succesively, in the ascending order of their complication, assumed the character of sciences, and will doubtless receive fresh confirmation from those of which the final scientific constitution is yet to come and which are still abandoned to the uncertainties of vague and popular discussion.

2) The movement is a process of following a leadership; the leader was physical sciences:[62]

> If there are some subjects on which the results obtained have finally received the unanimous assent of all who have attended to the proof, and others on which mankind have not yet been equally successful [...] it is by generalizing the methods successfully followed in the former inquiries and adapting them to the latter that we may hope to remove this blot on the face of science.

And:[63]

> The backward state of the moral sciences can only be remedied by applying to them the methods of physical science, duly extended and generalized.

And 3) "There may be sciences which are not exact science."

As a thinker of Victorian England, J.S. Mill believed in progress and evolution. He pointed out: "Astronomy was once a science, without being an exact science." Although worded differently, the true message of his discourse on meteorology and tidology as inexact sciences is that they deal with statistically repetitive phenomena, and that owing to this circumstance as well as to the current level of scientific knowledge, they were established as statistical sciences.

If we put Bronowski's figurative terms into Mill's thesis, we have to admit that "abacuses" are numerous, and some of them are better than the others. If we call the better ones "calculators," then some "abacuses" have been turned into "calculators." These changes were spontaneous intellectual movements. Normal historical development rarely needed an artificial movement, let alone one that moves "the abacus" toward "the rose." The issue of interchangeability between the two, the abacus and the rose, will be clarified soon.

C.P. Snow had a legitimate worry. Yet in retrospect it seems ironic that he

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61 J. S. Mill, editor E. Nagel, *Philosophy of Scientific Method* (Hafner, NY, 1950), p. 305.
62 *Ibid.*, p. 309.
63 *Ibid.*, p. 307.

made a sharp division between the two. Into these two categories nowadays we do not know to assign many branches of studies. Even J.S. Mill would have experienced the same difficulty. We can, however, solve the difficulty by turning Snow's "two bags" into a linear, dynamic configuration. One end of the latter is occupied by branches of the exact science; the other end, witchcraft, horoscopy, divination, and things of the short. More specifically, thus, we may imagine that a the scientific end of the spectrum is, for instance, geometry, whereas at the non-scientific extremity is fortunetelling, which, as a "science," has too irrelevant and too narrow a data basis and makes too audacious predictions about too complex—nonrepetitive—phenomena. Many "candidate" branches of science are moving, according to their efforts and performance, between these two extremities.

J.S. Mill was optimistic about fields of studies being transformed into science. But he did not mean that all human intellectuality moves in this same direction. The external necessity of non-science, such as art, fiction, music, history, etc. is apparently a conviction shared by the majority of scholars. Yet, occasionally we would encounter an idea to the contrary. As part of his reason to refuse to define science, I.4.a, J.D. Bernal made a bold all-abacus-and-no--rose conjecture:[64]

> In the future it may well be that scientific knowledge and method will so pervade all social life that science will once again have no distinct existence.

But this will not be a likely outcome. For instance, the distinction between science and the arts can never be obliterated. Let us imagine an advanced mathematician reviewing a well-written book of calculus and a virtuoso conductor reviewing the score of a symphony. What they read are familiar to them; they both are satisfied, and you may say, aesthetically. But, after all, mathematics appeals to a person's reason, whereas music appeals to a person's feeling. In other words, the abacus and the rose are not interchangeable. On the other hand, in realms where the synthesis between science and would-be science is possible and corresponding spontaneous movements are going on, synthesis should not be realized by lowering the standards of science, but by clearly announcing them and welcoming "open competition."

Hence the necessity of a note. Although I leave many branches of studies between science and non-science, call them "candidate" fields, or would-be science, and "instigate" them to compete with each other, I do not mean to "credit" them by their proximity to science. After all no one knows how to measure this proximity quantitively. I did all that to take into account the facts that there have been synthesizing intellectual movements, and that a number of studies are adopting scientific methods and standards, or are labelling themselves as science. Regardless their proximity to science, these studies, thanks to the character of their subject-matters and their achievements, all have their own intrinsic value and interest.

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[64] J. D. Bernal, note 37 above, p. 31.

CHAPTER 2. SCIENCE DEVELOPMENT: PRINCIPLES

II.1 *The Non-scientific Aspects of Science*

As will soon be explained, science cannot always be treated scientifically. But science should be treated as science itself, neither mystified nor "demystified." The confounded situation, described in the previous chapter, in the current study of the history, philosophy, or sociology of science that I intend to come to grips with is, in a sense, rooted in this mistreatment. Why do so many scholars, working in the non-scientific aspects of science, want to tell people that "science is not all that scientific"? This is indeed a fascinating paradox: Whereas science is characterized by inherent inexorable logic, it seems all right during the recent past, for many people, and in a number of fields, to ignore this logical inexorability.

I myself seem to be particularly sensitive to this type of paradox. Several years ago I was intrigued by the paradox that on the one hand traditional China shows evident inertia in developing science, on the other hand, a couple of Western authorities describe China's scientific past in a glowing tone. My national background doubtless played a role in imparting to me that particular sensitivity. I want to diagnose China's chronic problems. Now I realize that I owe my fascination in the paradox that science being treated not as science to my professional background, i.e. teaching theoretical physics.

This paradox in turn has its roots. Modern science is no longer its bare (scientific) definition, data and prescience. It is now burdened with a variety of superstructures, many of which are shared by scientific and non-scientific disciplines. In other words, modern science has its non-scientific aspects. Owing to the intristic character of the subject-matters of these aspects, it is impossible to conduct the study of them with exclusive scientific methods, or to demand scientific exactness and reliability for the study. It is simply not easy to be always clear-headed in distinguishing non-scientific aspects of modern science from its scientific essence. In the case of J.D. Bernal on the possibility of defining science, as we studied before, I.4.a, this point was shown clearly. Some important non-scientific aspects, such as the use of science, the developmental priority of science, etc. are enormously value-laden and emotion-stimulating. Science made radar and atomic bomb, yet the application of these hardwares has been based on considerations utterly alien to science itself. Intensive concern and debate on these issues cause obliviousness among many people of the basic fact that science is primarily a body of objective knowledge, not a human institution.

II.2 *The Messy Construction Site of a Logically Designed Building*

Another non-scientific aspect of science, its history, is also a perplexing factor that tends to make people forgetful of scientific inexorability. In this regard the caprice of history in general influences people's views on the history of science; besides, the panoramic picture of science development in various civilizations, again, causes people to overlook the logical structure within science history per se.

For the relationship between science and its history I would like to use an illustrative analogy. If we compare the established science with a building—not necessarily finished, but it is already a building, visible and usable—and the making of science with the building under construction, i.e. the construction site, a historian of science today may want to remind me that any construction site looks messy. But my emphasis is different. I want to point out that if the finished building is logically patterned, it is always possible to identify the basic rationale behind the messy goings-on of any aspect and at any moment of the construction site. This basic rationale decides, in the first place, the sequence of the construction of the various parts of the building. Therefore, it is like constructing a multi-story building; it would be illogical to finish an upper floor earlier than a lower story.

Science has never been a finished building, and no one can empirically demonstrate or logically prove the existence of a God who had logically designed the universe. But the development of science does show the inherent logicality in every realm of nature. It is this logic that prohibits an illogical order in the human recognition of this logic itself. This is the idea of a quasi-deterministic theory of the accumulation of scientific results. It is not deterministic, because no theory can tell exactly when a certain positive achievement would be accomplished in a certain civilization. It is quasi--deterministic, because it ascertains the logical, sequential order.

II.3 *Quasi-determinism Solves "The Needham Hesitation"*

The idea of quasi-determinism sprang from historical study and solved an interesting paradox. In 1950 Joseph Needham delivered a lecture titled *Human Law and the Laws of Nature in China and the West*, and published in the next year. Needham concludes this speech with an increasing confidence in the "organic" nature of science—and of nature itself. He believed that traditional Chinese thought was close to this perception:[1]

> The Chinese world-view depended upon a totally different line of thought. The harmonious co-operation of all beings arose, not from the order of a superior authority external to themselves, but from the fact that they were all parts in a hierarchy of wholes forming a cosmic pattern, and what they obeyed were the internal dictates of their own natures. Modern science and philosophy of organism, with its integrative levels, has come back to this wisdom, fortified by our new understanding of cosmic, biological, and social evolution; though who shall say that the Newtonian phase was not an essential one?

At the same time Needham started cherish the novel idea of a hypothetical type of physics that might have replaced the physics that has been bestowed to

[1] J. Needham, *Human Laws and the Laws of Nature in China and the West* (Oxford University Press, London, 1951), p. 43.

us, or decided for us, by the actual history. He hypothesized that had modern science been developed on the basis of Chinese mode of thought, the outcome could have been very different:[2]

> Had these conditions been basically favorable to science, the inhibiting factors considered in this lecture would perhaps have been overcome. But all we can say of that science of Nature which then would have been developed is that it would have been profoundly organic and non--mechanical.

But Needham was not completely sure about this hypothesis. We certainly cannot ignore his follow-up question "though who shall say that the Newtonian phase was not an essential one?" in the first quotation. Within the context this simple question sounds logically jarring.

Six years later, in 1956, Needham published the second volume of *Science and Civilization in China*, the last chapter of which is again "Human Law and the Laws of Nature in China and the West." The above two quotations are here basically repeated. The simple question that we are particularly interested in was now put as an independent sentence: "Yet who shall say that the Newtonian phase was not an essential one?"[3] After another six years, Needham published Part 1, Vol.4 of the same series. At the very beginning of this book, he wrote:[4]

> Most important in this Section will be, of course, the development of knowledge about magnetism, in particular the discovery and exploitation of the directive property of the lodestone. The Chinese were so much in advance of the Western world in this matter that we might almost venture the speculation that if the social conditions had been favorable for the development of modern science, the Chinese might have pushed ahead first in the study of magnetism and electricity, passing to field physics without going through the stage of "billiard-ball" physics. Had the Renaissance been Chinese and not European, the whole sequence of discoveries would probably have been entirely different.

In order to reach his hypothetical physics, which may be termed the Needhamite "organic physics," there should have been a hypothetical history: Chinese natural philosophy, Chinese achievement in magnetism, together with "social conditions" which "had been favorable for the development of modern science."—Too bad, the last item was completely unrealistic for China. If it had been real, the end product, according to Needham, would be "magnetism and electricity" and "field physics." Implicitly Needham assumed that these branches do not need the label "Chinese"; they would be universal science, an assumption acceptable to many of us. Another implicit assumption, is, however, puzzling. It is the assumed equivalence between Needham's "organic physics" and the universal "field physics." Whereas Needham thought that his "organic physics" is intrinsically incongruent with the mechanical "billiard-

[2] *Ibid.*, pp. 43—44.

[3] J. Needham, *Science and Civilization in China*, Vol. 2 (Cambridge University Press, 1956), p. 582.

[4] J. Needham, *Science and Civilization in China*, Part 1, Vol. 4 (Cambridge University Press, 1962), p. 1.

-ball" physics, most physicists would have difficulty to recognize the presumed "contradiction" between Newton's dynamics and Maxwell's electrodynamics. Anyhow, from the quotations it is clear that his confidence in the "essentiality" of "the Newtonian phase," or "the stage of billiard-ball physics," was fading away during those twelve years, although his last words were still loaded with evident hesitancy: "we might almost venture the speculation that [...]." More study of the "organic" traditional Chinese thought pulled him off from a vaguely perceived inexorable sequence of the development of physics. What we see here is the reflection of an inner, intellectual tension—developing over a period of twelve years—in the mind of a great scholar. Once the tension is located, naturally concerned scholars would want to continue pondering it. After all, probably no one has, or very few have, ever dwelled on the meaningful theoretical issue of whether there is an inexorable sequence in the development of exact sciences. I suggest that we give a name to the above-mentioned historiographical episode, or Needham's 1962 speculation. It is indeed most thought-provoking. I would like to call it the Needham Hesitation, or the Needham Ambiguity.

In my published works I refuted the speculation with both scientific logic and historical logic.[5] The complexity of the theoretical framework of electrodynamics so evidently surpasses its equivalent in Newtonian dynamics that a reversed order in their establishment is simply inconceivable. The publication of Newton's *Principia* and J. C. Maxwell's *Treatise on Electricity and Magnetism* were separated from each other by two scientifically hectic centuries. The historical refutation is more interesting; it shows how much ideological preference could bias a scholar's view and decide the character of his/her obliviousness. A historian needs only compare Needham's venturous speculation with the following chronicle to see the folly in the former. In 1600 William Gilbert, a London physician, published a comprehensive and influential book on the science of magnetism; England of 1600 was part of the Renaissance and science was in the ascent there; Newtonian mechanics developed in 17th-century England; and Faraday-Maxwellian electromagnetism was established in 19th-century England.

II.4 *The Principle of Quasi-Determinism*

Thus the Needham Hesitation seems to be a solvable question and the key to it seems to be in the idea, or a certain principle, of an unyielding sequence in related scientific discoveries. As indicated before, quasi-determinism is intended to be the official term for this inflexibility, because scientific discoveries are

[5] Wen-yuan Qian, *The Great Inertia: Scientific Stagnation in Traditional China* (Croom-Helm, London, 1985); "Science Development: Sino-Western Comparative Insights," [in:] *Knowledge: Creation, Diffusion, Utilization*, Vol. 6, No. 4 (June 1985, Special Issue: Science in Changing Civilizations), pp. 377—405.

inflexible in the order, but not in exact timing, of their appearance. A full name for the idea can be the "principle of quasi-deterministic accumulation of positive scientific results." This principle affirms the fact that science is built up according to logical possibility, that, in turn, is prescribed by natural laws themselves. In the orderly construction of science, arbitrariness has little room; so-called serendipity is simply not a reliable idea.

The Needham Hesitation refers to the order of the establishment of two fields of physics, but quasi-determinism certainly also applies to specific discoveries within a field. It is when sufficient number of cognate phenomena are confirmed for their regularity—their repetitive or repeatable appearance--scientists feel the need of defining a separate field. Works at both levels (confirmation of phenomena and founding new fields of study), however, are positive scientific achievements. Thus, we can reach a unified formulation for the principle: The accumulation of related positive scientific results does not follow arbitrary sequence; on the contrary, the order is restrained by the intrinsic logic of natural laws.

A principle is not supposed to be proved, or derived from other principles. But a valid principle should be supported by evidence and arguments, from as many aspects as possible. I will describe a couple of such supports; they are of a historico-logical nature. In the history of science and technology, there have been countless instances of independent discoveries of the same or similar results. Many of these were made at approximately the same time—hence countless cases of dispute over priority. Were these coincidences or not? It is easy to see that they could be either coincidence or good support for the principle of quasi-determinism. It is also easy to infer that they could not be coincidence. Because scientific and technological discoveries are difficult and strenuous work. They represent unusually many or exceptionally hard steps pushed ahead in either technical practice or logical reasoning. Why would two or more persons take these same steps, and at about the same stage of development? It is extremely improbable that similar intellectual activities of these persons occurred *coincidentally* along similar courses of progression. The contrast between this logical inference and the historical fact that there have been numerous independent discoveries of the same results, led us, *reductio ad absurdum*, to the conclusion that the assumption that they were coincidence must be wrong.

The ultimate support of quasi-determinism is in scientific inexorability. The explanation of the history of Rutherford's atomic model in Chapter One, I.3.e, shows that at every step of research and development, in either experimental or theoretical work, the direction of progress was almost predestined. Admittedly, as a historical conclusion, this may not be universally true. At many historic junctures, two or more parallel, or alternative, developmental possibilities are conceivable, or have been carried out in actual history. In the following chapters that examine the history of several branches of physics, we will pay special attention to this issue. The principle of quasi-determinism stresses that in the history of science, inevitability—determinism, uniqueness—in choosing developmental direction is dominant. Or, to accomodate the possibility of parallel developments, we probably should talk about certain "quasi-inevitability," although here the prefix means the sever limitation in choice, while in quasi-determinism it indicates uncertainly in timing.

Another historico-logical support can be presented more specifically. Modern scholarship and education in science encourage innovation as well as pose stringent scientific criteria over innovative attempts, experimental or theoretical. Let us take a look at the situation with theoreticians. Although many of them are characterized by an independent and creative frame of mind, they are common-sensical, intelligent people, sharing the basic rationality of the majority of *homo sapiens*. Their peculiarity is that they always try to formulate, or re-formulate, scientific theories in their own way. Nevertheless, genuine innovations are rare; most of the efforts of these creative minds end up in results which are not truly innovative. If these results are not the outcome of false logic or erroneous calculation, they would be identified, one way or other, as part of the established old structure. In the case of theoretical physics, these established structures are mechanics, electromagnetism, thermodynamics, statistical mechanics, relativistic physics, quantum mechanics, quatum field theory, etc.; these are infrequent historic landmarks. For centuries every ambitious student of theoretical physics ended his/her theoretical ventures in one of these realms—either as one of the few lucky creators or as one of the majority who verified the theory by his/her own rational reasoning and accurate calculations. The vast majority who ended up trailing along beaten tracks are not proselytized converts. In science, in principle no one should be a blind follower, because the final check of theoretical arguments is empirical tests. In any class of physics majors in any university you can always find a few young people who are maniacally engaged in designing a system to replace an established one. And so are some matured physicists. Trying to convince oneself and others with a theory that conforms to tests better and represents logicality better is a normal attitude in science; the tradition established during the Scientific Revolution (1543—1687) justifies this attitude. Novelists can spin out as many stories as they want to; but theoretical physicists find themselves mostly and basically repeating the same "old stories." The limitation in theoretical creation in science reflects quasi-determinism in the making of science itself.

Chapter One offered a unified definition of science and technology. Does the principle of quasi-determinism apply to the latter? I believe it does. In any civilization, one of the most salient characters of the history of technology is its sequential and accumulative progress. In all those classical series—materials for tools and weapons, natural forces harnessed, means used in transportation, machines (such as steam engines, spinning and weaving machines, etc.) renovated through successive generations—we constantly see a scenario of step-by-step improvement. It is to be hoped that, prompted by this newly formulated principle, many experts in various fields may want to point out cases that imply the contrary, to wit, the possibility of independent or parallel technological developments. For instance, we may raise questions such as: Were vacuum tubes in electronics a necessary historical stage? Could it be completely bypassed by an early realization of solid state devices? Every field, I believe, has questions of this nature. I am certainly not in a position to make an encyclopaedic survey. That will depend upon forthcoming discussion on the applicability and generalizability of the principle of quasi-determinism.

The principle says nothing about the possibility of scientific discoveries in a civilization. That issue is, in the final analysis, decided by socio-historical conditions, or externalist reasons. The present work—arguing for the intrinsic scientific standards of science, extracting general principles that restrain the history of the exact science, and characterizing the development of physics with a unified axiomatic epistemology—is admittedly internalist. But I am neither an internalist nor an externalist. My first book in the history of science, *The Great Inertia*, is externalist, because it considers the possibility, or social encouragement and inhibition, of advancing science in a civilization. Both the scientific and non-scientific sides of science are indispensable. No one can properly understand the history of science by an exclusive approach, either externalist or internalist.

II.5 *The Empirical Basis: A Pool of Measurables*

One regularity in the history of the exact science is that it has been developed in an apparently endless series of establishing new branches of science, while the chronological parade of these branches unmistakably points out an advancement in accuracy, profundity, comprehensiveness, and—unfortunately—sophistication in our understanding of the physical world. It is possible to formulate this regularity in an exact language that we have learned from the exact science itself? Yes, it is. The rest of this chapter will show this possibility.

II.5.a *It is Heuristic to try to Resolve Paradoxes*

Neither of the two refutations of the Needham Hesitation is meant to be an argument that "proves" the principle of quasi-determinism. The truth of a principle is supported by empirical and logical conformity with known facts and will be evinced or denied by confirmed facts in the future. As a principle concerning historical development, it should be viewed favorably even if it leaves some special cases out, insofar as it synthesizes major events well. The second refutation, "the historical logic," simply shows that Needham was self-contradictory. The expose of any human error, however, does not prove anything universally true. But the argument was heuristic: since one scholar made a mistake by assuming certain arbitrariness in the sequential development of the exact science, we should probably think differently.

The first refutation, "the scientific logic," points out that it is simply inconceivable that human intellect would solve a complex problem earlier than a less complex one. Neither does this argument "prove" the principle, of course. It is likewise heuristic, because it prompts us to ask: What causes the difference in relative complexity in the theoretical frameworks of various branches of physical science? Are there any deep reasons? The answer turned out to be an encouraging "Yes." Now with this affirmative answer, the first refutation of Needham's speculation is provided with a theoretical basis. The inquiry, nevertheless, promised a more profound prospect than criticizing an individual's mistake. Another general regularity, or principle, in the development of physical science, was revealed as a consequence of the inquiry.

II.5.b *Every Branch of Physics Corresponds to a "Pool of Measurables"*

The fundamental equations of electromagnetism are by nature more complicated, or advanced, than those of mechanics: partial differential equations include ordinary differential equations as special cases. It is simply because electromagnetism has to deal with more independent variables—measurables. Electric fields and magnetic fields, and a number of other measurables peculiar to electromagnetism, do not appear in mechanics. In other words, electromagnetism is built up a larger "pool of measurables."

In fact why do we group a range of phenomena under one label—the title of a branch of physics? We have already addressed the issue of defining a separate field in the previous section, II.4, when we talked about "a sufficient number of cognate phenomena." By what relationship are these phenomena cognate? Because they share things in common. What are these things? In the final analysis, measurables. Hence again the idea of a "pool of measurables."

All geometrical phenomena are correlated by one common measurable, lengths. Angles are ratio of two lengths; areas are lengths squared; and solids, lengths cubed. All kinematical phenomena are described in terms of two measurables, lengths and time intervals: speeds are lengths divided by time intervals; accelerations, speed differences divided by time intervals; and directions are specified geometrically. All dynamic phenomena are described by three measurables, lengths, time interwals, and masses, because according to Newton's second law, a force is measured by mass times acceleration. All electromagnetic phenomena share all these plus electric fields and magnetic fields, and not completely independently electric charges and electric currents. (Electric capacities, electric resistance, and electromagnetic inductances are not treated as fundamental variables. They are on the same level as density, elastic constants, etc. in mechanics.) All thermodynamic phenomena again include all these plus temperatures and quantities of heat. The measuring pool of geometrical optics is like geometry. That of wave optics, however, needs all the elements in describing wave notion: time, phase, and amplitude; whence luminosity would be a derived measurement.

Thus, each branch of physics has its own "pool of measurables," and all factual and theoretical statements in each branch are derived from experimental and mental manipulations of these measurables.

II.5.c *"Science is Measurement"*

The exaltation of the status of measurables naturally brings us back to this old, sagacious, and brief definition of science: science is measurement. When we explored the very essence of science, in Chapter One, we realized that repetitiveness, or repeatability, of phenomena, is a necessary condition for the phenomena to serve as subject-matters of science. But the idea of repetitiveness is meaningless apart from measurement. If one does not measure, how can one ascertain that events are going on in a repetitive fashion? Or that one entity is a repetition of the other? Therefore, the idea of repetitive or repeatable phenomena itself implies the existence of a number of measurables, or the possibility of measuring a number of observables. It is true that measurement here is understood generally. When Gregor Mendel "measured" the traits of his garden peas, he hardly used any instruments.

A pendulum swings in an exactly repetitive manner—for a while at least; a ball rolls down a certain inclined plane in an exactly repeatable experiment. A planet revolves around the sky with an exact average period, but the periods of the revolutions noticeably differ from each other, a fact Ptolemy's celestial kinematics had to ignore but Copernicus' celestial kinematics could explain. Meteorology is now a respected science, but remains a statistical science. Weather forecast is based upon a vast collection of data, fast calculation, and a good deal of educated guesswork. Forecast are statistically true but not exactly dependable. The longer terms the forecast is made for, the "more" one expects the actual measurement to deviate from the predicted (most probable value). ("More" deviation is not meant in a proportional sense; it is in the "mean--square-rooterror.") The ultimate reliance of forecasts is data, which are the products of measurements. In sum, the feasibility of generalized measurements is entailed in the very definition of science.

II.5.d *Exact Sciences as Special Cases of Statistical Sciences*

With measurements, for any measured parameter, a recurrent phenomenon, is represented by a Gaussian distribution curve—the bell-shaped curve with the center at the most probable value. If the "bell" is narrow, even sharp, then the phenomena are exactly repetitive, and they form the subject-matters of an exact science. If the "bell" is wide, the phenomena belong to the study of a statistical science. If no bell-shaped curve comes out of the measurement, or equivalently, the "bell" is too wide, the phenomena lie outside the realm of scientific pursuit.

If planets are true "wanderers," as whimsical as human beings themselves, if the timing of a free fall were as unpredictable as the outcome of throwing dice, then neither planetary kinematics nor terrestrial kinematics would become the cornerstones upon which modern mechanical science grew. On the other hand, since Pierre Fermat (1601—65) and Blaise Pascal (1623—62), scientists have succeeded in establishing probabilistic sciences that include throwing dice and analogous phenomena that would be represented by an average bell-shaped curve. A little after the French mathematicians had done their work, John Graunt, William Petty, and others started the statistical studies of social phenomena in England; that was the beginning of modern social sciences.

More than one hundred years ago, J.S. Mill perceptively stated that physical sciences served as models for other sciences, I.6. Since the 19th century, in addition to physical laws, there appeared laws of biology and laws pertaining to human society. These are mostly statistical laws; prominent examples physical, biological, and social sciences are respectively the Maxwellian distribution of molecular velocity, the Mendelian law of genetics, and the great (not necessarily accurate) Malthusian law of population growth.

It is also noticeable that the difference between exactly repetitive phenomena and statistically repetitive phenomena is in degree, not in kind. Both correspond to representative curves of the same character, some relatively narrow, some relatively wide.

II.6 *The Principle of the "Expansive Measuring Pool"*

How were the "pools of measurables" historically formed? Should we try to outline a lineage of evolution? Yes, we should. Apparently it will be a meaningful inquiry.

II.6.a *The Ways the Pool Expands*

To a historian, who never neglects to look at events chronologically, the correspondence between fields of science and their pools of measurables, as presented in II.5.b, shows a general tendency of progression. This developmental viewpoint immediately helps us to answer an interesting question: Why did geometry become the first branch of axiomatized physical science? The answer is: it is the simplest; it depends upon one measurable only. Then naturally another question would follow: In terms of simplicity, or complexity, what would the next branch be? Historically it was astronomy—more exactly, celestial kinematics—as represented by Ptolemy's *Almagest* (2nd century AD), although the latter could not achieve the predictive accuracy and axiomatic neatness of Euclid's *Elements*. Ideally it should be a field that corresponds to two measurables, and preferably including lengths as one of them. Celestial kinematics satisfies these conditions exactly. So, historically the pool of measurables expands: expansion by new addition. But very quickly we would notice a difficulty. How about kinematics in general? We are confronted with a large "gap": although celestial kinematics was developed early, kinematics itself, which correspond to the same two measurables, was founded many centuries later, when Galileo published his last masterpiece, *The Two New Sciences.* Does this "gap" indicate any insurmountable difficulty that would kill an emerging theory? No, it does not pose any intrinsic difficulty; it will not kill, instead it will help the emerging theory. In terms of physical feasibility—measurements—celestial kinematics of Ptolemy and his predecessors and terrestrial kinematics of Galileo and his 14th century Parisian and Oxfordian predecessors are very different. Ancient astronomy used only large intervals of time, and the periodic movements of celestial bodies themselves were taken as the measure. By the turn of the 17th century, Galileo became the first scientist who heroically pioneered the measurement of time intervals comparable to or less than one second. Aware of this historical difference, the identity as well as the difference between celestial and terrestrial kinematics dawned upon us that a pool of measurables expands not only by new additions, but by extending practical measurements into new ranges. Usually the extension would involve major technical breakthroughs, and result in the establishment of a new field of study.

This is true even in the case of one measurable—lengths, for instance. When we look back into this simplest case with a new perspective, we have to admit that geometry deals with only lengths of "medium" magnitude. In the 17th century, "telescopic astronomy," founded by Galileo, and "microscopic biology," founded by Malpighi, Leeuwenhoek, et al., appeared. From the viewpoint of measurement, these events represent extending the ranges of the measurement of lengths respectively to the distant and the minuscule. By the beginning of this century, the establishment of crystallography, i.e. geometrical analysis of crystal structure by means of X-rays, should again be viewed as a new stage to the finer level of measuring lengths. How about the measurement of weights? In this regard we can also use a good, illustrative episode.

In a sense the 18th century "Chemical Revolution" was ushered in by the improvement in the use of the balance: the increased accuracy in determining weight changes in a chemical reaction. The crucial conceptual breakthrough occurred in the 1770s when in the mind of a few scientists oxygen substituted for phlogiston in the explanation of combustion, calcination, respiration, etc. How could this be achieved? It was, in the final analysis, the advanced use of the balance that disclosed the *ad hoc* nature of the phlogistonic theory of "negative weight" and showed solid physical plausibility in the assumption of weight transfer that accompanies the transfer of chemical elements.

Thus, the "measuring pool" experiences another "internal expansion," i.e. the increase in measuring accuracy, which is not the same as the extension of measuring ranges. Galileo was the first astronomer to "measure" the distances between lunar features of the moon, yet nowadays scientists are able to determine those distances incomparably more accurately. With a crude microscope, Leeuwenhoek was the first biologist to tell the approximate length of a bacterium; now with the aid of electronic microscopes, scientists can determine accurately the size of any unicellular microorganism. In other words, pioneers like Galileo and Leeuwenhoek extended the ranges of measurements, whereas subsequent progress increased the accuracy, or resolving power, of these measurements. Therefore, *in toto*, the pool of measurables expands in three manners.

II.6.b *Formulation*

Measuring is a technical problem. Nevertheless, the expansion of the measuring pool interacts intimately with conceptual innovations. It was the laws of constant and multiple ratios that offers the first scientific clue about the existence of atoms and molecules; and Faraday's law of electrolysis, the granular composition of electricity. Then again measurements paved the path. J. J. Thomson confirmed the existence of a particle whose mass is less than one thousandth of that of a hydrogen atom; he also determined the charge-mass ratio, e/m, of this particle, the electron. Following him, R. Millikan measured directly the charge, hence also the mass, of the electron. The quantum nature of electricity was thus first experimentally confirmed; meanwhile theoretically, the study of basic natural laws entered the stage of quantum physics, in which every measurable is atomic. Both as verified and axiomatized theories, Newtonian mechanics and quantum mechanics enjoy a harmonious relationship: the latter includes the former as a limiting case. Mathematically, when the mass in quantum mechanics grows, from, for instance, that of an electron to that of a marble, the laws of quantum mechanics undergo mutations and turn into the laws of Newtonian mechanics. In this sense, in comparison with Newtonian mechanics, quantum mechanics represents a more comprehensive and unified understanding of nature.

The principle under our consideration can be named "the principle concerning the physical basis of a branch of physical science," or "the principle of the expansive measuring pool." We may formulate it this way:

The experimental and mental manipulations of a "pool of measurables" from the physical basis of a branch of physical science. Along with progress in measuring technology, a pool of measurables expands in one, or two, or all of three manners: new added measurables, extended measurable ranges, and increased accuracy of measurements. As a rule, the expansion of a pool leads to the establishment of a new branch of physical science, which represents a more comprehensive and unified understanding of the natural phenomena that are described and correlated by the expanded pool of measurables.

The confirmed correlations of measurables—as a result of the experimental and mental manipulation of the latter—are physical regularities and laws which constitute the cadre of a branch of physical science, empirically verifiable in its predictions and ideally axiomatic in its format.

II.6.c *Progress toward Accuracy, Thoroughness, and Unity*

Although theoretical reasoning often—and evidently with increasing frequency—provides stimulants for innovations in the development of physics, measurements have to follow up in order to check and test theories. Up to the establishment of thermodynamics, the progress had been featured by new additions of measurables. Thereafter, two "internal expansions" of the pool were in vogue for about a century. Yet nowadays in the physics of elementary particles, again we see a befuddling scenario of the proliferation of new measurables, whose physical meanings are mostly waiting for further clarification and familiarity.

The kinship between classical mechanics and quantum mechanics, and between these two and relativistic quantum mechanics, are easy to characterize: from the past (classical) to modern, the sequence shows an ascending generality. In other words, in comparison with a classical field, modern field includes more measurables, applies to extended ranges of measurements, and shows better validity when the measurables are determined with increased accuracy. When the mass of a household particle decreases and approaches that of an atom, classical mechanics becomes incompetent, whereas quantum mechanics begins to show its strength. When the speeds of moving objects approximate that of light, "classical" quantum mechanics in turn becomes incompetent, whereas relativistic quantum mechanics begins to show its strength. The statement that periods of normal science and extraordinary science alternate with each other amounts to a tautology of describing the familiar fact that physics develops by stages. But the principle of "the expanding pool" discloses forthrightly the essence of this feature of development: Doing normal science means working, experimentally or theoretically, with the existing pool of measurables; doing extraordinary science means trying to extend, experimentally or theoretically, the pool of measurables—in one or more of the three possible manners.

The history of science has been journeying along an inevitable path: we had to, and still have to, try to understand nature with a vast, expanding, deepening, yet compartmentalized body of knowledge. It is in the expansion of the pool of measurables, which in the final analysis depends upon technical progress, that we entertain the hope that the incomplete unity of the study of nature will gradually approach the perfect unity of nature itself.

II.6.d *Practical Significance*

Naturally I am tantalized to fantasy about the scientific significance of the principle of the expanding pool. My current guess is that it may serve as a reminder to scientists, experimentalists or theoreticians. A practising scientist may want to check his/her existing pool of measurables carefully. Are all the measurables "balanced" in terms of their range and accuracy? What are the best opportunities to extend the pool?

II.7 *Plato's Intuition: The Order of the Quadrivium*

Among all the intellectual factors none was more significant than a sustained tradition of mathematical education—though for centuries suffering from extreme hardship—that enabled the West to become the land that finally generated modern sciences. The core of the tradition was the quadrivium—arithmetic, geometry, astronomy, and music—which had been first designed by Plato. From the *Republic* we see that Plato was an education reformer, allegedly in whose Academy one could never meet a person who was not conversant in geometry. In Book VII of the *Republic* the dialogue was between Socrates and Glaucon; the topic was education: the enlightenment of people—kings, warriors, and others. Plato told us that formerly the curriculum was gymnastics and music; now, he suggested that it be changed to arithmetic, geometry, solid geometry, astronomy, and music—in this order. As if to emphasize "the order of the sciences,"[6] Plato arranged to let Socrates make a mistake. Solid geometry should have followed plane geometry; then astronomy, "motion of solids," follows solid geometry. Socrates, for some reason, had first forgotten solid geometry. Then he apologyzed to Glaucon for delaying him. The interesting point for us, of course, is the correspondence between Plato's order and the early chronological sequence of the "expansive pool of measurables."

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[6] Plato, translator, B. Jowett, *The Dialogues of Plato,* Vol. II (Bigelow, Brown and Co., NY), p. 285.