Mario Bunge

Treatise on Basic Philosophy

Volume 6

SPRINGER-SCIENCE+BUSINESS MEDIA,B.V

TREATISE ON BASIC PHILOSOPHY

Volume 6

EPISTEMOLOGY AND METHODOLOGY II:
UNDERSTANDING THE WORLD

TREATISE ON BASIC PHILOSOPHY

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MARIO BUNGE

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GENERAL PREFACE TO THE *TREATISE*

This volume is part of a comprehensive *Treatise on Basic Philosophy*. The treatise encompasses what the author takes to be the nucleus of contemporary philosophy, namely semantics (theories of meaning and truth), epistemology (theories of knowledge), metaphysics (general theories of the world), and ethics (theories of value and of right action).

Social philosophy, political philosophy, legal philosophy, the philosophy of education, aesthetics, the philosophy of religion and other branches of philosophy have been excluded from the above *quadrivium* either because they have been absorbed by the sciences of man or because they may be regarded as applications of both fundamental philosophy and logic. Nor has logic been included in the *Treatise* although it is as much a part of philosophy as it is of mathematics. The reason for this exclusion is that logic has become a subject so technical that only mathematicians can hope to make original contributions to it. We have just borrowed whatever logic we use.

The philosophy expounded in the *Treatise* is systematic and, to some extent, also exact and scientific. That is, the philosophical theories formulated in these volumes are (*a*) formulated in certain exact (mathematical) languages and (*b*) hoped to be consistent with contemporary science.

Now a word of apology for attempting to build a system of basic philosophy. As we are supposed to live in the age of analysis, it may well be wondered whether there is any room left, except in the cemeteries of ideas, for philosophical syntheses. The author's opinion is that analysis, though necessary, is insufficient—except of course for destruction. The ultimate goal of theoretical research, be it in philosophy, science, or mathematics, is the construction of systems, i.e. theories. Moreover these theories should be articulated into systems rather than being disjoint, let alone mutually at odds.

Once we have got a system we may proceed to taking it apart. First the tree, then the sawdust. And having attained the sawdust stage we should move on to the next, namely the building of further systems. And this for three reasons: because the world itself is systemic, because no idea can become fully clear unless it is embedded in some system or other, and because sawdust philosophy is rather boring.

The author dedicates this work to his philosophy teacher

Kanenas T. Pota

in gratitude for his advice: "Do your own thing. Your reward will be doing it, your punishment having done it".

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PREFACE TO *EPISTEMOLOGY II*

This is the sequel to *Epistemology I: Exploring the World*. In that work we studied cognition as a brain process, and communication as a social transaction. In particular, we studied perception and conception, the formation of propositions and proposals, exploration and systematization, discovery and invention. We regarded knowledge as an outcome of processes occurring in animals that learn by themselves and from one another. We took concepts and propositions, problems and proposals, to be equivalence classes of brain processes rather than ideal objects detached from brains and from society. However, we also stressed the need for studying such abstractions as well as the corresponding real processes.

In other words, we admitted that cognition ought to be studied both concretely (as a biopsychosocial process) and abstractly (with disregard for personal and social idiosyncrasies). We hoped in this way to favor the merger of the various hitherto separate approaches to the study of knowledge and knowledge-directed action: the neurophysiological and the psychological, the sociological and the historical, the epistemological and the methodological ones. After all, these various approaches have a single aim, namely to improve our understanding of the ways we get to know reality, and the ways knowledge can be utilized to alter reality.

In this volume we will study the ways theories and proposals (e.g. technological designs) are put to the test and used to understand or alter reality. We will stress the difference between belief and inquiry. We will study the kinds of knowledge and the ways human knowledge grows, declines, or alters course. We will distinguish basic science from applied science, and both from technology and ideology, and we will seek to demarcate genuine knowledge from bogus. We will analyze the two mechanisms for enhancing the cross-fertilization and the unity of the various branches of knowledge: reduction and integration. We will stipulate the conceptual and empirical conditions a proposition has to fulfill in order to be valued as (sufficiently) true, and a proposal to be regarded as (suitably) efficient. (We shall do so in the light of real cases drawn from contemporary research rather than in obedience to a priori philosophical principles.) We will analyze a number of important yet vague notions, such as those of truth and efficiency, background and framework, paradigm and revolution. And we will explore the possible limits to our exploration of the world, as well as the limitations of the classical philosophies of knowledge.

The upshot of our study is a descriptive and normative epistemology that cannot be compressed into a couple of slogans, although it combines some features of rationalism with others of empiricism. This synthesis may be called *scientific realism* because the criterion for adopting or rejecting any given thesis is its compatibility or incompatibility with the practice of research in contemporary science (basic or applied), technology, or the humanities. We find no use for a theory of knowledge, however exact or ingenious it may be, that is divorced from knowledge.

SPECIAL SYMBOLS

$\neg p$	*not-p*
$p \& q$	*p and q (conjunction)*
$p \vee q$	*p or q (disjunction)*
$p \Rightarrow q$	If p, then q (*implication*)
$p \Leftrightarrow q$	If p, then q, and conversely (*iff*)
$A \vdash B$	Premises A *entail* conclusions B
$=_{df}$	Identical by *definition*
$\{x \mid Fx\}$	The *set* of objects possessing property F
$\varnothing$	The *empty set*
$a \in A$	Individual *a belongs to* set A
$A \subseteq B$	Set A is *included* in set B
$A \cup B$	The sets of objects in A *or* in B
$A \cap B$	The set of objects in A *and* in B
$A - B$	The set of objects in A but *not in B*
$A \Delta B$	The set of objects in A *or* in B but *not in both*
$\langle a, b \rangle$	The *order pair* of a and b
$A \times B$	The *cartesian product of* A and B, i.e. the set of ordered pairs $\langle a, b \rangle$, where a is in A and b in B
2^A	The *power set* family of all subsets) of set A
$\lvert A \rvert$	The *cardinality* (numerosity) of set A
$\mathbb{N}$	The set of *natural numbers* (0, 1, 2, . . .)
$\mathbb{R}$	The *real line*
$\mathbb{R}^+$	The set of *non-negative real numbers*
$f: A \to B$	The *function* f maps set A into set B
$f(x)$	The *value* of function f at x
$F = \langle F_1, F_2, \ldots, F_n, \ldots \rangle$	A state function for some system
R & D	Research and development
S & T	Science and technology

PART IV

UNDERSTANDING AND CHECKING

CHAPTER 10

UNDERSTANDING

We do a number of things, from eating to learning to write, without understanding what we are doing. Automatic machines and computers, too, perform their task without understanding. But their designers, repairmen and users are supposed to understand, at least to some extent, what they do. All intellectual and technical operations require some understanding unless they follow routines; and even when they do so the control of the results requires some understanding of the nature, goal and value of the operations.

Understanding is not an all-or-none operation: it comes in several kinds and degrees. Even after studying a subject for decades we may feel that we can still make progress at understanding it: we learn to see it in different ways, to relate it to different themes, or to use it in different ways. Until a time may come when, having understood the subject fairly well, we realize that it is error ridden or has become obsolete.

Consider the following imaginary dialogue. "What is new?—President *X* died.—How, when, where?—He was murdered yesterday at *Y*.—Why?—The inquiry is still under way." The point of this dialogue is to note three different and successive stages in any thorough inquiry into factual matters, namely finding the *what*, the *how*, and the *why*—or information, description, and explanation respectively.

A set of raw data tells us that something is or is not the case. A description tells us how the fact has come about. An explanation is one more step in the process of inquiry. It involves conjecturing mechanisms of some kind—causal, stochastic, teleological, or a combination of these—underlying the facts being described. In the above example any of the following may turn out to be part of the correct explanation. The assassin was crazy; he wanted to attain notoriety; he was an enemy agent; he had a grudge to avenge; he regarded the victim as public vermin; he wanted to replace the victim; he wanted to trigger an uprising. Any of these statements, though schematic, goes beyond the mere description of the fact: it is a hypothesis. By adopting any of it we may expedite or block the research and thus favor or hinder the process of refining the original description.

We may understand or fail to understand the what, the how, and the why of a fact, and we may do so in different manners and to different degrees. But, in all cases of genuine knowledge, knowing that something is (or could be) the case precedes knowing how, which in turn precedes knowing why. (This does not hold for ideological explanation, which dispenses with inquiry altogether.) It follows that explanatory knowledge is superior to descriptive knowledge, which is in turn superior to mere information. That is, knowing why implies knowing how, which in turn implies knowing that.

To admit the above involves adopting an epistemology at variance with empiricism, which counsels description and discourages explanation. By the same token it involves sharing the belief, widespread among scientists, technologists, and humanists, that explanation is a goal of research as distinct from mere inquiry, which usually stops as the how. But admitting all this is of little value unless we know what is explanation. We shall therefore undertake to examine explanation.

1. Understanding and Explaining

1.1. *Modes of Understanding*

We can understand, or misunderstand, objects of various categories: facts, signs, and constructs. Thus we can understand what happens around us, we can understand a sentence, and we can understand a hypothesis. In principle anything can be the object of understanding—even non-facts such as travel at superluminal speed and the adventures of disembodied spirits. Nor are the understandings of objects of different types unrelated. Thus to understand a fact one needs to know other facts and some hypotheses. And to understand a theory one needs to understand some of the sentences in which its formulas are expressed.

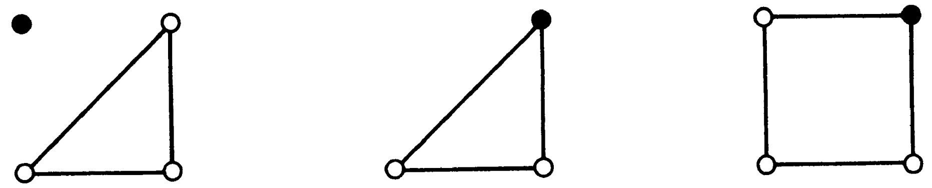
One may understand in different ways and degrees. One may understand with the help of words or diagrams, examples or analogies, empathy or hypotheses. To be sure, these various modes of understanding are inequivalent, but the point is that they constitute understanding even if not all of them amount to genuine explanation. Thus we may understand the apparently odd behavior of someone else if we make an effort to put ourselves in his shoes: this is an act of empathic understanding or *Verstehen*, made possible by the basic similarity of all human brains. We understand *A*'s dislike for *B*'s philosophy when informed that B stole A's bride (or hypothesis or experiment). Dalton understood atomic combinations in terms of tiny hooks. Faraday understood electromagnetic induction in terms of elastic tubes. Darwin understood natural selection by likening it to economic competition.

Metaphorical explanation is not limited to lay discourse: one often finds it in the beginning stages of scientific and technological developments, even in sophisticated fields such as many body physics. Thus consider a network of atoms or molecules. One of them absorbs a photon and so gets excited, i.e. jumps to a higher energy state. This excitation, an event, is reified into an "exciton", which is treated *as if* it were a particle. Indeed, when the excitation propagates to a neighboring atom or molecule, the exciton is said to pass on from one atom (or molecule) to the next. Since this hopping goes on at random, it is treated *as if* the exciton "were enjoying a random walk through the lattice", as one author puts it. (I.e., the metaphorical exciton is likened to a drunkard—a metaphor of a metaphor—so that the theory of random walk can be used.) Eventually the exciton ceases to wander: this event is treated *as if* the exciton had fallen into a trap ("trapping center") where the exciton triggers a chemical reaction. The entire metaphor has as much heuristic value as Faraday's analogy between the electromagnetic field and the elastic solid. And in both cases this heuristic scaffolding is eventually dismantled: only the equations remain, which are interpreted in a different (literal) way. A new, superior, mode of understanding is attained.

In any of its modes understanding involves systematizing—or, as Piaget would have said, assimilating. Either we fit the given item into our pre-existing cognitive or epistemic network, or we transform (e.g. expand) the latter to accommodate the new item. See Figure 10.1. Needless to say, this is a metaphorical explanation of the phenomenon of understanding. Like some other metaphorical explanations, it may prove to be of heuristic value: it may suggest a precise formulation in terms of changes in the state space of a psychon or plastic neural system.

Every student is familiar with the phenomenon of the growth of understanding, which at times is gradual and at other times sudden, and at all times is a matter of degree or level. The first reading of a paper may give him a dim understanding, a second a deeper understanding, and a third, perhaps apparently unrelated experience, may show how everything falls into place. We understand a thing or a process if we are given a description of it; we understand it even better if the description follows from some mechanismic model.

What holds for individual learning holds, *mutatis mutandis*, for the social



(a) (b) (c)

Fig. 10.1. Understanding as systematization. (*a*) Before understanding item symbolized by black dot. (*b*) Item is understood by fitting into pre-existing epistemic network. (*c*) Item is understood by transforming epistemic network.

process of growth and diffusion of knowledge. Along the history of any epistemic field metaphors are replaced by literal descriptions, which are systematized into black boxes, which are superseded by mechanismic theories, which in turn make it possible to refine descriptions. A few historical examples will help us understand this point.

Take for instance solidity: we understand it on being told that the inter-particle distances in a solid are constant. If we now ask why those distances are constant, we demand a theory involving inter-particle forces, i.e. a mechanismic or dynamical theory. Take disintegration next: an exponential decay curve, or the corresponding equation, answers the how-question but does not explain why there is disintegration rather than stability. An explanation requires a theory involving the hypothesis that special forces are involved. Or take high energy processes such as the high speed collision of protons with neutrons and the resulting ménagerie of particles. Until recently physicists were satisfied with data fitting and with black box models capable of absorbing and occasionally predicting large amounts of data but incapable of explaining them. The currently popular theories go far beyond that phenomenology and hypothesize that many a so called "observed particle" is a system—e.g. one composed of quarks and antiquarks bound together by peculiar forces.

Nor is the above pattern of progress restricted to physics. Chemistry has long ceased to be a huge catalogue of reactions and compounds. We know now the composition and structure of millions of compounds, and are beginning to explain their very existence in terms of forces and collisions. Likewise biochemistry is advancing from description to explanation. At the time of Claude Bernard biochemistry was mainly concerned with establishing balance sheets, i.e. with relating what the organism consumes with what it excretes. For example, the amount of protein consumed was correlated with the amount of urea and creatinin in urine: the intermediate processes were unknown. Nowadays a number of metabolic pathways have been uncovered, and many biosynthesis processes, such as photosynthesis, have been analyzed. The same holds for the adjoining field of genetics. Classical genetics treated the organism as a black box and, accordingly, performed observations and experiments on the whole organism level—e.g. hybridization experiments. With the discovery and chemical identification of the gene, and particularly with the formulation and confirmation of the double helix hypothesis, the genome became a grey box. True, biochemistry and genetics are still primarily structural and kinetic, and only marginally dynamical. But it is likely that more and more dynamical (mechanismic) models, explaining the known structures and processes, will be proposed as theorists invade the field in increasing numbers. As Francis Crick, of double helix fame, once said, "One man's black box is another man's problem".

Biology is no exception to the general trend from description to explanation. True, much of biology is still descriptive and classificatory, and talk of information flow, and even of goal, often passes for explanation. Still, all biologists pay at least lip service to the synthetic theory of evolution, which is a mechanismic theory, and acknowledge that biological research cannot go far without physics and chemistry in the search for the mechanisms of processes. An increasing number of biologists are working on mathematical models of biological processes on the various biolevels, from the cellular to the ecosystemic. All biologists realize that, in order to know how a bird flies, it suffices to film it in flight; but that, if we wish to understand how it manages to fly, we must create and test models of the mechanics and physiology of bird flight. Likewise psychologists are beginning to learn that behavior, far from explaining, is the thing to be explained: that there are no kinematical explanations but only kinematical descriptions. In particular some of them are learning that only physiology and sociology can explain human behavior. At the same time historians are realizing that chronicling does not suffice: that we can understand historical processes with the help of disciplines other than history, in particular psychology, demography, sociology, economics, and politology. Moreover they have learned that consideration of these disciplines elicits the search for new data as well as the framing of descriptions that would otherwise not be forthcoming.

In sum, in all epistemic fields the tendency has been to advance from description to explanation, from black to grey to translucid boxes, from the ledger to the clockwork. This is no accident but an indication that, although we can understand in various ways, only a mechanismic (or translucid box) theory can give us a satisfactory understanding of things, for it alone can tell us what makes things tick. Not that the above mentioned trend is inevitable: like every other trend it might be reversed. As a matter of fact the current computer fad is already distorting that trend: the lust for computation and simulation dries up the passion for explanation. To be sure we need both computation and simulation, but neither is enough, for neither brings insight. (In particular, the numerical solutions to equations are unwieldly and hard if not impossible to interpret.)

1.2. *Subsuming and Explaining*

Let us analyze three epistemic operations that, though distinct, are often confused: description, subsumption, and explanation. From a logical viewpoint a *description* is an ordered set of factual statements. For example, "The car came at high speed, crashed against the fire hydrant, and stopped". (If the data came in reverse order, the description had to be conjectured.) Given that description we may understand some of the related facts, such as the final state of the driver, the car, and the fire hydrant—but this only because we use tacitly certain generalizations about the effects of impact. But the description does not explain why the driver crashed in the first place: was he drunk, was he sick, did the car have a mechanical defect, was the pavement slippery? Only further data and hypotheses can answer the why-questions.

A *subsumption* too is an ordered set of statements, but one such that the last statement follows from the preceding ones. For example, suppose we want to account for the fact that a given population of bacteria has increased eight-fold in the term of one hour. We investigate the matter, counting (or sampling) the population every so often, and come up with a table or a graph population *vs.* time. Suppose we find that the growth pattern is 1, 2, 4, 8, 16, 32, 64, etc., at 20 minute intervals. This accounts for the fact to be understood, since we recognize it as the 4th member of the sequence. In other words the fact has been *subsumed* under a general pattern, namely: The given population of bacteria doubles every 20 minutes. In other words, we have turned the fact to be explained into a particular case of a law statement.

The inference has been this:

For every x and every t, if x is a population of bacteria of species A in condition B, then the population of x at $(t + 20)$ min is twice the population of x at t. [In general, $N_t = N_0 2^t$, where N_0 is the initial population and t is the number of 20 min periods.]

b at $t = 0$ is a population of N_0 bacteria of species A in condition B.

b at $t = 60$ min is a population of $N_3 = 8N_0$ bacteria of species A in condition B.

Generalizing, the basic subsumption schema is this:

Pattern	*For all x, if Fx then Gx*	*Deduction* ↓
Circumstance	*Fb*	
Given fact	*Gb*	

Sometimes the pattern occurring in a subsumption is merely a systematic or classificatory statement. For example, "Johnny does X because he is (in the class of children who are) 7 years old" is typical of Piagetian psychology. It accounts for a property of an individual in terms of its belonging to a certain kind. And "Helium has a very low reactivity because it is a noble element" accounts for a property shared by all the members of a species by including the latter in a certain genus. In both cases the inference patterns are strictly deductive:

Explanation of individual property in terms of belonging to a species

Premise 1: Definition of species S by the characteristic properties A, $B, \ldots, X, \ldots$ of its members.

Premise 2: b belongs to S.

Conclusion: b possesses property X.

Explanation of species property in terms of inclusion in a genus

Premise 1: Definition of genus G by the characteristic properties M, $N, \ldots, Y, \ldots$ of its members.

Premise 2: S is included in G

Conclusion: Every member of S possesses property Y.

We understand the given fact as a particular case of a general pattern. Still, we do not understand the pattern itself, and therefore the fact to be accounted for remains half understood. We are told how things are, not why they should be the way they are. From a logical point of view bacteria might grow in many different ways or in none. But microbiological research reveals that bacterial colonies happen to grow by division. In our example, every bacterium of species A divides at the end of 20 min; in turn, every daughter bacterium divides after 20 min, so that the original bacterium has been replaced by four, and so on. This then is the (main) growth mechanism in the case of populations of bacteria (and other unicellular organisms): cellular division. (To be sure it is a nonmechanical mechanism.) We have now attained an *explanation* proper, or mechanismic account.

The logical reconstruction of the explanation process is this. From the observation and timing of cell division we hypothesize (and check) the law of growth, namely $N_t = N_0 2^t$. (We can obtain this hypothesis by a rigorous application of the mathematical principle of complete induction. But of course this is no proof that bacteria do grow that way. Only experiment can tell whether the hypothesis is true. In fact we know that it fails for high population densities, i.e. where there is strong competition for resources.) The rest follows like in the case of subsumption.

The difference between subsumption and explanation is not logical: both are cases of deduction from regularities and circumstances, in particular law statements and data. The difference is another one. Subsumption answers only how-questions, explanation how-or-why-questions, such as 'How does this machine work?' and 'Why did this machine go out of order? Both answers or accounts are given in terms of patterns, such as trends, empirical generalizations, or law statements: the given fact to be accounted for is shown to be a particular case of such pattern. But whereas in the case of subsumption the pattern itself remains unaccounted for, in the case of explanation it is a mechanismic hypothesis or theory.

The basic logical pattern of subsumption was

(1) *For all x, if Fx then Gx, Fb* $\vdash$ *Gb.*

The corresponding pattern of explanation is this one:

(2) *For all x, if Fx then Mx, For all x, if Mx, then Gx, Fb* $\vdash$ *Gb,*

where 'M' symbolizes some mechanism such as cell division. (For example, the first premise could be "For every x, if x is a bacterium, then x reproduces by cell division", and the second "If x reproduces by cell division, then the offspring of x grows geometrically".) Now, the two general premises of (2) jointly entail the general premise of (1). Phenomenalists, conventionalists and simplicists would say that this shows that 'M' is dispensable or replaceable. A realist concludes instead that *explanation subsumes subsumption*, logically, epistemologically and ontologically. Logically because, given an explanation, we can detach the corresponding subsumption. Epistemologically because explanation presupposes more knowledge than subsumption. Ontologically because explanation goes deeper into the matter than subsumption, by pointing to some (conjectured or established) mechanism that may be hidden to the senses. Let us dwell for a moment on the epistemological and ontological aspects of explanation that we have just noted.

That explanation involves subsumption is suggested, though not proved, by the following examples. (*a*) Knowing that the mechanism of radio communication is electromagnetic field propagation, we know how it can be produced, how it proceeds, how long it takes—hence when the signal is to be expected—how it may be detected, amplified, etc. (*b*) Knowing that the mechanism of heredity is summarized in the vicissitudes of the parental DNA molecules, we know in principle how hereditary traits appear and fail to appear, and how they could be modified. (*c*) If we knew the neurophysiological mechanism of problem solving we would be able to solve a whole cluster of problems, such as: What particular neural systems work out the problem?, How does the problem solving process proceed, and When do the various stages in the process occur? In sum, knowing the most involves knowing the least. Hence descriptivism—the epistemology that extols description and decries explanation—hampers the advancement of knowledge.

The relations between explanation and ontology are as follows. Firstly, the quest for explanation is a quest for knowing deeper and deeper levels of reality. (For the concept of a level see Vol. 4, Ch. 1.) Secondly, the very choice of a type of explanation presupposes some ontology or other. Thus a theological world view suggests explaining natural and social facts ultimately in supernaturalistic terms, or even holding that they are unexplainable. A physicalist (or vulgar materialist) ontology suggests looking for explanations in physical terms even in the case of biological, psychological, and social facts. On the other hand, emergentist materialism (Bunge, 1981a) acknowledges the emergence of qualitative novelty and it encourages its explanation. (Holism denies that such explanation is possible, and reductionism denies emergence altogether.) Thirdly, to explain a fact is to show how it comes about, not to eliminate it. Thus to explain an illusion is to disclose its neurophysiological mechanism, not to show that it did not occur to begin with. (On the other hand Coren and Girgus (1978, p. 23) hold that "Ultimately, when we know exactly how the visual system works, visual illusions should no longer exist".) In sum explanation, unlike definition, does not eliminate. And inexplicability (or mysteriousness) does not define. For example, *pace* numerous philosophers, "emergence" is not definable as unexplained novelty, and "illusion" is not definable as unreality.

An explanation is an epistemic process involving three components: (*a*) an explainer or animal doing the explaining; (*b*) the object(s) of the explanation, i.e. that which is explained, and (*c*) the explanatory premises or *explanans*. All the explainers we know are humans: we do not know whether any other animals explain, and we will never know unless the question is seriously posed. (True, we often say that a hypothesis or a theory explains such and such facts. But this is by way of metaphor: we mean that a knowing subject explains facts with the help of a theory and data.) The objects of explanation are facts. And the explanatory premises are the hypotheses and data involved in the explanation.

What can be objects of explanation? A sybilline answer is: Anything problematic. But of course what is problematic depends on the general framework as well as on the state of knowledge. Thus a philosopher obsessed with nothingness finds it natural to ask why there is something rather than nothing, whereas a scientist takes the universe for granted and asks instead why things change the way they do, as well as why certain things seem not to change at all. Neither believers nor atheists ask why there is a deity: the former because they take it for granted, and the latter because they deny its existence. The evolutionary biologist asks why the known species have the properties they have, whereas the creationist has no such problem. The nonconformist asks why we should keep certain institutions which the conformist does not question. In sum, each world view encourages the search for certain explanations while discouraging others.

Aside from considerations about world views, the objects of explanation can be things, properties or states of things, or events (changes in things). A (concrete) *thing* is explained if a possible process is found or conjectured that ends up in the emergence of the thing—e.g. the formation of a molecule or of a social system. (Accordingly the stable elementary particles, such as the electrons, are not explainable except when they result from processes such as nuclear disintegration or high energy collision.) Likewise a *property* is explained in terms of some process of qualitative change—e.g. the emergence of the questioning ability during human development. (Accordingly the basic properties of matter, such as that of possessing energy, are not explainable.) And an *event* is explained in terms of forces or of other events, e.g. collision or random shuffling. (Accordingly the uniform motion of a body or of a photon in a vacuum are not explainable: only their origin and their eventual deviation from uniform motion are explainable.)

(Note that we have not included statements nor, a fortiori, sets of statements, among the objects that can be explained. To be sure a statement, unless it is an axiom or a definition, may be deduced from other statements. In particular law statements can often be deduced from higher level law statements. But we had agreed to restrict the meaning of "explanation" to subsumption *cum* mechanism. To be sure, sometimes we deduce law statements from higher level formulas, but not every such deduction amounts to an explanation of the corresponding pattern. For example, every equation of motion and every field equation is deducible from some variational principle, but such deduction does not explain motion or field propagation. On the other hand the deduction of a "deterministic" equation of motion or some other pattern from probabilistic laws does amount to an explanation of the "deterministic" pattern. Likewise we explain behavioral regularities in neurophysiological terms. In sum, deduction may or may not correspond to explanation.)

Obviously not everything explainable is worth explaining. The value of an explanation depends on the world view, the state of knowledge, or practical considerations. For example, sometimes we need to explain regularity—e.g. why Johnny has above-average school attendance; at other times anomaly—e.g. why Johnny, who so rarely misses school, is missing today. When we know or suspect that a given process is causal, the observation of accidental departures cries for explanation; but if the process is assumed to be random, then we must explain its regular features. And whereas some students take certain data for granted, others try to explain them. Different people itch in different places at different times.

We have drawn a sharp distinction between explanation and subsumption, suggesting that the latter is little more than description. This distinction is not made in the philosophical literature (except for Bunge, 1967b). The reason for this is that most philosophers have focused on the logical form of explanation, disregarding its epistemological and ontological aspects. This neglect has suggested to some philosophers that the standard view on explanation, found in Popper (1959) and Hempel (1965), is not just incomplete but basically mistaken. In particular it has been suggested that explanation is not a hypothetico-deductive operation but consists in drawing analogies or metaphors, or in empathizing with the actors of the psychological or social phenomena to be explained. These criticisms rest on a confusion between the psychological category of understanding and the methodological category of explanation. As noted in Section 1.1 we may indeed understand via metaphor or *Verstehen*, but explanation is different.

What is true is that most of the explanations offered in ordinary life and even in science and technology are incomplete, and so seem not to match the hypthetico-deductive model. In fact they presuppose (assume tacitly) a number of hypotheses. The most famous of all such incomplete (or enthymematic) arguments is Descartes' "I think, hence I exist (I am alive)". The tacit premise here is "All thinking beings are alive", which, together with the explicit premise "I think", yields the conclusion "I am alive". So although it looks like a proposition it is actually an abbreviated argument.

Incomplete explanations, or *explanations in principle*, abound in all fields of inquiry. Thus the technologist is often content with pointing to "the principle" of his design: he invokes a law statement, or a theory, to explain in outline why his artifact should work. The economist who succeeds in explaining an economic trend does so in principle, not in detail, because his equations (even supposing they were accurate) contain far too many unknown parameters (Hayek, 1955). Likewise the evolutionary biologist can explain in principle the evolution of some biopopulations with the help of the general principles of the synthetic theory of evolution. A full explanation of the evolution of any particular biopopulation would require a precise mathematical model involving mutation probabilities, viabilities, and environmental factors such as the competing populations. However, even a partial explanation is, or can be cast in the form of, a deductive argument: *Pattern and circumstance, ergo explanandum* (fact to be explained).

We conjecture that every explanation, if properly analyzed, will prove to be *incomplete* in failing to explain, with total accuracy, every feature of the object of explanation. This is but one aspect of the incompleteness of all our factual knowledge. Irrationalists should derive no comfort from this postulate, for we also conjecture that every explanation is *perfectible* with the help of further research. Such research generates *explanation chains* of the form *A because B because C* ... *M because N*. For example, a girl goes to school because she wants to learn. She wants to learn because she needs a good job. And she needs a good job because her parents cannot support her. An explanation chain is a partially ordered set of explanatory statements of the form "X because Y". And, as we saw a moment ago, every statement of this type is an enthymeme analyzable into an argument.

Is there a limit to every explanation chain? That is, are there ultimate facts such as Aristotle's Prime Mover? We have no evidence for such ultimates. All we know is that no explanation satisfies forever. For example, Newton explained the planetary orbits in terms of inertia and gravitation. His adversaries, in particular Leibniz and the followers of Descartes, were dissatisfied with Newton's hypothesis of gravitational interaction at a distance: they wished to explain gravitation itself. This explanation came much later with Einstein's field theory of gravity.

Still, there are dogmatists who proclaim that whatever we cannot explain satisfactorily now will remain mysterious forever. One such mystery would be the precise conditions of the "big bang", for the evidence needed to investigate the cause of the primal explosion was destroyed in the explosion itself (Jastrow, 1978). Yet it may well happen that such evidence will eventually be discovered, or even that evidence against the "big bang" hypothesis will be produced. Another alleged perennial mystery is that of consciousness (Eccles, 1978). It is more fruitful to assume instead that this problem had been approached in the wrong ways, namely via theology, philosophy, and mentalistic psychology rather than neuropsychologically. Even if no satisfactory solution to the present mysteries were forthcoming, we would learn more by investigating them than by declaring them to be insoluble mysteries. Research pays more than dogma.

Explanation dissolves mystery but need not remove marvel. The widespread fear that scientific explanation may suppress marvel and our capacity to enjoy nature, art, and human relations, is groundless. On the contrary, (*a*) searching for explanation and finding it is in itself an enjoyable experience; (*b*) by expanding and deepening knowledge, scientific research allows us to marvel at things and processes that were unsuspected before; and (*c*) the practical fruits of science allow millions of people the leisure necessary for enjoying nature, art, and human relations.

Nor does explanation reduce the unfamiliar to the familiar. On the contrary, science explains many familiar phenomena, which we take ordinarily for granted, in esoteric terms. Think of the way meteorology explains the familiar facts of weather, the way chemistry explains the transmogrification of food in the kitchen, the way psychology explains perception, memory and thought, or the way economics attempts to explain inflation. It all sounds, to the layman, very much like the *explicatio obscurum per obscurius* that the modern philosophers attributed to the schoolmen. But at least it is not an *explicatio ignotum per ignotius*: on the contrary, we can now explain the unknown in terms of the known or conjectured. Moreover, obscurity is relative to the subject: in principle anyone can master the technicalities of science. (By the way, the fashionable claim that the use of technicalities by scientists is a manifestation of elitism, which a science for the people should avoid, is sheer obscurantism. There is no deep knowledge without tears. The remedy is not to degrade science but to upgrade education.)

Another widely held belief is that an explanation is nothing but an answer to a why question. Let us test this account with a simple example: Why do people who walk on snow wear snow shoes? Answer: So as not to sink. No doubt, this answer is correct—but it does not explain anything. An explanation requires a more complete answer, such as this one. One sinks in snow the deeper, the harder one presses on it; snow shoes, by distributing the weight over a greater area, decrease the pressure and so also the sinking. In short, although every explanation answers a why (or a how) question, the converse is not true. Moral: Ordinary language is no substitute for conceptual analysis.

We wind up this section with a handful of maxims or rules for explanation.

R1: *Before rushing to explain a fact make sure it is fact not illusion.* (E.g. before explaining spoon bending at a distance establish that it is a fact not a fraud.)

R2: *Explain existents by existents* (established or conjectured) never by nonexistents. (Hence mistrust the explanation of elementary particle events in terms of virtual particles or processes, and of human behavior in terms of disembodied souls or immaterial processes.)

R3: *Explain the observable by the unobservable* (e.g. color changes by chemical reactions) *or the unobservable by the observed* (e.g. societal facts by individual behavior) instead of keeping them separate.

R4: *Mistrust ad hoc explanations*, i.e. explanations with the help of hypotheses that cover only the fact to be explained.

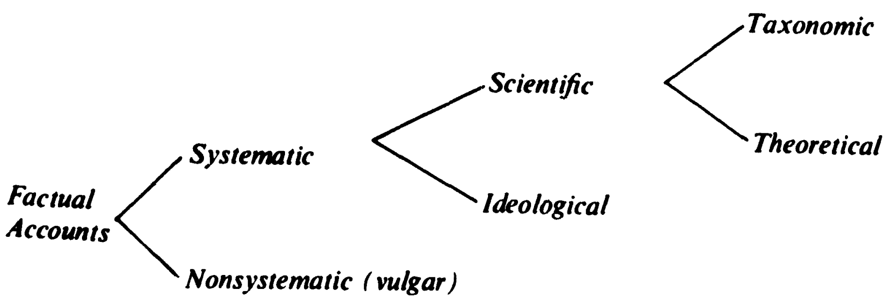
R5: *Mistrust hypotheses and theories that purport to explain everything* (such as psychoanalysis, which professes to explain all human life, and psychoneural dualism, which professes to explain all neural and mental processes).

Only one type of explanation satisfies all of the above rules, namely that performed with the help of scientific theories. Let us then have a look at it.

2. Systematic Account

2.1. *Vulgar, Ideological, and Scientific Accounts*

Descriptions, subsumptions, explanations, and forecasts will be called collectively *accounts*. Typically, accounts involve hypotheses as well as data; and, whereas some hypotheses are testable and even true, others are just myths. An account can be systematic or nonsystematic. A *systematic account* is one involving some conceptual system: a classification, a theory, or part of either. The system may be scientific or ideological (religious or sociopolitical). If the former, the account will be called *scientific*, if the latter, *ideological*. A nonsystematic account employs only ordinary knowledge (true, half true, or sheer superstition); it may also be called *vulgar*. In short, we have the following classification of accounts of matters of fact:



Examples of each of these categories abound. Thus when explaining a young man's absent-mindedness by his being deeply in love we may be right, but such explanation is nonsystematic: it makes use of a piece of popular wisdom ("People in love tend to act absent-mindedly"), not a conceptual system. The fact that some children resemble some of their long deceased ancestors can be explained either ideologically, by reincarnation, or scientifically, by the genetic hypothesis of recessive genes. Survival after death may be accounted for by the doctrine of the immaterial and immortal soul. In this case there is no rival scientific account, for there can be no evidence for survival after death: to science this is not a fact, hence it does not call for an explanation. What does call for a scientific account is the origin and persistence of the belief.

The typical vulgar account is *ad hoc*, i.e. concocted for the occasion, and *unifactorial*, i.e. it involves a single factor or variable. On the other hand every scientific account is multifactorial, i.e. it involves at least two variables. The reason for such multifactoriality is that every variable is related to at least one other variable, and usually to more than one; so, any change in one of them is bound to affect others. However, because unifactorial accounts are the simplest and have the greatest (though illusory) unifying power, even scientists fall occasionally for them. One instance is the attempt to explain all behavior and all thought in terms of language (Luria, 1961). Another is the cognitivist account of all cognition as rule-directed computation of the same kind as that performed by computers (e.g. Pylyshyn, 1980). A third example of a unifactorial account is the explanation of social change, hence of history, in terms of imitation or innovation, class struggle or political strife, population growth or environmental change, the power of ideas or that of rulers. Seasoned historians know that history is far too complex a process (or bundle of processes) to be explainable by a single factor. They know that social change may start in the economy, or in the culture, or in the polity, and propagate to the other subsystems of society: they know that there is no absolute prime social mover. Likewise biologists explain evolution in a multifactorial fashion: in terms of chance genic variations, geographical isolation, climatic change, ecological catastrophes, natural selection, etc.

Ideological accounts too tend to be unifactorial, but they are all-embracing rather than *ad hoc*: because of this simplicity and unifying power they are as dangerous as they are attractive. Thus political credo X-ism blames all the evils of society on anti-X-ism, and conversely. Astrology attributes every event in human life to the stars. Psychoanalysis blames every psychological trouble on early sexual experiences, whether real or imaginary. Interactionist mind–body dualism (e.g. Popper and Eccles, 1977) accommodates every possible mental event, and so dispenses with all specific theories (of e.g. perception, memory, imagination, and thought) as well as with experiment. (But it explains nothing, for it just states—in an inexact way to boot—that mind and body interact in such a way that they pass one another the buck, without suggesting any interaction mechanism.)

The believer in an ideology has the great advantage, over the vulgar, of being able to offer a single account of every single fact, and of finding it ready made in his system instead of having to search for it. In an open context, on the other hand, every fact can be accounted for in infinitely many ways. Thus if a fact described by a proposition q is observed to happen, then we can deduce that q because p, where p is an arbitrary proposition. (Indeed, the inference "q entails If p then q" is valid in ordinary logic.) Not so in a closed context: here only propositions logically and referentially germane to the given proposition q can be invoked. Moreover in a theory, whether ideological or scientific, there is always at most one explanation for each observation. Therefore, paradoxically, not only the ideologist but also the scientist are spared the agonizing uncertainties of the vulgar: the former because he knows all the answers in advance, the latter because he is confident that he, or someone else, will eventually come up with a satisfactory answer if he investigates the matter. Both are sustained by faith—irrational in the first case, rational in the second.

Scientific accounts are or tend to be systematic and multifactorial, and they avoid the two extremes of the *ad hoc* (or single case) and the all-embracing accounts. Of course there are some extremely general scientific systems, such as the relativistic theory of gravitation and the neo-Darwinian theory of evolution. However, they do not embrace every possible fact in all possible domains, and they do not propose essentially the same account of every fact. (Anything that purports to explain everything actually explains nothing.) Indeed only very specific theories, i.e. models, can account for facts without much further ado. Any account of specifics done with the help of a general scientific theory requires enriching the latter with subsidiary assumptions and data. Let us make a quick study of three kinds of scientific account: description, subsumption, and explanation; prediction will occupy us in Section 4.

A description will be said to be a *systematic scientific description* if it is couched with the help of a scientific system. In particular, a *scientific taxonomic description* is made with the help of some scientific classification, such as one of elementary particles, plants, or economies. *Example*: "Humans evolved from more primitive primates about three million years ago". A *scientific theoretical description* is one made with the help of a scientific theory. It consists in specifying the values of the state variables of a thing as hypothesized by the theory. *Example*: "The electromagnetic wave, of 1 cm long wavelength, hit the two slot screen; the waves emerging from the latter interfered with one another, and were absorbed by a second screen lying 10 m behind the first". Contrary to the empiricist prescription, scientific systems are not built on presystematic descriptions: on the contrary, scientists describe their facts with the help of their conceptual systems.

Caution: Not everything that sounds theoretical is so. Thus over the past few decades a number of scientists have tended to account for facts in the language of the statistical theory of information. They talk about information flows between molecules, the quantity of information locked in a gene, the way the brain processes the incoming information, information networks in society, and so on. Such accounts do not really use the statistical *theory* of information but only its *vocabulary*. In fact, typically they do not identify the sender, the channel and the receiver; they do not assume that the channel is subject to random perturbations; and they do not calculate or measure quantities of information. (As one biologist put it, "it is doubtful whether information theory has offered the experimental biologist anything more than vague insights and beguiling terminology": Johnson, 1970.) Such accounts are therefore *pseudotheoretical*. To be sure they had initially, particularly in genetics, neuroscience, and psychology, some heuristic value. But they have now become obstacles to theorizing in those fields, because they suggest that the theory—namely the statistical information theory—is already there. Besides, even if one were to make use of information theory in those fields one would achieve at most an exact description: information theory is far too general to explain or predict anything. The same holds for other general system theories: the very notion of a general system theory of a particular system, such as a cell, a lake, or a school, is self-contradictory. An extremely general theory is at best a basis for building a specific theory or model. (Recall Ch. 9, Section 1.2.)

Let us now turn to subsumption. As we saw in Section 1.2, a fact (or the proposition describing it) is subsumed under a pattern (or the corresponding proposition) if shown to be a particular case of it. A subsumption will be said to be a *systematic scientific subsumption* if the subsuming construct is a scientific system, such as the periodic table of the elements or the input–output model of an economic system. In particular, a *scientific taxonomic subsumption* is one where the subsuming construct is a scientific classification. *Example*: "Copper, silver and gold are simple metals". A *scientific theoretical subsumption* is a subsumption under a scientific theory, i.e. such that the explanatory premises are theoretical formulas and data. For example, every instance of light refraction falls under Snell's law. In particular this law, together with the values of the angle of incidence and the refractive indices of the two adjoining media, entails the value of the angle refraction and thus accounts for it. But, because the Snell law is phenomenological (or black box), nothing gets properly explained in this way: the account looks very much like a description, except that it involves a deduction.

(The logic of subsumption boils down to the following. Let $\mathcal{T}$ be a scientific theory in a context $\mathcal{C} = \langle P, Q, R \rangle$, i.e. such that (*a*) the set of formulas of $\mathcal{T}$ is a subset of P, and (*b*) some formulas in $\mathcal{T}$ refer to entities in R. Further, let p and e be propositions in P but not in $\mathcal{T}$, and such that (*a*) p describes a fact or class of facts f involving entities in R, and (*b*) e is a bit of empirical evidence relevant to p (and therefore referring to R's or to things, such as measuring instruments, that are somehow connected with R's, as is the case with telescopes aiming at stars). We stipulate that $\mathscr{T}$ *subsumes* f iff there is in P, but not in $\mathscr{T}$, a (possibly empty) set S of subsidiary assumptions such that p follows from $\mathscr{T}$, S and e.)

The fact being subsumed may be some thing being in a given state or undergoing a given change. If a whole class of facts is being subsumed, then the proposition that is being derived with the help of the accounting theory is a hypothesis not included in exactly the same form among the general principles of the theory. For example, the various laws of geometrical optics—rectilinear propagation in a homogeneous medium, mutual independence of the components of a light beam, reflection and refraction of rays—are subsumed under wave optics. In geometrical optics the refraction index is an irreducible phenomenological parameter; in wave optics it is a kinematical parameter, namely the ratio of the velocities of propagation in the two media. In electromagnetic optics the refractive index is the former as well as a function of the molar electric and magnetic properties of matter; the index is explained properly only by the theory of the atomic structure of matter.

We construe explanation as a particular case of subsumption, namely as *subsumption under a mechanismic* (or translucid box) *theory*. Indeed we stipulate that a fact or class of facts F is *explained* by a theory $\mathscr{T}$ iff (a) f is subsumed under $\mathscr{T}$ and (b) $\mathscr{T}$ is mechanismic (instead of phenomenological). This definition covers the explanation of factual items of all kinds: properties (hence states) of things, events (changes) in things, and patterns of things, such as trends and even laws. Thus we say that the theory of electron dispersion explains the refractive index of quartz; that the electromagnetic theory of light explains the refraction process; that quantum mechanics explains the formation and breakdown of molecules; and that economic theory is supposed to (though it is doubtful that it does) explain the inflation characteristic of the 1970s.

Note the following points. First, every explanation is a subsumption but the converse is false. Second, explanation is richer than subsumption, which is only a special case of deduction: explanation tells us not only what happens but also how it happens. Therefore not all theoretical models are explanatory. In particular, a mere mathematical representation of a set of facts, even if true, does not explain anything. Third, whereas in physics, chemistry, and biology, explanation is nomological (i.e. involves law statements), from psychology upwards rules and evaluations may be involved as well. Thus we may explain somebody's action A by saying that A

causes B (*law*), the individual knows this and values B (*valuation*), and he has adopted the *rule* or maxim that he will always endeavor to attain what he values. Fourth, theories about matters of fact contain law statements and they may also contain valuations and rules, but they do not contain trends or changes in trends. Far from explaining anything, trends and their changes are objects to be explained in terms of laws, valuations, rules, or circumstances. For example, the steady increase in the international price of crude oil is explainable by the decision of the OPEC countries to stop financing the affluence of the OECD countries. And the growth of the tertiary sector of the economy (i.e. the services) is explainable by the increasing importance of technology in agriculture and industry, as well as by the passing of welfare legislation.

I submit that the above explicates what scientists usually mean by 'explanation', namely an account in terms of some mechanism or other, be it causal or stochastic, physical, biological, social, or other. However, once in a while pseudoexplanations appear in the scientific literature. For example, a biologist may lapse into teleology when forgetting that no modern biological theory (in particular no mathematical model) contains the concept of goal or final cause. Or he may be tempted to explain morphogenesis in terms of "morphogenetic fields", ignoring that, in order to speak literally about a field, there must be a field theory with definite and testable field equations. Likewise the computer scientist and the cognitivist (or functionalist) psychologist may feel that they explain the mind when translating mentalistic psychological descriptions into the language of computer science—e.g. when they translate 'x recalled y' into 'x retrieved y from his memory bank'. Translation into one's professional jargon produces familiarity not explanation.

We have called 'theoretical subsumption' what most philosophers call 'explanation'. The classical account of that operation, though one different from ours, is that of Hempel and Oppenheim (1948), usually called 'the deductive-nomological (or D-N) model of explanation'. It has been criticized in various ways, some correct, others incorrect. Ordinary language philosophers have complained that in ordinary language one often calls 'explanation' statements of the form "q because p", which are not logical arguments and need not mention any law statements, so that they are true of false. This criticism, like that of the use of logic in philosophical analysis generally, misses the point: philosophers of science are interested in a technical concept of explanation, which may be absent from ordinary knowledge considerations but happens to occur in scientific contexts.

A relevant criticism is that the Popper–Hempel–Oppenheim relation of explainability holds between almost any general hypothesis and almost any singular sentence (Eberle, Kaplan and Montague, 1961). Consequently there is no great merit in subsumption—not more than in curve fitting, a close relative. Weightier criticisms than this can be raised against the D-N "model" of explanation. One is that it usually requires the explaining premises to be true. This requirement is unrealistic, for in science and technology total truth is hard to come by: our hypotheses and theories are at best partially true, and sometimes our data too are far off the mark—to the point that we may seek to subsume "facts" that, under closer examination, prove to be nonexistent. (Strangely enough most of the repairs proposed to the D-N account of subsumption keep the condition that the explaining premises be true.) A second defect of the D-N "model" is that it is about an open-context operation: it does not require the explanatory premises to belong to theories (hypothetico-deductive systems). But, as we have seen, in an open context almost anything may be accounted for in a number of alternative ways: it is only relative to a definite conceptual system that one can speak of *the* subsumption, or *the* explanation, of a fact. Thirdly, the D-N "model" does not mention the subsidiary assumptions that occur in almost every systematic account of facts but are not part of the theory itself—e.g. the composition and structure of the system to be accounted for. (This oversight may be due to the well-known fact that most philosophers of science think of classical particle mechanics when they write about theories.)

But the most damaging criticism that can be made of the D-N "model" of explanation is one that is seldom voiced, namely that it is concerned with theoretical subsumption, not with genuine explanation: it ignores the mechanisms of things. In this regard—not with regard to the logical apparatus—it is less satisfactory than the classical account of explanation, from Aristotle to Meyerson (1921). According to this alternative view we explain events by their causes, and if we do not know the causes then we may describe or subsume but not explain. As Whewell (1847, P. I, p. 652) put it, "No sound theory without Aetiology". (For an exposition and criticism see Bunge (1959a).) This doctrine became obsolete the moment probabilistic theories began to invade science and technology. We submit that a genuine explanation is one that invokes some mechanism or other, whether causal or stochastic (probabilistic). We shall devote the next section to these two modes of explanation.

We close this section with some thoughts on the power of theoretical accounts in general, be they descriptions, subsumptions, explanations, or forecasts. Two rival accounts of the same fact, or class of facts, may not be equivalent: one of them may be better than the other. Because rationalists are supposed to justify their valuations, let us explicate the notion of preference involved in the statement schema "Account X of fact f is preferable to account Y of f". We shall say that a theoretical account is the better, the wider its coverage of facts, the more accurate, and the deeper.

(More precisely, let A_1 and A_2 be two accounts the reference classes of which have a nonempty intersection R. We stipulate that (*a*) A_1 has a greater *range* than A_2 iff the hypotheses of A_1 are more general (less *ad hoc*) than those of A_2; (*b*) A_1 has a greater *accuracy* than A_2 iff A_1 involves truer premises than A_2 and, in the case of prediction, A_1 scores better (is better confirmed) than A_2; and (*c*) A_1 is *deeper* than A_2 iff A_1 involves more levels of organization than A_2. These three aspects are mutually unrelated: depth is compatible with either generality or specificity, as well as with accuracy or inaccuracy. The *best* of two roughly equally accurate and equally general accounts is the deeper, and the best of two equally deep accounts is the more accurate and general one.)

2.2. *Basic Explanation Types*

We have stipulated that, unlike a description, a subsumption, or a prediction, an explanation invokes some mechanism or other (Section 2.1). We may distinguish five basic or pure types of mechanism or mode of becoming: causal, stochastic, synergic or cooperative, dialectical or conflictive, and teleological (Bunge, 1981a). These five pure types may combine by twos or more to form a total of 26 mixed types. So, there are at least 31 types of mechanism, or modes of becoming, hence 31 types of theories and models, and therefore 31 types of explanation. This number would decrease if it were proved that some of the modes of becoming which we have regarded as basic are reducible to others; but future research might also increase the number of basic types of mechanism.

It seems safe to assume that the pure types occur in reality only approximately: that only mixed types of becoming occur in nature and society (Bunge, 1959a). For example, the processes accounted for by the quantum theories are causal as well as stochastic, at least wherever forces (or potentials) are involved (Ch. 8, Section 3.1); every realistic theory of systems of any kind must take into account cooperation (in system formation and maintenance) as well as conflict (among system components or between them and environmental items); evolutionary processes involve causation, chance, cooperation, and competition; and human social processes involve all five basic types. However, most theories and theoretical models in science and technology involve only one or two basic types of mechanism. This restriction is sometimes due to the fact that there is often only one, or a couple, of overriding mechanisms; at other times it is merely the result of ignorance or even prejudice. Let us make a quick review of such models and the corresponding explanations.

Causal mechanisms, and the accompanying explanations, are probably the most familiar. There is causation if some properties of a thing change under the influence of external events. The former event (or process) is called 'effect', and the latter 'cause'. (These concepts can be elucidated in terms of the notion of history of a thing: the total effect is the distortion in its history due to external influences. See Bunge, 1982.) The best known example of a causal theory is automata theory. Indeed a deterministic automaton stays in a given state unless it accepts an input, and when it does it jumps to another state. No input, no change of state and therefore no output. (Formally: the next state function M, that maps the set of ordered pairs $\langle$state, input$\rangle$ into the set of states, satisfies the condition $M(s,0)=s$, where s is an arbitrary state and 0 the null input.) Only such changes of state are in need of explanation, and they are explained in terms of inputs and previous internal states. Similarly in mechanics all causes are forces or constraints, and the corresponding effects are deviations from uniform and straight line motion, or changes in inner stresses. Only such deviations, not motion itself, are to be explained. (However, mechanics is not a purely causal theory, if only because, unlike automata theory, it involves self-movement in the guise of inertia: see Bunge, 1959a.) Similar causal or partially causal mechanisms, some mechanical and others not, are hypothesized in all fields of factual research.

All events involving large scale systems have joint multiple causes. There are two ways of disentangling such causal factors, i.e. of imputing the observed or conjectured effect to the various causes at play. One is to "freeze" experimentally all factors but one, by using such procedures as insulation and compartmentalization—which are not always practicable. The other method is to perform a theoretical analysis. For example, the radio waves emitted by radio stars lying behind the sun are deflected by the latter. This deflection is due partly to refraction by the solar atmosphere (corona) and partly to the solar gravitational field. The way to separate these effects is to use the knowledge that refraction depends on wave length, so that the total deflection equals the sum of the gravitational deflection and a wavelength-dependent deviation. By measuring the total deflection for different wavelengths one can identify the gravitational part of the total effect.

The philosophical literature on causality is rather meagre both quantitatively and qualitatively. (For a review see Bunge, 1982.) The two most popular views on causation are the logical and the probabilistic ones, neither of which is adequate. According to the former the causal relation is faithfully represented by a conditional proposition relating the cause to its effect. On this view a cause is just a sufficient (or perhaps necessary and sufficient) condition for its effect. This view is wrong for two reasons: one is that causes are events not reasons, not even sufficient reasons. (See Bunge, 1959a.) Another is that, given any event, one may invent any number of "causes" capable of "explaining" it without violating logic, though in most cases science will be violated. Indeed, a proposition e describing the given effect entails $\ulcorner e$ or not-$c\urcorner$, where c is an arbitrary proposition describing any old "cause"; but $\ulcorner e$ or not-$c\urcorner$ is equivalent to $\ulcorner c \Rightarrow e\urcorner$. So, causal "explanations" out of context are cheap, hence we should not care for them. What science does cherish is systematic or theoretical explanations, whether or not they involve causal laws. In other words, only if $\ulcorner c \Rightarrow e\urcorner$ is a law statement (hence one included in a confirmed theory), does c explain e.

The probabilistic view of causation regards the latter as a particular case of a probabilistic relation. (See Suppes, 1970.) The heart of this view is the definition: "*c is the cause of e* $=_{df}$ The probability of e given c equals 1", or some weaker version, such as "*c is a cause of e* $=_{df}$ The probability of e given c is greater than the absolute probability of e". Two trivial counterexamples are enough to ruin this view. The probability of my dying, given that I was born, equals unity—yet my birth could not possibly be regarded as the cause of my death. Second counterexample: The probability of rain, given a sudden drop in the barometer reading, is greater than the probability of rain *tout court*—yet the barometer reading is just an indicator of rain, not its cause. (For further criticisms see Bunge, 1973e.) In sum, causation is not definable in terms of probability. Nor is the converse reduction of probability to causation possible. What is possible and useful is to relate the two concepts.

We use the concepts of causation and probability on a par in science and technology. Thus geneticists speak of the probability that radiation of a given kind will cause a certain mutation. In the quantum theories we compute not only probabilities of spontaneous (uncaused) events, such as the spontaneous radiative decay of atoms. We also compute the probabilities of caused events, e.g. that a given external perturbation will cause the atom to emit a photon of a given frequency. The concepts of cause and effect are retained in the quantum theories but they are related in a probabilistic, not in a causal way. (Recall Ch. 8, Section 3.1.) In general, in the context of stochastic theories, instead of asking "Does c cause e?", we ask "What is the probability that c causes e?" Correspondingly, pointing to the cause is necessary but not sufficient to explain: we must add the probability of its efficiency. In other words, we need two concepts instead of one: those of cause and of efficiency of a cause, which is in turn reduced to that of probability (that the cause will produce its effect).

A probabilistic subsumption is one from premises at least one of which is a probabilistic law (not just a statistical generalization). Thus given a velocity distribution we can infer the average velocity and the spread around that average. (Actually we can infer infinitely many statistical parameters, namely all the moments of the distribution.) This will not explain anything unless it is explicitly stated that the distribution is one of entities moving randomly. The least a probabilistic subsumption does is to show events of a certain kind to be possible, i.e. to have a nonvanishing probability. Such account is of course far poorer, and therefore less satisfying, than the proposal of a definite random mechanism, i.e. a stochastic explanation proper.

Consider the following examples of random mechanisms. The inner face of a window pane becomes fogged in cold days. This process (or the corresponding proposition) can be subsumed under a rate equation representing the increase in thickness of the layer of water vapor on the pane. But this does not explain anything. The explanation is that the water molecules and droplets move randomly about the room and arrive independently from one another to the cold pane, to which they transfer their kinetic energy. This uniformity, and the huge numbers, explain the uniformity of the fogging. Second example (P. and T. Ehrenfest): A dog ridden with fleas meets a clean dog. After a while each dog has approximately the same number of fleas: the two-dog system has attained the equilibrium state. The explanation is that the fleas jump more or less at random from one dog to the other until, in the end, about equal numbers of fleas jump in both directions. Third case: the nearly equal distribution of sexes in humans and other animals is explained by the chromosome composition and the random shuffling, at fertilization, of the genes composing them.

A last example, which is also one of exactification. We all know that, when a message is transmitted along a noisy channel, such as a long distance telephone line, the receiver is likely to fail to understand some words. As the message is repeated, its intelligibility increases; intuitively one expects its intelligibility to be proportional to the number of repetitions. The stochastic model that explains this process boils down to the following. Suppose a given message is composed of N words, and pretend that all of them have the same probability p of being understood by the receiver. The expected number of words that are identified correctly on a first presentation is Np; consequently the number of failures is $(1-p)N$. If these wrongly understood words are repeated, we may expect to understand, on the average, $p(1-p)N$ of them. Therefore after two presentations the total expected number of correctly understood words will be $pN + p(1-p)N$; hence the expected ratio of understood words to total number of words is $p + p(1-p) = 1-(1-p)^2$. By iteration we find that, after the nth presentation of the message, the cumulative fraction of correctly understood words, or total intelligibility, is $I_n = 1-(1-p)^n$. As suspected, this number is close to np. (An obvious generalization is to unequally probable words. And a deeper model would treat p not as an ultimate but as a function of certain properties of both channel and receiver.)

In all cases of probabilistic explanation what is explained is some statistical regularity, such as an average or a correlation (e.g. a trend). Thus we explain the fact that nearly all individuals in a crowded sidewalk, and nearly all cars on a very busy road, have roughly the same speed, by pointing out that a faster pedestrian (or car) would be stopped, and a slower one would be pushed. (The theoretical problem is: Given the average $\bar{F}$ of a certain magnitude F, conjecture some probability distribution p of F with mean $\bar{F}$. This is an inverse problem and therefore has no unique solution. The direct problem, which has a unique solution, is: Given p compute $\bar{F}$.) What holds for averages holds, *mutatis mutandis*, for spreads and other statistical parameters. In the case of statistical correlations we may try either of the following tactics. One is to assume an underlying causal relation, e.g. via a third factor upon which the two given variables depend. If this move fails one may try a stochastic model including one or more probability distributions allowing one to compute the observed correlations. Comparison with the data will either confirm the model or call for its modification—or for a new, more extended or more precise run of observations. (The correlation may have been accidental.)

Unlike causation and design, chance is often regarded as unintelligible, hence stochastic models as merely temporary devices incapable of explaining anything. Correspondingly only causal (and possibly also teleological) explanations are regarded as genuine. (Thus Salmon (1977, p. 162): "To give a scientific explanation is to show how events and statistical regularities fit into the causal network of the world".) But after more than fifty years of highly successful quantum theories—and thirty of failure to produce any worthy rivals containing only "hidden variables"—it is time to take chance as a basic mode of becoming. In fact these theories, in the realist interpretation, assume that every single entity, be it a particle or a field quantum, possesses ("obeys") objective probabilistic laws, such as position and momentum distributions (Bunge, 1967c). This type of chance is radically different from the accidental crossing of two or more initially independent causal lines—the kind of chance known to the Stoic Chrysippus and inherent in classical statistical mechanics. In the quantum theories we understand events in terms of an interplay of causation and chance.

The philosophical literature on probabilistic explanation is disappointing: it simply does not concern this type of explanation. Thus Hempel (1965, Ch. 12), in a pioneering study on the matter, made an assertion that has been repeated uncritically by a number of other philosophers. He stated that what he called "the inductive statistical" model of explanation differs from the "deductive-nomological" one in two respects. First, the laws are "statistical rather than universal"; second, the explanandum follows from the explanans with high probability rather than deductively. These two propositions are false.

The first proposition involves a confusion between a probability *law*, such as the Maxwell–Boltzmann law or the Schrödinger equation, and a mere statistical generalization such as "f percent of the population of X are illiterate". The stochastic laws occurring in science and technology contain universal quantifiers ranging over their referents, instants of time, etc. (For example, "For all systems of particles in equilibrium, the probability density is proportional to $\exp(-E/kT)$".) As for the second assertion we are discussing, all the inferences made in stochastic theories or models are strictly deductive, for they consist either in proofs or in computations. Thus averages are computed from distributions and the definition of an average. In addition to ignoring the probabilistic laws of science and technology, Hempel has confused probability theory (a chapter of pure mathematics), mathematical statistics (an application of the former), and theories of random processes (distributed among the various chapters of science and technology). I have singled out Hempel's study not because it is the worst but because it is the clearest and it has inspired an uncounted number of equally wrong-headed treatments of an important problem.

Now a few words about synergy and dialectics, of which more elsewhere (Bunge, 1981a). Every process of accretion, clumping, self-assembly, or merger, be it of atoms, molecules, organisms, or social systems, is one of cooperation—mostly unwitting to be sure. Thus we explain the formation of biomolecules light and heavy as a self-assembly process, and in turn explain this process in terms of physical forces and chemical bonds. Likewise we explain the breakdown of systems on all levels in terms of conflicts among their components, or among these and environmental things. In turn, a conflict may be explained or analyzed as some dissociation process such as receding motion or competition for a resource.

Both modes of becoming or change mechanisms, cooperation and competition, coexist and intertwine on all levels (Bunge, 1976). Consider two simultaneous chemical reactions of the forms "$A + B \rightarrow C$" and "$B + D \rightarrow E$". In each of these processes the reactants may be said to cooperate to form the reaction product; but A competes with D for B. Hence this is a case of mixed mechanism. (The entire chemical kinetics of systems of reactions is concerned with cooperation-competition processes.) Certainly, saying that C is the outcome of the cooperation (or competition) of A with B explains the emergence of C, but in a superficial manner. The cooperation (or competition) itself can in turn be explained, at least in principle, in terms of forces, constraints, interests, or what have you.

The last type of mechanism in our list is goal-seeking (or teleological) behavior. Thus when dealing with higher vertebrates we often account for animal A's doing B by saying that B serves to attain C, which we assume tacitly to be of value to A. The explicit argument is: "A expects that doing B will bring forth or favor the occurrence of C. Now, A values C. Hence A does B." This is not a deductive argument. It is not even intuitively correct, for A might abstain from doing B, although it values the outcome C, because it is prevented from acting, or because it lacks the means, the energy, or the courage to act. To be sure, we may complete the account by adding that A is free and capable of and willing to do B to attain C. But even so completed the conclusion that A does B does not follow deductively from the premises. So, teleological accounts are not arguments proper, hence they do not qualify as subsumptions, let alone as explanations. In order to transform them into such we need something like a calculus of means and ends yielding the desired conclusions from premises consisting in law statements, rules, and value judgments. (See Bunge, 1977e.) As long as no satisfactory calculus of this kind is available we should avoid teleological accounts, or at least abstain from attributing to them any explanatory power.

3. Unification

3.1. *Reduction*

Genic variation has been explained as change in the composition or in the structure of genetic material, i.e. DNA molecules. This new account of a previously known process has enabled biologists to effect the reduction of a part of genetics to molecular biology (or biochemistry). In this case the key to understanding has been reduction; in others it is integration. Thus bioevolution is not understandable on the molecular level alone, but calls for a merger of genetics (both molecular and populational), anatomy, physiology, ecology, biogeography, palaeontology and systematics—actually all the branches of biology. In this section we shall study reduction, a much misunderstood operation; we shall leave integration, a generally ignored operation, for the next section.

We can reduce concepts, propositions, and theories. In some cases reduction results in a deeper understanding, in all cases it unifies fields of inquiry. Thus the identification of light with electromagnetic radiation gave us a deeper understanding of light and brought about the unification of two previously separate fields of inquiry. The identification of mental states with brain states of a certain kind is producing not only understanding but also the merger of psychology with neurophysiology, and of psychiatry with neurology. And the identification of historical processes with social changes elucidates history and invites the rapprochement of all the social sciences. Far from impoverishing, such unifications enrich both the reducing and the reduced field of inquiry. However, as we shall see in the next section, integration is no less valuable a device for attaining the unity of knowledge.

Let us start with concept reduction. Table 10.1 exhibits a handful of famous cases of reduction in modern factual science. Every one of them can be understood as a case of concept reduction, which in turn is reducible to a special case of definition. Indeed we stipulate that, if concepts A and B belong to contexts (e.g. theories) $\mathscr{C}_A$ and $\mathscr{C}_B$ respectively, then A is *reducible to* B *in* $\mathscr{C}_B$ if and only if A is definable in terms of B and other concepts of $\mathscr{C}_B$. We call such definition a *reductive definition*, or simply

TABLE 10.1
Some illustrious cases of concept reduction

Reduced Concept	*Reducing Concept*
Heat	Random atomic or molecular motion
Light	Electromagnetic radiation
Chemical reaction	Atomic or molecular combination, dissociation, or substitution
Gene	Segment of DNA molecule
Metabolism	Sequence of biochemical reactions
Goal directed behavior	Negative feedback controlled process
Mental state	State of a plastic neuronal system
History	Process of social change

(concept) *reduction.* Thus the first line of Table 10.1 reads: "The concept of heat (in thermodynamics) is reduced to the concept of random atomic or molecular motion (in statistical mechanics)". Or, to put it in the linguistic mode: "The expressions 'heat' and 'random atomic or molecular motion' have the same denotation in the language of statistical mechanics." Every reductive definition identifies concepts (not things or attributes) that had been treated separately before. This explains its being commonly called a 'bridge formula'.

Our regarding bridge formulas as definitions, hence as identities (Vol. 1, Ch. 10), does not entail that they are arbitrary or conventional. They are definitions *in the new context* (the reducing one). But when first proposed they are *hypotheses* and, as such, they have to win acceptance on theoretical or empirical grounds. (Many an attempt at reduction has failed: we honor only success.) But once the more powerful body of knowledge has passed the tests, the bridge formulas, which were initially hypotheses, are incorporated as intratheoretical definitions. Thus, regardless of the way "light ray" was defined before, one defines it now as the normal to the wave fronts of an electromagnetic radiation field.

The above discussion sheds light on the current controversy over the status of the bridge formulas. Some philosophers admit that they are identities but call them 'contingent identities' (Kripke, 1971); others regard them as identities of things or properties (Causey, 1977); finally others regard them as hypotheses and even laws (Nagel, 1961). The former reason that the reductive definitions, unlike the necessary identities of logic, may not hold in all possible worlds. It must be supposed they have privileged information about such imaginary worlds; in particular they must know that logic holds in all of them, whereas in some of them heat is not random atomic or molecular motion. Having no access to such wondrous information I shall regard reductive definitions as strict identities of concepts. They cannot assert the identity of things or of properties because, if two factual items are the same, then they are one.

As for the view that bridge formulas are laws on a par with laws of motion and others, it may have been suggested by the way they are originally introduced—namely as hypotheses—not by the role they perform in mature theories. Surely it is not a law of nature that the internal pressure of a gas is identical with the average molecular momentum transfer; nor is it a law of nature that combustion is fast oxydation. These are concept identifications: they have no ontic counterpart and have therefore a purely epistemological status. (An omniscient being would dispense with reductive definitions.) To put it into the linguistic mode: a reductive definition effects the translation of an expression of one language into another.

The reduction of propositions follows from the reduction of concepts. Thus "We inherit genes" is reducible to "We inherit segments of DNA molecules". Truth values are preserved under such transformations. Not so meanings, for meaning is contextual (Vol. 2, Ch. 7). Thus the sense of "heat" in classical thermodynamics is the set of all its logical relatives, which is not the same as the set of the logical relatives of "random atomic or molecular motion" in statistical mechanics.

The reducibility of theories (hypothetico-deductive systems) is more complicated. To begin with we distinguish reduction from restriction. Thus the restriction of classical mechanics to point particles is particle mechanics. The latter is a subtheory of the former, obtained by setting the mass density equal to a point-like distribution (a delta), and by discarding the stress tensor and the boundary conditions. (Cf. Bunge, 1967c.) The general definition is this: Let $\mathcal{T}_1$ be a theory with reference class or domain R. Then $\mathcal{T}_2$ is the *restriction* of $\mathcal{T}_1$ to S, where S is a proper subset of R, iff $\mathcal{T}_2$ is a subtheory of $\mathcal{T}_1$ and refers only to the members of S. (For the concept of subtheory see Ch. 9, Section 1.2.) Unlike reduction, restriction does not involve reductive definitions. (Restriction can be characterized in a formal way. Let Q_1 and Q_2 be the sets of basic or undefined predicates of $\mathcal{T}_1$ and $\mathcal{T}_2$ respectively. Then (*a*) Q_2 is a proper subset of Q_1—i.e. some predicates in $\mathcal{T}_1$ have no counterpart in $\mathcal{T}_2$; (*b*) every function in Q_2 is the restriction of the corresponding function in Q_1 to the reference class S of

$\mathscr{T}_2$, or its value in some limit. Thus by setting $c = \infty$ in a relativistic theory, or $h = 0$ in a quantum theory, the corresponding classical theory results.)

Unlike theory restriction, theory reduction involves reductive definitions acting as bridges between the new or reducing theory, on the one hand, and the old or reduced theory on the other. In the most interesting cases, usually ignored in the philosophical literature, reduction involves also additional assumptions not contained in the reducing theory. For example, acoustics is reducible to the theory of elasticity with the help of the reductive definition of "sound" as "wave propagating in an elastic medium". On the other hand the reduction of ray acoustics to general (or wave) acoustics requires the assumption that the wavelengths be small enough so that diffraction is negligible. (The relation between wave and ray optics is parallel.) Likewise the law of ideal gases ($pV = kT$) is deducible from the theory of particle mechanics enriched not only with reductive definitions of "p" and "T" but also with the assumption of molecular chaos. (Incidentally, such reduction has a bonus: it interprets in atomistic terms the phenomenological constant occurring in the phenomenological law.) We shall speak therefore of two kinds of reduction: *strong* or full, and *weak* or partial. Whereas strong theory reduction involves only enriching the reducing theory with reductive definitions, weak theory reduction requires, in addition, subsidiary hypotheses not contained in the reducing theory.

(We summarize the preceding discussion in the following definition. Let $\mathscr{T}_1$ and $\mathscr{T}_2$ be two theories with overlapping reference classes, D a nonempty set of reductive definitions, and A a nonempty set of hypotheses not included in either $\mathscr{T}_1$ or $\mathscr{T}_2$ but couched in the language of $\mathscr{T}_1$. Then (*a*) $\mathscr{T}_2$ is *fully* (or *strongly*) *reducible* to $\mathscr{T}_1$ iff the union of $\mathscr{T}_1$ and D entails $\mathscr{T}_2$; and (*b*) $\mathscr{T}_2$ is *partially* (or *weakly*) reducible to $\mathscr{T}_1$ iff $\mathscr{T}_2$ follows logically from the union of $\mathscr{T}_1$, D, and A. Moreover we shall say that $\mathscr{T}_1$ *helps explain* $\mathscr{T}_2$ iff $\mathscr{T}_1$ is mechanismic and $\mathscr{T}_2$ is phenomenological, and $\mathscr{T}_2$ is fully or partially reducible to $\mathscr{T}_1$.)

Theory restriction is straightforward. (However, it requires more than mathematical competence, namely the knowledge and intuition necessary to choose intelligently what to restrict.) Theory reduction is far more complicated: it requires familiarity with the two theories concerned, as well as an ability to grasp analogies between facts of different appearance. And partial reduction requires, in addition, the imagination that goes into the invention of the suitable additional hypotheses. Think of the ingenuity that went into the construction of modern solid state theory, which is based on quantum mechanics but is neither a subtheory nor a restriction of the latter. More on this below.

The most interesting philosophical feature of reduction is that it explains the emergence of novelty, i.e. the formation of things possessing properties not shared by their components. Scientists are often suspicious of the concept of emergence for regarding it as obscure. However, it can be defined in exact terms, namely as follows. Let s be a system with A-composition, i.e. the components of which (at a given level of analysis) are of type A, where A is a natural kind such as the collection of all atoms, or cells, or societies. If P is a property of s, then P is *emergent* with respect to A if, and only if, no component of type A possesses P; otherwise P is a *resultant* property with respect to A. *Example*: Neural connectivity is not a property of individual neurons but of neuronal systems. And mental activity is not a process in an individual neuron but one in highly complex neuronal systems with variable connectivity. Hence a neurophysiological explanation of perception, memory, or intelligence, will require not only knowledge of individual neurons but also data and hypotheses concerning the interactions among neurons to produce neuronal systems (psychons) endowed with the emergent properties which we call 'mental'. In short, sometimes emergence can be explained by reduction, i.e. in terms of lower level entities and their interactions.

Emergence is not exclusive of the higher levels of organization of matter but occurs on all levels. Consider three things, A, B, and C, of different kinds and capable of assembling into three component systems. In principle six different linear systems are possible, among them ABC and BCA. The structure (order, configuration, pattern of interactions) of the system will certainly be determined by the interactions among its components. But the structure of a system is an emergent property of it, one that neither of its components possesses, and therefore one that could not have been predicted from a knowledge of the components alone.

Consider two specific examples, and in the first place the formation of one of the simplest systems we know of, namely a hydrogen atom. It is composed of a proton and an electron interacting via an electric field. To a first approximation we may disregard its environment, i.e. we may assume that it is placed in a perfect dark vacuum. The application of quantum mechanics to the study of this system explains the emergent as well as the resultant properties of this system. (The explanatory schema is that of a partial reduction: quantum mechanics, enriched with a set of subsidiary assumptions concerning the composition and structure of the system, entails the explanandum. The resulting conceptual system is a bound theoretical model in the sense of Ch. 9, Section 1.2.) The resultant properties of the system are the total mass, charge, angular momentum, and spin. The emergent properties are the discrete energy spectrum (a denumerable infinity of energy states), the probabilities of radiative decay and excitation, and the corresponding probabilities of transition among the various energy levels. None of these properties is possessed by either of the components of the system.

One of the most spectacular successes of the reductionist program is contemporary solid state physics. Consider a length of copper wire. Every one of the atoms that composes it can be accounted for, in principle, by the quantum theory of atoms. However, the body composed of these atoms has bulk properties, such as electrical and thermal conductivity, malleability, and brilliance, which are emergent with respect to the component atoms. These properties are not represented in the quantum theory of atoms, hence they cannot be explained by the latter without further ado. However, solid state physics explains those emergent properties on the basis of quantum mechanics, namely adding to it certain hypotheses concerning the copper crystal lattice, the electrons wandering through it, and the interactions among the copper ions and the electrons moving about in the lattice. Clearly, then, this reduction is of the partial or weak kind. So is the reduction of genetics to molecular biology.

From an ontological viewpoint there are two kinds of reduction: micro and macro. *Microreduction*, or bottom-up analysis, takes the elementary components for granted and attempts to explain the whole in terms of the former and their mutual actions. On the other hand *macroreduction*, or top-down analysis, takes the whole for granted and tries to explain the behavior of its parts in terms of the former. Both accounts have been known from Antiquity: microreduction to the Greek and Indian atomists, macroreduction to Aristotle. Of the two reductionist programs microreductionism has proved by far the more fertile.

The reasons for the microreduction (or atomistic) program are three. One is the ontological hypothesis that all macrosystems are composed of microphysical things such as atoms. Another reason is epistemological, namely that, if one knows the way the components of a system behave, one should be able to infer how the system as a whole behaves. The third reason is methodological: the laws of the microcomponents are supposed to be simpler, hence more readily known, than the laws of the systems they compose. The first reason is legitimate, but we have known this only since the beginning of our century. The second reason holds only in part, for in order to account for the emergent properties of a whole we need to know not only its components but also the mode of their composition (i.e. the system structure) as well as its environment. And the third reason turned out to be false: in fact most known macrolaws are simpler than the microlaws. (Think of the quantum theories *vis-à-vis* their classical counterparts.) The reasons for this greater simplicity of macrolaws are the following: (*a*) a system behaves as a unit in certain respects; (*b*) some of the random fluctuations in the components either cancel out or are unimportant at the macrolevel, and (*c*) the system can be freer than its components, every one of which interacts strongly with other components—for otherwise there would be no system.

The first of the above-mentioned reasons suffices to explain the sensational success of the microreduction program. However, in many cases microreduction is insufficient and must be supplemented with the macroreduction perspective. Thus to explain the peculiar properties of the surface of a liquid we need to know that it is the boundary of a system; to explain the activities of the motor cortex we need to know that it controls the limbs; to explain a machine we need not only a knowledge of its parts but also of its global functions. In all these cases a synthesis of micro and macroreduction is called for.

Theory reduction is one way of explaining emergence. (It is not the only way: in some cases one succeeds in building from scratch a theory explaining the formation of systems of some kind.) However, philosophers concerned with preserving the marvellous variety of reality fear that reduction may eliminate emergence. This fear betrays uncertainty about emergence itself and misunderstanding about explanation. The explanation of a fact does not eliminate it. (What we do eliminate sometimes is nonfacts, i.e. false conjectures about nonexistent facts, as when we explain the fact that two people happened to think of the same object at the same time as a coincidence instead of as evidence for telepathy.) Far from denying the existence of different levels of organization, reduction relates them: i.e. it helps understand the level structure of reality instead of leveling it down or up (Bunge, 1977b).

In any event, the reduction operation has a number of aspects in addition to the logical one, which is almost exclusively that studied in the philosophical literature. See Table 10.2.

How far can or should reduction be pushed? Three different answers to this question have been proposed: antireductionism, radical reductionism,

TABLE 10.2

The various aspects of theory reduction. $\mathcal{T}_1$ = reducing theory, $\mathcal{T}_2$ = reduced theory, D = set of reductive definitions, A = set of additional promises (data or hypotheses)

Aspect	*Full or strong reduction*	*Partial or weak reduction*
Logical	$\mathcal{T}_2$ follows from $\mathcal{T}_1$ plus D.	$\mathcal{T}_2$ follows from $\mathcal{T}_1$ plus D and A.
Semantical	Every predicate in $\mathcal{T}_2$ is in $\mathcal{T}_1$, possibly via D. (So all the peculiar predicates of $\mathcal{T}_2$ are eliminated.)	Some predicates in $\mathcal{T}_2$ are not in $\mathcal{T}_1$ but are built in terms of predicates in $\mathcal{T}_1$, D, and A.
Ontological	The reference class of $\mathcal{T}_2$ is included in that of $\mathcal{T}_1$.	Some referents of $\mathcal{T}_2$ are emergent with respect to those of $\mathcal{T}_1$.
Epistemological	$\mathcal{T}_1$ helps explain the facts accounted for by $\mathcal{T}_2$.	$\mathcal{T}_1$ helps explain the facts accounted for by $\mathcal{T}_2$, which in turn explains emergents w.r.t. $\mathcal{T}_1$.
Methodological	$\mathcal{T}_2$ is a motivation and touchstone for $\mathcal{T}_1$. $\mathcal{T}_1$ may force some changes in $\mathcal{T}_2$.	$\mathcal{T}_1$ is a tool for building $\mathcal{T}_2$. Confirmation of $\mathcal{T}_2$ reinforces $\mathcal{T}_1$, but refutation of $\mathcal{T}_2$ may be blamed on A or D.
Pragmatical	Some computations are simpler in $\mathcal{T}_2$ than in $\mathcal{T}_1$.	Some computations feasible in $\mathcal{T}_2$ are impossible in $\mathcal{T}_1$ (for want of A's).
Historical	Usually $\mathcal{T}_2$ precedes $\mathcal{T}_1$ in time and helps set up the semantic assumptions of $\mathcal{T}_1$.	Usually $\mathcal{T}_1$ precedes $\mathcal{T}_2$ in time and serves as a foil for building $\mathcal{T}_2$.

and moderate reductionism. *Antireductionism* rejects any attempt to understand facts on one level of organization in terms of hypotheses and data concerning some other levels, on the ground that "the whole is more than the sum of its parts", a popular slogan presumably meaning that wholes possess properties not shared by all its components. The ontological counterpart of antireductionism is *holism*, or the view according to which reality is composed of wholes whose properties are independent of those of their components, which are subordinated to the totalities. The antireductionist strategy is to concentrate on wholes in the hope of understanding the nature of their components. (*Examples*: studying whole organisms and whole institutions independently of the study of their components.) Antireductionism, still found in the backwaters of science and the humanities, is refuted every time a chemical process is explained with the help of physical theories, every time a biological function is explained with the help of physics and chemistry, and every time a social process is explained in terms of the needs, beliefs and aspirations of individuals.

Radical reductionism claims that all concepts, hypotheses and theories concerning things on a given level are reducible to those referring to things belonging to some other levels. The ontological companion of radical reductionism is *physicalism* (vulgar materialism), according to which every thing is a physical entity, and the differences we draw between the physical, the chemical, the biological, and the social, are merely differences in complexity. The radical reductionist strategy is to concentrate on the components of a system in the hope of understanding the latter. (*Examples*: studying cells before whole organisms, and individuals before social systems.) Although in the vast majority of cases reduction is partial rather than full, radical reductionism is extremely popular among natural scientists.

Radical reduction has failed even in physics. In fact physicists currently admit that there are at least three fundamental distinct theories, none of which is reducible to the other: quantum mechanics, quantum electrodynamics, and the relativistic theory of gravitation. (The status of chromodynamics is still uncertain.) These theories are mutually irreducible because they refer to things of radically different kinds: particles, electromagnetic fields, and gravitational fields. (They can of course be mutually related but not reduced to one another.) Classical mechanics has not yet been successfully reduced to quantum mechanics: it is not known which are the subsidiary hypotheses to be adjoined to quantum mechanics in order to obtain the full theory of the motion of extended bodies, i.e.

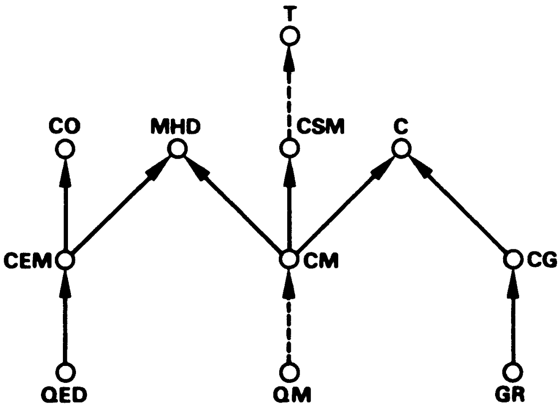


Fig. 10.2. Three basic physical theories of the day: quantum electrodynamics (QED), quantum mechanics (QM), and general relativity or the theory of gravitation (GR). Arrows designate reduction, still incomplete in the case of dashed lines. Classical optics (CO) reduces to classical electromagnetism (CEM), which in turn reduces to quantum electrodynamics (QED). As for QM, one hopes it will be shown to entail classical mechanics (CM), which in turn yields classical statistical mechanics (CSM), which, suitably enriched, should eventually yield thermodynamics (T). Relativistic theory of gravitation (GR) entails the classical theory of gravitation (CG). The union of CEM and CM entails magnetohydrodynamics (MHD), and the union of CM and CG entails celestial mechanics (C). All these reductions are partial. From Bunge (1977b).

continuum mechanics. On the other hand, for strong electromagnetic fields quantum electrodynamics goes over into classical electrodynamics. Likewise, for weak gravitational fields the relativistic theory of gravitation reduces to the classical theory of gravitation. See Figure 10.2. Most of the other reductions cited in the scientific and philosophical literature are instances of wishful thinking. (See Bunge (1973a) for details on reduction relations in physics.) The positivist dream of building a unified science by successive microreductions (Neurath, 1938; Oppenheim and Putnam, 1958; Causey, 1977) has turned to be just that: a dream not even fulfilled in physics. But at least it has been an inspiring dream unlike the antireductionist nightmare. Besides, the unity of science and, indeed, of all genuine knowledge, is fact not fancy: see Ch. 14, Section 3.

Finally, *moderate reductionism* is the strategy of reducing whatever can be reduced (fully or partially) without ignoring variety and emergence—nay, aiming at accounting for them. The ontological counterpart of moderate reductionism is *emergentism* (see Bunge, 1981a), according to which wholes have properties not shared by their components but which, far from being self-existent, result from the latter. The moderate reductionist strategy is to study the whole as well as its parts, in the hope

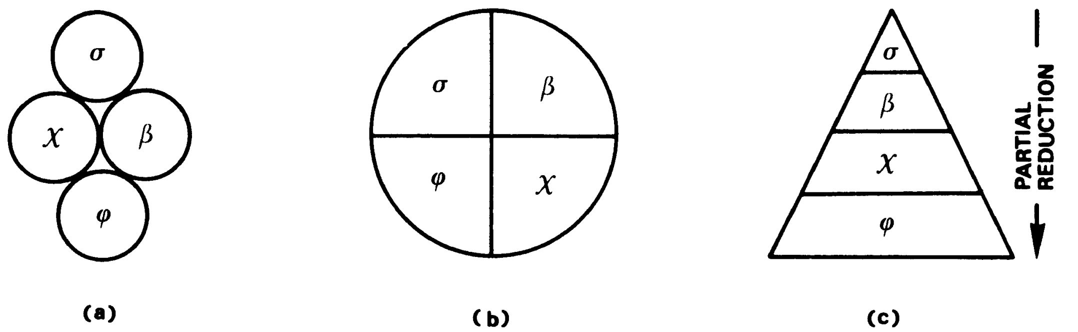


Fig. 10.3. The world and its knowledge. φ = Physics, χ = Chemistry, β = Biology, σ = Social Science. (*a*) Antireductionism: unrelated compartments. (*b*) Radical reductionism: different sectors of a single whole. (*c*) Moderate reductionism: each level higher than the physical one is rooted to the previous ones.

that each kind of research will supplement and spur the other. (*Examples*: studying both individual neurons and neuronal systems, persons and social systems.) Properly analyzed, the most important cases of reduction have been instances of partial reduction. Such successes have refuted both antireductionism and radical reductionism.

Contrary to antireductionism, the various fields of research are becoming more and more integrated, not only by the use of a common methodology underlying the diversity in techniques, but also by relations of (partial) reduction as well as by mergers. And, contrary to radical reductionism, an increasing number of research fields are revealing that the world, though one, has a marvellous variety (accounted for by an increasing number of theories) and a level structure (somehow mirrored in the hierarchy of the sciences). See Figure 10.3. In sum, moderate reductionism is a progressive and realistic strategy, whereas antireductionism is regressive, and radical reductionism utopian.

3.2. *Integration*

Things cannot always be explained by microreduction: quite often they can be explained only by placing them in a wider context. See Figure 10.4. In turn the consideration of such a wider context requires bringing together or consolidating a number of scraps of information and conjecture dispersed among several fields of inquiry. Occasionally such an operation of integration or synthesis brings about the merger of theories and even entire disciplines.

For example, one does not explain the meaning of a word by analyzing it

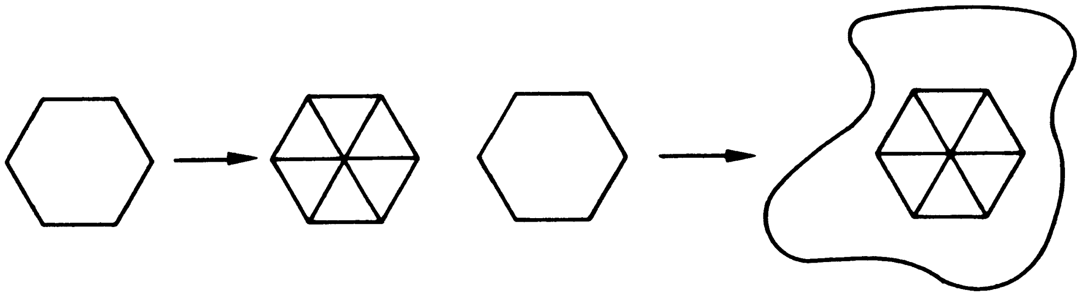


Fig. 10.4. (*a*) Analysis through reduction. (*b*) Analysis through integration.

into its component letters, or even phonemes, but by relating it to other words. The life history of an individual organism is explained not only in terms of genetics and physiology but also in ecological terms. In particular, the behavior of a psychologist could not be explained by only looking into his brain or by watching some indicators of his brain activity: one must also study the systems of which he is a component, such as his family, his laboratory, his scientific community, and even his entire culture. A last example: classical microeconomics, a typically reductionistic enterprise, has failed to explain the behavior of households and enterprises for disregarding their wider social context. One cannot account for the actions of a thing apart from the state of the system it is embedded in. Thus one could not possibly understand why the manager of a firm decides to manufacture product X unless there is a market for X (or one can be created by advertising). And this depends not only on individual needs and wants but also on macroeconomic circumstances and, indeed, on a number of political and cultural variables as well. In sum, what we need is the integration of microeconomics and macroeconomics, not the reduction of one to the other. (More on this in Vol. 6.)

The integration of approaches, data, hypotheses, theories, and even entire fields of research is needed not only to account for things that interact strongly with their environment. Epistemic integration is needed everywhere because there are no perfectly isolated things, because every property is related to other properties, and because every thing is a system or a component of some system (Vol. 4). Thus, just as the variety of reality requires a multitude of disciplines, so the integration of the latter is necessitated by the unity of reality. See Figure 10.3.

Because the unification of knowledge via integration has been sadly neglected by philosophers, particularly those of reductionist leanings, it will be convenient to draw a somewhat long list of successful integrations in science. (*a*) Until recently physicists tried to build one theory per particle species. The large number of particle species, as well as the many relations among them, has necessitated an integrative approach based on the conjecture that each member of a particle species is a state of an underlying particle, much as the electron and the positron are two states of a single particle. (*b*) The relativistic theory of gravitation is not self-contained: in order to solve its equations one must borrow the so-called constitutive relations (determining the so-called matter tensor) from other fields of inquiry. (*c*) The solid state physicist does not start from scratch but makes use of a number of theories, from quantum mechanics to statistical mechanics. (*d*) The theoretical chemist can rarely afford to make *ab initio* calculations: usually he uses both classical chemistry (in particular chemical kinetics) and quantum mechanics, and sometimes even classical particle mechanics. (*e*) Until recently the formation of mountains, earthquakes and vulcanism were treated as separate processes and even by different disciplines. Ever since the plate tectonics revolution, geology treats them as different aspects of the same process. (*f*) Contemporary theoretical meteorology is the outcome of the synthesis of two formerly disjoint theories, namely fluid dynamics and thermodynamics. (The former, which dealt with ideal viscosity-free fluids, had to be rendered more realistic before it could be applied to the atmosphere and the ocean.) (*g*) The stellar atmospheres (in particular the solar corona) could not be studied properly until fluid dynamics and magnetism were fused into magnetohydrodynamics. (*h*) Physiological psychology (or neuropsychology) is a merger of neurophysiology and psychology. Its aim "is not to replace one science by another, but to close the gap between the two sciences—to proclaim their unity, in principle" (Bindra, 1976, p. 19). This unified approach should overcome the one-sidedness of the earlier, reductionist schools, which had attempted to explain behavior, affect, perception, and cognition, in terms of either external stimulation (behaviorism), affect (psychoanalysis), cognition (cognitivism), or some other alleged first mover.

We are now ready to analyze the merger or amalgamation of two or more theories (hypothetico-deductive systems). During the discussion it will be convenient to bear in mind such paragons of theory merger as analytic geometry (the synthesis of synthetic geometry and algebra), celestial mechanics (the union of mechanics and the theory of gravitation), electromagnetic theory (the merger of the theories of electricity and magnetism), thermomechanics (the synthesis of continuum mechanics and thermodynamics), the synthetic theory of evolution (the merger of Darwin's classical theory of evolution and genetics), and evolutionary ecology (the currently attempted merger of population genetics and population ecology).

The simplest case of theory merger is the union of two or more theories (or rather their sets of formulas). Obviously, not every such union is "meaningful". The precursor theories must share referents and therefore also some specific concepts (variables, functions). This is the case with mechanics and the theory of gravitation, the precursors of celestial mechanics. Here the force that occurs in Newton's laws of motion is calculated in the theory of gravitation. (The latter theory did not exist in Newton's time: it was a 19th century creation.) Likewise, the synthetic theory of evolution is the union of Darwin's theory with genetics. However, there are not many more examples of theory union.

In the vast majority of cases the precursor or founding theories have to be supplemented with formulas connecting concepts of the two theories and thus acting as a glue between them. For example, an algebra with a $+$ operation and another with a $\times$ operation can be amalgamated only if both operations are defined on the same set (i.e. if the referents are the same) and if glue formulas such as "$(x + y) \times z = x \times z + y \times z$" are added. To take an even more telling example, and the pioneer in theory merger: Descartes built analytic geometry by assigning every point in Euclidean n-space a unique n-tuple of real numbers and conversely (i.e. by postulating a bijection between E^n and $\mathbb{R}^n$), thus mapping every geometric figure onto a set of n-tuples of real numbers. The theories of electricity and magnetism could not have been synthesized into the electromagnetic theory without further ado: Faraday's law of induction and Maxwell's conjecture of displacement currents had to be added.

In general we shall say that $\mathcal{T}$ is a *merger* of $\mathcal{T}_1$ and $\mathcal{T}_2$ if, and only if,

(i) $\mathcal{T}_1$ and $\mathcal{T}_2$ share some referents and some concepts (e.g. functions);
(ii) there is a (possibly empty) set G of (glue) formulas relating some concepts of $\mathcal{T}_1$ to concepts of $\mathcal{T}_2$, and
(iii) the (glue) formulas in G are sufficiently confirmed.

Condition (i) excludes theories that have nothing to do with one another. (*Caution*: the sharing of spatial or time coordinates does not count, for they are universal features.) Condition (ii) makes room for theory union as the particular case when G is empty. And condition (iii) is added because, in principle, there are infinitely many possible glue formulas. Only those matching the available data will hold the original theories effectively together. By the way, note the methodological difference between glue formulas and the reductive (or bridge) identities of the previous sections: whereas the latter are definitions, the former are postulates.

To sum up, integration is at least as frequent as reduction and just as important a factor of the unity of knowledge. See Figure 10.4. Integration and reduction accompany specialization and make up for its narrowness and centrifugal tendency. Integration is particularly conspicuous in the study of macrosystems, natural or social, because of their multiple aspects and the several levels of organization they cross. Despite their enormous variety, science and technology are one in multiple ways: (*a*) all the chapters of science and technology share a general outlook or philosophical background; (*b*) all the sciences and technologies share logic and mathematics; (*c*) all the sciences and technologies share the scientific method; (*d*) every science and every technology deals with one part or aspect of reality (the only world there is); (*e*) every science and every technology is closely related to at least one other science or technology, so that the totality of sciences and technologies forms a system; (*f*) all of the sciences share the goal of getting to know laws, systematizing them into theories, and using them for cognitive or practical purposes; (*g*) all of the technologies share the goal of using knowledge in the design of artifacts. Therefore there is no need to try and reduce all sciences to physics (as physicalists have demanded) or to psychology (as suggested by phenomenalists), or to merge all theories into a single all-encompassing theory. Science and technology are already one in spite of the inevitable failure of such quixotic attempts. What has to be done is to tighten the system, by reducing the reducible and integrating the integrable.

4. FORECASTING

4.1 *From Expectancy to Forecast*

All higher vertebrates seem capable of expectancy, i.e. of adopting the attitude of anticipating impending events. The animal that expects something to happen may be curious, elated, or frightened, but not indifferent to the future: he has a definite "set" (*Einstellung*). He may expect something to happen or he may have a definite expectation on the strength of a learned association. If he has learned to expect event *B* to follow event *A*, either regularly or with some frequency, a characteristic wave (the *E* wave) will appear in his forebrain (Grey Walter *et al.*, 1964).

The predictive abilities of some animals are quite remarkable. If I throw a stick for my retriever and continue to walk, the animal will meet me further on along the pathway without retracing his steps: somehow he has "computed" my path and adjusted his own to it. When we perceive a regular wave motion we detect its periodicity and anticipate the motion. Similarly we can often complete sentences that other people start and fragments of melodies we have caught. We do all this without using any explicit predicting techniques. Expectancy, and a modicum of forecasting ability, seem to be in the survival kit of all higher vertebrates. *Praemonitus, praemunitus* (Forewarned, forearmed).

Presumably, the more strongly an event is correlated with a previous event, the easier it is to learn that the latter is a predictor of the former. However, such objective correlation is not sufficient for learning about it. Contrary to what one might expect intuitively, experiment shows that animals learn to associate an event *A* to a latter and correlated event *B* only if they are *unable* to predict *B* from *A*: the more surprising an event the better we learn to associate it with its predictor (Rescorla and Wagner, 1972). The reason could be that the more surprising events are more intensively "processed" in the brain.

Although the neurophysiology and psychology of expectancy and forecast have made a good start, we still have to conjecture the way we learn predictive relations. An attractive conjecture is the following (Bindra, 1976). Suppose an animal learns that event *A* is often followed by event *B*, and so learns to predict *B* from *A*. What happens in its brain is that the events as well as their relation are represented by activities of neuronal systems which Bindra calls 'contingency organizations'. If the contingency organization representing event *A* is activated in the animal that has learned the *A–B* connection, then it stimulates or primes the perceptual representation of *B* even before *B* happens. That is, the psychon of the *A–B* connection causes an advance excitation of the psychon of the predicted event *B*, or at least predisposes the latter to activation (by lowering its excitation threshold). The actual perception of *B* is therefore facilitated by the predictor event. In other words, the animal that knows that *A* is followed by *B* expects *B* upon perceiving *A* (or imagines *B* upon imagining *A*) and is thus better prepared to perceive *B* and act in consequence. See Figure 10.5.

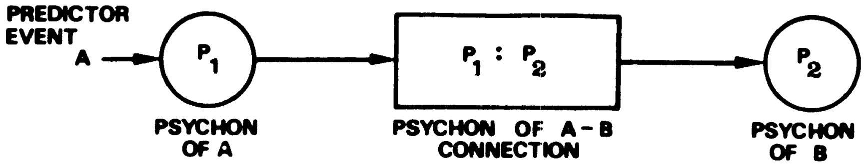


Fig. 10.5. The neurophysiology of prediction. Predictor even A activates psychon p_1, which acts on contingency organization $p_1 : p_2$, which in turn controls the excitation level of p_2, the psychon of the predicted event B. From Bindra (1976, p. 132).

The fact that we have definite neural mechanisms for prediction does not mean that all our forecasts are correct. Many things may go wrong with our predictive mechanisms. We may have learned the incorrect pairings, e.g. to expect a catastrophe every time our path crosses a black cat. Or we may have learned the correct pairings but the predictive mechanism was inhibited, or was interfered with by some other psychon. Or, and this is the most common case, we may have failed to learn any predictive pairings at all—and yet we may be foolish or hard pressed enough to make predictions. This explains the well-known fact that most intuitive forecasts, and even a high fraction of expert forecasts (in medicine, business, and politics) are erroneous.

We hardly needed psychological research to tell us this. What psychologists can do is to try and find out what prediction strategies people use and how they perform. Kahneman and Tversky (1973) studied the problem and found that their subjects, far from using all the evidence at their disposal, selected evidence of one kind only and, moreover, ignored evidence that, if attended to, would have elicited a different judgment. (Specifically, the subjects were asked to predict the area of specialization that a certain individual, whose personality sketch had been drawn on the basis of projective techniques, and which was known to them, was likely to choose. The subjects relied only on this personality sketch, even though knowing that projective techniques are unreliable; and they predicted, e.g. that a given subject would choose computer science, even though knowing that the fraction of the student population that chooses computer science is small.)

From a logical point of view forecasts are special cases of description. They describe events or processes that are not yet or may never be, and therefore are conjectural rather than known with certainty. Indeed a description may refer to past, present, or future events. A description of future events is called a *prediction*, one of past events a *postdiction*. Predictions, postdictions and descriptions of current events are all nonexplanatory accounts. True, some predictions and postdictions are more chancy than some descriptions, but this is not always the case. Thus, a prediction of planetary movements is more accurate than either a postdiction or a prediction of human behavior. In sum, prediction and description do not differ in logic and they need not differ in truth value. The difference between them is methodological: the ultimate check of factual hypotheses and theories consists in checking the predictions or postdictions made with their help. But checking belongs in Ch. 11.

Forecasts can be classed into intuitive and rational. An *intuitive forecast* uses only tacit premises or perhaps none; a *rational forecast* employs only explicit premises: it is the logical consequence of a deductive argument from hypotheses (e.g. law statements) and data. Intuitive forecasts are more or less imaginative inventions, and often just wishful or dreadful thinking. Examples of this kind of forecast are the long term weather forecasts that the *Farmer's Almanac* has been publishing every year since 1792 with the help of a "secret formula". Though widely believed, such forecasts have been shown to be not much better than those obtained by flipping a coin (Walsh and Allen, 1981). Because the premises of an intuitive forecast are hidden or nonexistent, we cannot learn anything when watching its performance. On the other hand the success or failure of a rational forecast is likely to teach us something about the relevance, accuracy, and sufficiency of the premises employed in formulating it.

Intuitive forecasts on a grand scale are called *prophecies*. In the past prophesying was an occupation of shamans, priests, astrologers, soothsayers, and politicians. It has now become an industry that may be called *prophetic futurology* (or *pop forecasting*), to distinguish it from serious futurology, which is concerned with planning on a grand scale, i.e. with redesigning society. Unlike the prophets of the Old Testament, who were interested mainly in domestic events and knew no electronic gadgets, the high priests of modern prophecy deal with world events and extrapolate from modern technology. For instance, they prophesy that before long everyone will be assured of a decent income and will enjoy programmed dreams and individual flying platforms (Herman Kahn). They tell us matter-of-factly that the 21st century will be run by computers that (who?) will dispense justice and keep us out of mischief, will solve all intellectual and practical problems, will keep track of all we do and fill in for us when we fail to do our duty, on top of which they will design and construct even more intelligent and efficient machines. Sadly, none of the modern prophets were able to forecast any of the major events of the recent past, such as the oil crisis, stagflation, the Green Revolution, the Islamic revival, the defeat of the U.S.A. in Vietnam, the Sino–American alliance, the ousting of Somoza, the return of Juan Perón and Indira Gandhi to power, the Falklands war, or the decline in the pace of technological innovation.

Modern prophecies, just like those of Ezequiel, Daniel, and Isaiah, are of the form *B will happen at time C*—e.g. money will have disappeared by the year 2000. That is, they are unconditional statements, unlike the conditionals of scientific prediction, which are of the form "If *A* happens (or continues to be the case), then *B* will (or may) be the case at time *C*". But, unlike the prophets of old, the modern prophets make wild extrapolations from current technological trends and they disregard the social constraints: they take it for granted that whatever the technologist may come up with will instantly be adopted across the frontiers. Moreover, they assume wrongly that they have sufficient knowledge of the present, which is never the case if only because they are usually specialists with a one-sided vision of the world.

Some of the pitfalls of intuitive forecasting can be avoided by pooling and sifting the opinions and speculations of a number of experts in a given area and determining the points of consensus. This is what the Delphi "method" boils down to—although it was initially advertised as "a tested technique of long-range forecasting" (Helmer, 1966). It is now regarded as merely a procedure for gathering "soft" (i.e. subjective and unchecked) data in the social technologies (Linstone and Turoff, 1975). It lacks theoretical justification and it is yet to be seen whether it is successful.

Rational forecasts, like descriptions, can be extratheoretical or theoretical. Most of the rational forecasts made in medicine, education, economics, and politics, are extratheoretical: they employ predictors, not theories. For example, the general practitioner prognosticates on the basis of vital indicators, the teacher and the businessman on the strength of past performance, and the economist and politologist extrapolate present trends. Any sign or indicator used to make forecasts is called a *predictor*. For example, we employ the results of entrance examinations as predictors of scholastic performance (even though we ought to know that they are poor predictors), on the assumption that the student will continue to behave as in the past. This is the way high school students are admitted to college, graduates hired as professors, employees promoted to management, and managers to directors. The hypothesis that anyone who is good at any given level must be good at the next higher level works much better than the superficial impressions one may get from an interview, and better than recommendations. However, it is not quite correct, because the abilities required in each case are different. Oversight of this well known fact leads to adopting the famous Peter Principle: "Everyone gets promoted to his own level of incompetence".

(One way of elucidating the notion of a predictor is as follows. Suppose we know two kinds of events, E_1 and E_2, and know also that they are related. This relation can be either constant (e.g. causal) or probabilistic. I.e., either some event of kind E_2 happens every time an event of type E_1 occurs, or there is a definite probability of an E_2 happening every time an E_1 occurs. In the first case any functional relation between some property of E_1's and some property of E_2's can be used as a predictor of E_2's. In case the relation is stochastic we may take the difference between the conditional probability $Pr(e_2|e_1)$, for e_1 in E_1 and e_2 in E_2, and the absolute probability $Pr(e_1)$, to measure the value of E_1 as a predictor of E_2. More precisely, we may define the *predictive value* of E_1 for E_2 as

$$v(e_1, e_2) = Pr(e_2|e_1) - Pr(e_1) \quad \text{for all } e_1 \in E_1 \text{ and } e_2 \in E_2.$$

This difference is positive only in case e_1 is favourable to the occurrence of e_2; it vanishes if e_1 and e_2 are statistically independent. If the predictive value of E_1 for E_2 is positive, E_1 can be said to be a *predictor* of E_2. From the above we see that a predictor may be a cause, an indicator of a cause, or any event functionally associated with the event of interest.)

All governmental agencies and many private enterprises extrapolate current trends in population, energy consumption, industrial production, use of various items, and so forth. Such extrapolations assume that nothing is done to curb the given tendencies. Such forecasts, which often involve massive use of data and computers, are sometimes called *scenarios*. Scenarios have little cognitive value because they result from mere extrapolation of current trends. But, by the same token, they may have immense practical value in sounding timely alarms and suggesting plans. For example, one of them tells us that, if the world population were to continue growing at its present rate, there would be no standing room left by the end of the next century; another that, if car sales continue at the present rate, every human, including infants, would own a car by the year 2050; still another, that the consumption of hard drugs would exceed that of food.

Because they are absurd, such scenarios suggest that something is wrong with some of our present systems: that they will break down unless we interfere with them. "By so doing one can plan interventions *now* rather than wait, as is usually the case, until the system is in a state of crisis" (Ackoff, 1978, p. 128). For example, all experts in nuclear warfare are agreed on the following: (*a*) in case of a (or rather *the* first and last) nuclear war, all the major towns in the northern hemisphere would be destroyed; (*b*) there is no effective shelter against nuclear blast or even against radioactive fallout; (*c*) as long as there are stockpiles of nuclear warheads it is possible that a nuclear holocaust be started by an irreflexive head of state or even by mistake. Such forecasts should suffice to persuade any rational being that total nuclear disarmament is necessary (though not sufficient) to preserve the human species, at least for a while.

The rate of success of extrapolations of current trends is far superior to that of intuitive forecasts, such as those based on clinical judgement, unstructured interviews, or projective tests. In particular, statistical forecasts, made on the basis of regression equations, work reasonably well at least in the short run, not only in the social sciences, especially in economics, but also in medicine and clinical psychology (Sawyer, 1966; Dawes, 1980). Even the simplest statistical model (linear regression or correlation) does better than the intuitive forecast. Still, statistical predictions (i.e. extrapolations of present tendencies) are far poorer than theoretical predictions, i.e. forecasts with the help of theories. The reason is that, unlike every other kind of forecast, theoretical prediction involves laws (and also rules in the case of social science), not just trends that can change overnight.

Thus a knowledge of statics, and the information that a particular lever is in equilibrium, allow us to predict that, if the weight or the length of one of its arms changes, the lever will become unbalanced. This forecast is qualitative both with regard to the imbalance condition and the time. An instance of a quantitative forecast is this. If the law of growth (or decay) of a certain magnitude (e.g. the number of individuals in a population) is exponential, and one knows or assumes both the initial value of the magnitude and its growth (or decay) rate, then one can forecast with precision the value of that magnitude at any time. One can also make several other predictions, e.g. the precise time at which the given magnitude will have doubled (or halved). To be sure, such a precise forecast may turn out to be false, but this failure will be instructive: it may indicate that some limiting factor (e.g. scarcity of resources, crowding, or competition) is at play. No such lesson can be learned from an intuitive forecast.

The truth of a forecast must be distinguished from its precision, for—as every fortune teller knows—the less definite a forecast the more chances it has of being true. Consider the following forecasts:

(i) Anything may happen.
(ii) Something will turn up to solve the energy crisis.
(iii) Some day man will learn to control society.
(iv) There won't be any fossil fuel left by the year 2100.
(v) The Halley comet will be visible from Earth on April 11, 1986.
(vi) The probability that an atom of species A in state B will emit a photon within time t, is p.

These forecasts are of different degrees of definiteness. The first is the least definite of all; fortunately we may disregard it, for it conflicts with the principle of lawfulness. The second is mere wishful thinking, since very little, aside from rather slow progress on fusion and satellite solar power, is being done to avert the impending catastrophe. The third too is rather indefinite but it is somewhat better grounded than the preceding, because social science is making progress, and planning is gaining world-wide acceptance. Forecast (iv) results from extrapolating current fuel consumption and population trends, so it is both definite and well grounded—though unreliable because those trends may not continue. The last two forecasts are both definite and well grounded. Unlike the preceding ones, they are theoretical predictions. Let us take a closer look at them.

4.2. *Scientific Prediction*

There are only actuals—by definition. But of course possibility is of the very essence of actuality: nothing can be, or cease to be, unless it is really possible to begin with. (For our definition of real possibility in terms of laws see Vol. 3, Ch. 4.) All actuals change and induce change in others. However, as long as such changes do not occur, they are nothing but possibilities. Nothing prevents us from, and everything advises us to, imagining possibles—possible things, states of things, and changes in such states, i.e. events. This is what forecast is all about: imagining possible individuals. Such imagination may be fanciful, as in fiction and prophecy, which are restricted at most by logic; or it may be controlled, as in scientific forecast.

Controlled forecast, as practised in science and technology, is of either of two kinds: *empirical* (though not intuitive) and *theoretical*. The former uses statistical correlations, regression lines, and other statistical tools; on the other hand theoretical forecast involves the knowledge of laws. Evidently, theoretical forecast is in principle superior to empirical forecast, because laws are deeper and firmer than correlations. However, theoretical forecast may score lower than empirical forecast if it relies on incomplete or inaccurate data, as is the case with weather forecasting. Let us catch a glimpse of the latter.

The traditional method of weather forecasting uses empirical generalizations, while the theoretical method employs a sophisticated mathematical model of the atmosphere that combines fluid dynamics with thermodynamics. Use of the former method calls for considerable flair that can be educated. On the other hand, once the theoretical model has been chosen and the data are in, numerical forecasting can be entrusted to a high speed computer (as is done at the U.S. National Meteorological Center and a few other institutions). Unfortunately the meteorological data are not only inaccurate but also incomplete. For one thing they do not include values of the entropy, the energy fluxes, and other functions; for another, they fail to cover vast regions of our planet, such as the oceans and the arctic and equatorial regions. So, even assuming that the theoretical model is correct—a very doubtful assumption, if only because it is a non-probabilistic model—it contains variables that go unmeasured, and others that are measured only in some places and with large errors. As a result, the empirical weather forecasts are still, after four decades of work on theoretical forecasts, more accurate than the theoretical ones.

This *impasse* has led some meteorologists to suggest giving up computer simulations of the atmosphere (Sanders, 1979) or even abandoning the project of theoretical forecast altogether (Ramage, 1976). Yet presumably something can be done about this crisis—e.g. to try more sophisticated models of the atmosphere, and to multiply the number of (automated) meteorological stations. (Incidentally, weathermen are in the habit of speaking of probabilities, e.g. of precipitation. This use of the term 'probability' is incorrect, because neither the empirical regularities nor the theoretical models involved in weather forecasting are probabilistic. When the weatherman learns that in the past p percent of the time the atmosphere looked in such and such a way the next day it rained, he forecasts: "The probability of rain tomorrow is p". Here p is a percentage not a probability. There are no probabilities unless there is randomness: see Bunge, 1981c.)

Empirical (statistical) forecasts are based on unexplained correlations, which are precise measures of the strength of the association between two variables, each of which represents a property of some concrete thing. Thus if we have found that X correlates highly with Y, then every time we find X

we may expect to find Y as well—provided the correlation holds out instead of being spurious as in the famous cases of the correlations between grain crops and sunspots, or between divorces and foxes killed in Finland. For example, for every human population there is a definite correlation between height and weight. This correlation can be used to predict the one from a knowledge of the other in the given population. (But of course it does not apply to a different population or to individuals, who may become leaner or fatter without changing in height.) Once we have obtained a correlation coefficient we can draw a line that allows us to extrapolate into the future—assuming that the correlation will hold out. In particular, we can build autoregressive models centered on autoregressive equations, or finite difference equations for a single variable, e.g. $X_t = a_1 X_{t-1} + a_2 X_{t-2}$, which allows us to forecast the value of X at time t in terms of its values at the earlier times t-1 and t-2. All such models are useful forecasting tools and they abound in the life and social sciences. But it is well understood that they yield soft forecasts far inferior to the ones based on theories.

As we saw in Section 4.1, a theoretical forecast is the conclusion of an argument from two groups of premises: theoretical formulas (in particular law statements) and data. We saw also that the occurrence of the time variable in the premises is sufficient but not necessary to make forecasts. For instance Gibbs' phase "rule" (actually a law statement) states that, for any system in equilibrium, with C independent components, P phases, and F degrees of freedom, $C + 2 = P + F$. Two general predictions follow: (*a*) If either of the three properties of a system in equilibrium changes, the system goes into a nonequilibrium state, and (*b*) if a system is in a nonequilibrium state, it will go over to an equilibrium state if either (or all) of the three properties change in such a way that the phase "rule" is satisfied. These predictions are made on the strength of a law statement that does not contain the time concept, and they require counting but not measuring. An even simpler law statement of the same type is the ecological law that, if there is a relation of mutualism between the members of two species, then if either of them increases in number, the other will increase as well.

Most scientific predictions are forecasts of states, events, or processes of known types. Paragons of accuracy are of course the predictions made with the help of celestial mechanics, quantum mechanics, and quantum electrodynamics. In most cases they forecast not only what will happen (or what cannot possibly happen) but also when the event will happen, or at least the probability that it will happen within the next time unit. (If such probability p is calculated then it can be expected that, on the average, the event concerned will happen pN times out of N occasions.) The standard inference schema of any such forecasts is: *Model of the thing(s) concerned plus data entails prediction*. Since in the cases we have mentioned a general theory is involved, the model is obtained by enriching the former with specific assumptions concerning the composition, structure, and environment of the system of interest. (For the notion of a bound model recall Ch. 9, Section 1.2.)

Some of the most exciting scientific predictions are not of states or events of known kinds but of the very existence of previously unsuspected things. Here is a random sample of the increasing collection of forecasts of this type: Darwin's postdiction of the existence of our hominid ancestors, Maxwell's prediction of electromagnetic waves, Mendeleev's prediction of the existence of the elements scandium, gallium and germanium; Oppenheimer's prediction of neutron stars (pulsars), and the prediction of new, as yet not synthesized molecules, made almost every day by organic chemists. All these predictions and many more were made with the help of scientific systematizations and they were borne out by observation or experiment—nay, they guided the latter. Predictions of the same type, but not yet verified, are those of gravitational waves and of superheavy elements (with nuclei around $Z = 114$).

The most satisfactory theories are those which are at the same time predictive and explanatory. Modern planetary astronomy is of this kind; on the other hand the theory of evolution is far too general to be able to make any quantitative predictions. Ancient planetary astronomy, in particular Ptolemy's, was predictive but not explanatory: it was but a device for computing appearances from appearances. Therefore the ancient skeptics were right in doubting that the sun would "rise" the next day. We can now be certain that it will "rise" because we know the mechanism of the alternation of days and nights, namely the spinning of the Earth. (We may doubt on the other hand whether there will be any life left on earth if the superpowers continue to prepare for the final solution to all our problems.)

How is it possible to predict the existence of unheard-of properties, events, and even things? If every scientific statement were nothing but a datum or an empirical generalization, such predictions would be miraculous. First, because before the actual finding of the predicted object there was nothing to generalize from; second because, on an inductivist philosophy, there is no reason to suppose that properties and things could be necessarily (lawfully) related to one another. On the other hand, on the view that theories are hypothetico-deductive systems, serendipity loses its miraculous quality while keeping its magnificence: if certain assumptions are made, a host of conclusions can be drawn, some of which must be unexpected when only the assumptions are examined. Something similar happened with the predictions made on the basis of classificatory schemata such as the periodic table of the elements and the systematics of elementary particles. In both cases certain pigeonholes appeared empty which arose the curiosity of experimentalists to look for things that could possibly fit into them. These discoveries, then, were not lucky accidents. They only illustrate the power of conceptual systematization (classification and theory) to guide experience and thus enrich it. See Appendix 4 for the notion of predictive power.

According to many thinkers, from Ptolemy and the judges of Galileo to Comte and Lakatos, prediction is the hallmark of science: everything else is unimportant. On the other hand Popper believes explanation to be more valuable than prediction, which he regards as unimportant except for technological purposes. As usual, the truth lies in between: prediction is a mark of science though not its only peculiarity nor characteristic of every bit of it, for none of the extremely general theories can make any prediction unless enriched by subsidiary hypotheses and data. Prediction is valuable in itself because it widens the collection of knowable facts. And it is *a* mark of science because, unless there is some prediction, nothing can be tested, and without testing there is no science. (Pseudoscience, in particular psychoanalysis and parapsychology, makes no precise predictions.)

Can we predict everything? Here again there are three possible answers. Fortune tellers, economic advisors, stock brokers, and some politicians claim that they can predict everything: after all they make a living off prophesying. On the other hand many social scientists and philosophers believe that nothing important can be forecast. As usual, neither the optimists nor the pessimists are right. Predictability is not a property of events but of our cognitive relation with them: we can predict provided we know enough. Moreover, since prediction is performed within a body of knowledge, and every such body is incomplete and inaccurate, not everything can be predicted at a given moment, and if predictable it is predicted with some error. And, granting that we shall never attain perfect knowledge, there will always be unpredictables. But, assuming that future generations will continue to care for scientific research, we can also forecast that knowledge will continue to grow, and thus the number of predictables increase.

Neither the rationalist nor the realist need lose any sleep over unpredictables. What should disturb them is the threat of diminishing research. Still, could we discover any unpredictables, i.e. facts either unknowable or lawless? If unknowable, we need not worry: out of sight, out of mind. If lawless, they would be beyond the reach of science, for lawfulness is a (philosophical) presupposition of scientific research. (This principle is irrefutable: if anyone were to point to a certain fact as lawless, we could ask for some time to investigate it in the hope of discovering its laws.) Two candidates come to mind: time series and the outcomes of research and invention. Let us catch a glimpse of each.

A *time series* is a sequence of data of some kind (e.g. total population, or precipitation) taken at regularly spaced intervals, e.g. years. Whereas some time series are random, others exhibit a pattern. The former are unpredictable unless the underlying mechanism is discovered; the others are predictable within bounds. (The degree of predictability of a time series is measured by the autocorrelation function, whose values are computed from the correlation between observation results separated by two, three, etc., intervals. In a predictable time series the correlation coefficient will, typically, fluctuate at the beginning, settling down to a constant value after a while.) In either case a time series is not a brute fact but a way of displaying a collection of brute facts. Hence it is not an instance of an inherently unpredictable fact.

The case of research and invention is more difficult. We know that problems are in the habit of seeming to pop up unexpectedly, and that hypotheses and ideas for experiments or designs come unanticipated. And we also know that even the most carefully planned research is full of surprises, to the point that the initial problem may be replaced with another, and a new method may be invented to handle it. (It follows that no research plan should be detailed and rigid for, if it were, it would suffocate original research. Only routine research can be planned and forecast in detail.) It would seem, then, that originality is unpredictable.

Still, research leaders do make forecasts concerning the research they are responsible for. They make them on the basis of what is known now, as well as on their knowledge of the available human and material resources. Thus Crick (1966) forecast the synthesis of enzymes and even genes, which were actually performed a few years later; and Watson (1976), in his well-known textbook, lists a number of open problems and notes that many of them will probably be solved within the next decade. So, some forecasts of the outcome of original research *are* made successfully. (And, *pace* Popper, such predictions are not logically impossible, for they do not consist in solving problems but in hazarding that the problems will be solved.) But they are intuitive forecasts, not scientific ones: they extrapolate from current events. (Moreover, they are partly self-fulfilling forecasts, for they incite young researchers to take up certain problems.) A scientific forecast of scientific and technological creations would require an extensive and profound knowledge of the neural springs of creativity and of the social conditions that stimulate and stifle it. And we do not even know whether such knowledge will ever be attained. It would be just as rash to claim that it will be attained, as to state that it is beyond our reach. It is more reasonable and rewarding to work on these problems than trying to predict the outcome of such work.

5. Concluding remarks

We all wish to understand what goes on in and around ourselves. Such understanding calls for answering what may be called *the six W's of science and technology: what* (or *how*), *where, when, whence, whither*, and *why*. In other words, understanding is brought about by description, subsumption, explanation, prediction, and retrodiction.

Neither of these epistemic operations is more important than the others. However, most workers specialize in only some of them. Thus collectors, classifiers and field workers are more interested in description than in explanation or prediction; on the other hand theoretical researchers and technologists want to know not only the facts but also why they are so, in particular what makes things tick. Subsumption, explanation, prediction and postdiction are typically theoretical operations, i.e. activities performed with the help of theories—the better the richer and the better organized.

Answering any of the six *W*'s of science and technology is intrinsically valuable: it yields new knowledge. But it may also be instrumentally valuable for allowing us to modify reality to our advantage. Thus if we know how a system works we may be able to keep it in good repair or even to perfect it. Explanation has therefore not only a cognitive but also a practical value. However, explanation is not enough: certain doctrines, such as fatalism, explain everything but, being predictively barren, cannot be put to the test and so cannot be evaluated on empirical grounds. Only theories that help predict or postdict can be contrasted with facts. However, the matter of checking deserves a separate chapter.

CHAPTER 11

PRODUCING EVIDENCE

If we proceed scientifically in any epistemic field, whether scientific, technological, or humanistic, we will try to check our hypotheses and recheck our data. We will not use authority, let alone ignorance, to justify our hypotheses or data. This maxim sounds obvious but it is not. To begin with, the argument from authority is still going strong in many humanistic fields—a remnant of the time when they were not distinguished from ideology. Even in science we cannot dispense with a modicum of authority if we are to use knowledge reaped by others: every time we borrow or quote a result obtained by other workers we hope our source to be "authoritative" (competent and responsible) and put our trust in it. However, this trust is not blind: it is the reasonable belief that the quoted author has proceeded scientifically, and we are ready to give it up the moment it is shown to be incorrect.

As for the argument from ignorance, it is of the form "Since we do not know exactly what X is, or how X came about, we can safely assume that X is Y". Assume, yes; safely, not, for every assumption must be checked. And yet this maxim is sometimes violated in science. Consider the following arguments from ignorance gleaned from the scientific literature. (*a*) "Since we do not know in many cases what the probabilities of the various alternatives are, we may assume that they are equal". (The correct policy is to assume equiprobability as the simplest though not necessarily the most likely hypothesis, and to proceed to check this hypothesis. In the absence of information all hypotheses are equally likely—or unlikely.) (*b*) "If an exception to a given well-tested law statement occurs, it must be for lack of precision in the observation or measurement." (Right policy: Redesign the observation or measurement to see whether the exceptions were genuine or "artifacts", or at least keep those exceptions in mind instead of dismissing them out of hand.) (*c*) "Since we cannot observe a microthing within an arbitrarily small time interval, we can assume that it does not obey any conservation laws between observations". (Wrong: the basic law statements do not say anything about conditions of observation, let alone non-observation. To suspend the laws of physics just because nobody is looking is shear solipsism—or insanity. Yet this is the usual justification for introducing the so-called "virtual" particles and processes. See Bunge, 1970.) (*d*) "Any apparent exception to biological adaptation must be just apparent: if we knew more we would realize that actually it is useful to the individual or the species concerned". (The right hypothesis is that many features of organisms are nonadaptive—e.g. because they are remnants of formerly adaptive traits. The presupposition that every property of an organism is adaptive clashes with the evidence of extinction.)

A characteristic of mathematics, science, technology, and the modern humanities, is that everything in them is checkable: every datum, theory, method, artifact, or plan of action is supposed to be able to pass some test or other, if possible multiple tests. To be sure testability, though necessary to attribute truth or efficiency, is insufficient to decide that an item of knowledge is scientific or technological. Thus astrology, though refuted long ago, never belonged to science; and alchemy, though shown to be both false and ineffective, was never part of technology. In addition to the possibility of testing we need actual testing and we cherish positive test results no less than negative ones. In particular we delight in learning that a theory has been confirmed or a machine actually does what it was designed to do.

We are interested in tests of two kinds: tests for truth and tests for efficiency. They concern objects of different kinds: the bearers of truth are propositions (or the underlying thoughts), and those of efficiency are actions and artifacts (or the underlying proposals, plans, designs, instructions, or norms). Thus whereas data and hypotheses can be true to some extent, norms and computer programs can only be effective to some extent. Truth guides the search for efficiency, and efficiency poses the problem of its explanation, but they are different categories. So much so that truth is often useless, and efficiency is sometimes based on untruth. Thus many true scientific hypotheses have no practical application, whereas some worthless products are commercially successful because of deceitful advertising.

Testability and truth, which are only accessory in the case of prescientific ideologies and public affairs, are central to science, technology, and the humanities. To be sure, doing research in either of these fields is investigating problems with the aim of solving them. But a problem is considered to be solved only if the proposed solution passes checks for truth (in the case of cognitive problems) or efficiency (in the case of practical problems). Therefore it is not true that the success of the scientific enterprise may be gauged by its ability to solve problems regardless of matters of testability and truth (as Laudan (1977) claims). A thoroughly untestable solution is not scientific or technological, and an untested one can be assigned neither a truth value nor an efficiency value.

1. From self-evidence to evidence

1.1. *Self-evidence*

We are all more or less gullible in some department or other. In some cases we regard our beliefs as self-evident, in others we rely on authority, and in still others we refuse to admit unfavorable evidence. Let us make a quick study of these three cases.

Intuitionist philosophers of all hues agree that there are certain obvious or self-evident truths, i.e. propositions the truth of which is recognized intuitively without any tests, i.e. without evidence, and which serve as a standard relative to which all other propositions are evaluated. Husserl went to the extreme of claiming that some people are endowed with a vision of essences (*Wesensschau*) allowing them to grasp the essence of any object without having to waste their time experimenting or theorizing. But of course the history of mankind is littered with the corpses of supposedly self-evident truths, which are just received opinions and often blatantly false superstitions. What is obvious to the layman may be puzzling to the expert, and conversely. Besides, specialists do not behave homogeneously: what is surprising to one may be self-evident to another. But, if competent and responsible, they will not follow blindly their gut feelings but will test them.

Faith in intuition is not restricted to intuitionist philosophers. In all societies most laymen conform to the judgment, prognoses and decisions of experts, be they plumbers or electricians, physicians or economists, shamans or statesmen. We usually take their word although their judgment is more often intuitive rather than scientific—and often wrong to boot. In particular clinicians are known to propose diagnoses in a hurry and to fail in a high percentage of cases, and economists are prone to make recommendations on the strength of theories which, in the best of cases, have not been tested.

Blind faith, i.e. trust in ideas or procedures that have not passed sufficient tests, is not limited to the professions. Many hypotheses in social science have never been checked or, when tested, have turned out to be false—notwithstanding which they are often kept with ideological zeal. Here is a random sample of such views. (*a*) According to Marxism, the modern state is nothing but the supermanager of capitalism; in particular it always protects the monopolies and takes upon itself to run the low return and losing sectors of the economy. The profitability of the French and Canadian state corporations has conclusively refuted that thesis (Niosi, 1981). (*b*) According to neoclassical microeconomics prices are fixed by the play of offer and demand: the price of a good or service is that for which the offer equals the demand. Actually the big corporations fix prices without regard to demand (Galbraith, 1967; Silberston, 1970). (*c*) According to Durkheim, the rate of crime in any stable society is constant over time, so there is nothing one can do about it. The data, at least for California between 1851 and 1970, are inconsistent with this hypothesis and show that the crime rate fluctuates rather wildly (Berk *et al.*, 1981).

We all tend to protect our most cherished beliefs, particularly when we have invested much time and effort acquiring them—as is the case with theologians, political ideologists, and economists. Thus if someone believes that prayer (or vitamin C) will protect him from the common cold, he will pray (or take vitamin C) regularly; should he catch a cold in spite of this, he will probably double the dose rather than question his belief. Failure to admit negative evidence is a very natural disposition: skepticism is learned only the hard way. Jevons (1877, p. 402) remarked long ago that "It is difficult to find persons who can with perfect fairness register facts for or against their own peculiar view". Contemporary psychologists have confirmed this "law of the disregard of negative information". Thus psycholinguistic experiments have shown that "Most people prefer to accentuate the positive" (Miller, 1967, p. 168). Even if they get to know the worth of their false beliefs and wrong actions, most people make hardly any use of such feedback. (See Festinger *et al.* (1956), Wason and Johnson-Laird, (1972), and Shweder (1980), for some experimental material on the tenacity of belief in the face of contrary evidence.)

The most common attitude in the face of unfavorable evidence is then one of disbelief or indifference, or at most doubt. This attitude prevails not only in ordinary life but also in science: "facts that fit into a preconceived hypothesis attract attention, are singled out, and are remembered; facts that are contrary to it are disregarded, treated as 'exceptions', and forgotten" (Luria, 1975, p. 339). There are several possible and mutually compatible reasons for such conservatism. One is that negative evidence is seen as failure or even as threat or humiliation: discarding it or "rationalizing" it is then regarded—particularly in success-oriented communities—as necessary for the preservation of self-esteem. Another reason is sheer intellectual laziness: it may be too hard to modify one's body of beliefs to accommodate the fresh evidence. A third is social (economic, political, or cultural) conservatism tied to vested interests (economic, political, or cultural)—as is the case with the persistence of moth eaten beliefs such as economic laissez-fairism, political centralism, and religious fundamentalism.

However, conservatism is not all that bad: it all depends on what it attempts to keep. Besides, cognitive conservatism is deeply ingrained in our brain and, to a point, it is methodologically justifiable. Let me explain. First of all, psychologists know that—unlike computers—we do not accept new information the moment we acquire it, but screen it for compatibility with our belief system (Rokeach, 1960). If the new information is consistent with our belief system or irrelevant to it, we tend to accept it without much scrutiny; otherwise we tend to alter it to adapt to our fundamental beliefs—or we just reject it. In short, we evaluate new information as it comes in, attributing far more value to favorable than to unfavorable data.

Such epistemic conservatism is double-edged. On the one hand it blinds us to new unsettling information. On the other it spares us countless mistakes. After all, the data themselves may be incorrect: we did not look or measure carefully, the entire experimental set-up was vitiated by some systematic error (e.g. a leak), or we were hallucinating. So, it is methodologically sound to question and even discard data incompatible with a body of knowledge—provided the latter has been well tested. It is not that we must prevent ugly facts from spoiling our beautiful theories: we must take care not to accept all data at face value, and we cannot assign truth values except in some body of knowledge or other. (Imagine trying to evaluate a report on the spin of gluons without having the slightest clue as to the meaning of "spin" or "gluon".)

Although we are all natural epistemic conservatives, we can learn to check ideas and things, and to modify them or give them up if they fail utterly. Such learning is part of the training of every craftsman, manager, technologist, scientist, and modern humanist. Indeed all the sciences, formal or factual, basic or applied, as well as the crafts and technologies, and the modern humanities, are supposed to abide by the following *testability principle*:

RULE 11.1. Every datum, hypothesis, technique, plan, and artifact must be checked for adequacy (truth or efficiency).

In other words, no matter how a bit of knowledge has been acquired, how a design has been conceived, or how an artifact has been manufactured, they should pass certain tests before being accepted. (Reichenbach (1938) emphasized the difference between the "context of discovery" and the "context of justification". There was nothing wrong with this; his mistake was to believe that it was possible to construct an a priori logic of discovery of generalizations from observations, i.e. an inductive logic.) Whereas we should be permissive with regard to discovery and invention—within the bounds set by the public interest—there is little play when it comes to evaluating them. The checking must be rigorous, for we want truth or efficiency, as the case may be, and neither can be ascertained without tests.

This does not entail that, unlike checking, discovering and inventing are utterly lawless and irrational and therefore beyond the scope of scientific research. The history of science, technology and the humanities teaches us that new findings do not pop up out of the blue: that they answer questions posed before against a certain epistemic background, that they are acquired with the help of means evolved before, and that they lead rather naturally to new developments—provided no social hurdles are in the way. Besides, like all other cognitive processes, discovery and invention can be studied both as brain processes and as social processes. Uncovering their mechanisms and those of their neural and social inhibitions should help us discover and invent, or at least prevent us from interfering destructively with them.

To return to the testability principle (Rule 11.1). To be sure, it would be impossible for us to test every proposition, proposal, and artifact that comes our way: the life and resources of an individual are too short for that. Therefore checking is a social endeavor: we submit our findings to the examination of our peers, and trust most of the knowledge we borrow from fellow workers. But this trust is neither blind nor unshakeable: we trust only the information that has been screened, and we do so only provisionally, i.e. until shown wrong. In other words we adopt the *fallibilist* principle:

RULE 11.2. Regard every cognitive item—be it datum or hypothesis, technique or plan—as subject to revision, every check as recheckable, and every artifact as imperfect.

However, there are degrees of adequacy, hence of inadequacy: some propositions are truer than others, some methods more powerful or accurate than others, some plans more effective than others, some artifacts more suitable than others, and some checks more rigorous than others. Consequently radical skepticism—according to which everything is equally worthless—is as false as it is paralyzing. Only moderate (or methodological) skepticism may help us advance from one inadequate proposition to a less inadequate one, from one ineffective proposal to a less ineffective one, from one unsuitable artifact to a less unsuitable one. Such moderation is controlled by the *meliorist* principle:

AXIOM 11.1. Every cognitive item, every proposal, and every artifact worth being improved on can be perfected.

Of course there is no way of proving this optimistic principle, and this is why, if accepted, it must be adopted as a postulate. All we can affirm with confidence is that so far it has been confirmed—a case of self-fulfilling prophecy. However, the principle has its built-in limitation, namely the proviso that the item to be improved be worth being improved. There are two reasons for this proviso. One is that some items had better been dropped altogether—e.g. torture instruments, cigarette lighters, and leather covers for moustaches. The other is that even serviceable ideas, procedures and artifacts reach a point where any further investments in their improvement would by far outweight the returns on them.

To sum up, self-evidence is illusory, authority can (and must) be challenged, and negative evidence, though we naturally dislike it, lurks behind everything. Hence we must be fallibilists—but, at the same time, meliorists. Our "worlds" of knowledge, plans, and rational action are very far from being the best of all possible worlds, but they are not the worst either and, what is better, they are perfectible.

1.2. *Evidence*

If self-evidence is no evidence, what is? A theoretical consideration can be; also an empirical datum, although not all data qualify as evidence. We shall deal with conceptual evidence in the next chapter, Section 3.1. Suffice it to say here that every well-confirmed theory is evidence for any other theories presupposing the given theory—and vice versa. As for the condition a datum must satisfy to rate as evidence, it boils down to relevance. Let us elucidate these concepts.

A *datum* is a particular piece of information of either of the forms "Thing x is in state (or goes through process) y", and "There are things of kind (or possessing property) K". ("Thing x has property P" is a particular case of the former, since a state is a list of properties.) An *empirical datum* is a datum acquired with the help of empirical operations, such as observation, measurement, experiment, action, or a combination thereof. And a *genuine empirical datum* is an empirical datum obtained with the help of operations that are accessible to public scrutiny. This excludes revelation, hunch, and authority.

Not all data constitute evidence for or against an epistemic item, such as a hypothesis, or a value judgment concerning the efficiency of a plan or the performance of a machine. Only relevant data may constitute evidence. An empirical datum may be said to be *relevant* to an epistemic or an evaluational item only if it refers to the latter. If preferred, a datum is relevant to a proposition if both share at least one predicate. For example, information about the performance and price of a machine are relevant to any statement about the profitability of the machine. On the other hand price and performance are mutually irrelevant even though both refer to the same machine, for a machine may be expensive yet perform poorly, or inexpensive and perform satisfactorily. In other words, there are neither laws nor rules that relate price to performance in a regular way: this is why price and performance are mutually irrelevant. (They may be statistically correlated, though.) On the other hand data about crowding are relevant to hypotheses about social cohesion, because the latter happens to depend upon the former.

Relevance and possible lawful relatedness are not enough for a datum to constitute evidence for or against a proposition or a proposal. In addition, every datum must be interpreted as possible evidence. Such interpretation is built into the very construction of measuring instruments or it is explicitly laid out in their operating manuals on the basis of knowledge about their design and operation. Thus we have learned to read time from a watch, and energy from an electric meter. But in most cases data come uninterpreted: we do not know what they "mean" unless we are able to interpret them in the light of some body of knowledge, preferably a theory. For example, the lay may interpret reports on experiences of dying as evidence for life after death, whereas the psychologist will "read" them as hallucinations. And, whereas the traditional Chinese "read" certain fossil teeth as dragons' teeth, the palaeoanthropologist, armed with the theory of evolution, sees them as remains of ancient apes or hominids. In short, the raw datum is no evidence: it becomes evidence when suitably interpreted in line with some of our background knowledge. (cf. Bunge, 1967b.)

We summarize the preceding observations into the following definition. An empirical datum e constitutes *empirical evidence* for or against a proposition or a proposal p (another datum, a hypothesis, a plan, a value judgment, etc.) if, and only if, (*a*) e has been acquired with the help of empirical operations accessible to public scrutiny (rather than made up, conjectured, taken from authority, or obtained by allegedly paranormal means): (*b*) e and p share referents (or predicates); (*c*) e has been interpreted in the light of some body of knowledge, and (*d*) some regular association (law or rule) between the properties represented by predicates in e and in p is (rightly or wrongly) assumed to exist.

The empirical operations referred to in clause (*a*) above are seldom purely empirical, particularly in modern science and technology. Thus the land surveyor employs scientific instruments, such as theodolites, together with geometry. Physicists, chemists and biologists use far more sophisticated instruments in conjunction with even more refined hypotheses. And, increasingly, they also use computers to drive microscopes, time the taking of pictures, control micromanipulators or servomechanisms, order the taking of measurements, and plot and even process the results of the latter. In all these cases a number of complex theories are involved in the design, performance, and utilization of empirical operations. In short, refined and exact empirical data are anything but theory-free perceptual reports. More in Section 3.

Data are seldom error-free. In particular, quantitative experimental data are likely to be subject to errors of two kinds: systematic (deriving from bias or defective design) and random (deriving, e.g. from random thermal motion and random external perturbations). We shall return to this point in Section 3. Let us now note only that, because data may not be error-free, they must be checked instead of being taken at face value. The new (checking) run of empirical operations may be done with the same technique or, preferably, with a rival (equivalent or better) technique. (Thus the ages of terrestrial rocks are estimated with a variety of methods, such as radioactive decay, accumulation of helium produced by cosmic ray impact, and even models of the evolution of the solar system.) Furthermore, ideally the checking is done by independent workers in order to minimize personal bias. Calculations are parallel.

In sum, the results of observations, measurements or experiments are not to be accepted at their face value but must be interpreted and screened. In particular, numerical data must be corrected in a number of ways, e.g. for parallax, pressure dependence, and sampling—not to speak of bias or systematic errors. Once corrected, the data are subjected to statistical processing, which yields aggregate data such as averages together with random errors (or scatters). In the process some data get eliminated, either because they are outlying (hence likely to be effects of artifacts or bias), or because they are in flagrant contradiction with well-confirmed theories. (Some outstanding scientists, such as Ptolemy, Newton, Mendel, and Millikan, have been accused of fudging or doctoring data in order to save their pet theories. This accusation is somewhat unfair: every good scientist knows that no data are to be taken at their face value.) The net result of a run of empirical observations yielding numerical data is a table or schedule from which a graph can be constructed, which in turn suggests a function. In short, the entire process from raw data to final evidence is the following chain:

$$\textit{RAW DATA} \xrightarrow{\textit{Interpretation}} \textit{EVIDENCE} \xrightarrow{\textit{Correction}} \textit{CORRECTED EVIDENCE} \longrightarrow$$

$$\xrightarrow{\textit{Statstical processing}} \textit{AGGREGATED DATA} \longrightarrow \textit{PRESENTATION.}$$

Administrators are often reluctant to fund the checking of data and calculations: they tend to see it as wasteful duplication of research. Yet rechecking is of the essence of scientific research. Besides, strictly speaking there is never exact replication, for there are never two identical research teams working in exactly the same circumstances. If smart enough, one of the teams is bound to find something—a new fact, a new hypothesis, a new method, or a new problem—that the other missed. And the team engaged in checking the work of others is bound to find some errors in the assumptions, procedures, or results of the first team. The outcome may be a more thorough checking, perhaps to the point of demolishing the earlier work or even showing that it had been triggered by an ill conceived problem.

As a matter of fact checking and rechecking are not the prerogative of scientists and technologists. Every rational man checks before, while, and after he acts or thinks. The horseman (or motorist) checks his mount (or car) before mounting (or driving); the craftsman checks his tools and raw materials before working with them; the biologist checks his preparations and instruments before and after conducting his experiments; the theoretical scientist checks his assumptions and the results of his calculations. Checking and rechecking is of the essence of good craftsmanship and of the practice of science and technology, just as it is alien to pseudoscience and prescientific ideology.

Practical men must have appreciated the value of checking procedures and things for thousands of years, or else they would not have left descendants. But the realization of the value of checking ideas seems to have dawned only recently. The first proof of a mathematical statement seems to be only 2,500 years old, and the first scientific experiments were not performed until the 17th century. For thousands of years physicians have prescribed cures, judges passed sentences, and religionists pronounced dogmas, without sufficient evidence and sometimes with no evidence at all. Most of mankind has lived and continues to live affirming or denying, at best debating, ideas supposed to be so important that they were deemed to be above checking. Only the humble hunters or farmers, stonemasons or scribes, took it upon themselves to check some ideas about their own work while allowing their economic, political and cultural leaders to mislead, oppress and exploit them in the name of unchecked or false principles.

This situation has changed, at least in the sciences, technologies and some branches of the humanities. In these fields it is now recognized that all truth claims—such as "p is true" and "p is truer than q"—and all efficiency claims—such as "p is effective" and "p is more effective than q"—are to be justified or validated by some objective means or other. However, philosophers are still divided on the acceptable ways of justifying truth claims, and they have not reflected much on the ways of validating efficiency claims. Leaving aside intuitionists—for they care only for self-evidence—and conventionalists—for not being interested in truth—we recognize the following philosophical camps with regard to the problem of the justification or validation of truth claims: see Table 11.1.

Unanimism (e.g. Ziman, 1979) holds that intersubjective agreement is necessary and sufficient for validation. Counterexamples: (*a*) the agreement of a thousand theologians concerning the existence of a deity does not prove it; (*b*) radically new theories are likely to be initially rejected or ignored by the great majority. *Pragmatism* (e.g. James, 1907) equates truth with success and therefore accounts for neither. We all know that some successful doctrines are false, whereas some true theories have found no practical application. *Rationalism* (e.g. Leibniz, 1703) holds that ideas are tested by checking their coherence with a body of basic (and possibly innate) beliefs. But coherence is insufficient: think of any consistent ideology and of that of some madmen. Besides, there is no reason to exempt

TABLE 11.1
How different philosophies of knowledge view the validation of truth claims

Philosophy	*Necessary and sufficient condition for validation*
Unanimism	1 Consensus of experts
Pragmatism	2 Practice (individual or social)
Rationalism	3 Coherence with background knowledge
Empiricism	4 Positive evidence
Critical rationalism	5 Lack of negative evidence
Critical realism	6 Numbers 3 through to 5 above

the principles themselves from rigorous checking. Nevertheless rationalism has an important grain of truth, namely that coherence is necessary: we dismiss hypotheses that are blatantly inconsistent with data, as well as data that fly in the face of well-confirmed theories.

Empiricism (e.g. Reichenbach, 1938) too contains an important grain of truth: experience is indeed a (though not the) test of truth and efficiency. In particular, positive evidence for the predictions calculated with the help of a theory confirm the latter. *Critical rationalism* (e.g. Popper, 1959) agrees that experience is a test of theories (its only concern) but claims that only negative evidence counts (against), for positive evidence is too easy to come by. True, unsuccessful attempts to refute a theory (or discredit a proposal or an artifact) are more valuable than mere empirical confirmation. However, (*a*) the most general theories are not refutable, although they are indirectly confirmable by turning them into specific theories upon adjoining them specific hypotheses (Bunge, 1973b); (*b*) true (or approximately true) predictions are not that cheap, as shown by the predictive barrenness of pseudoscience; (*c*) positive evidence for the truth of an idea or the efficiency of a proposal, procedure, or artifact, does count: thus the US Food and Drug Administration will rightly demand positive evidence for the efficiency of a drug before permitting its marketing.

Critical realism is a sort of synthesis of rationalism (the coherence requirement), empiricism (positive evidence), and critical rationalism (negative evidence), plus the realist thesis that the theories in science and technology represent (poorly or accurately) parts or aspects of the real world. I submit that critical realism is the tacit epistemology and methodology of science and technology. True, there is never overall coherence because new ideas are bound to be inconsistent with some bits of background knowledge; but this only shows that the latter is changeable and that justification or validation is never final. True, all experience is partial and inconclusive; but it can be enriched and checked by further experience as well as by theory. True, lack of negative evidence is valuable; but it is not enough, particularly since it is not more certain than positive evidence. In sum, critical realism demands at the same time coherence with the (changing) background knowledge, substantial positive evidence, and lack of significant negative evidence. It demands it not only from science and technology but also from the nonformal branches of philosophy, namely ontology, epistemology, and ethics. This empirical constraint on philosophy allows one to write off, as mere phantasies, such doctrines as that the mind is immaterial, that all knowledge is a matter of convention, and that moral behavior is unaffected by society. We need not waste much time in examining the ingenious arguments offered to buttress such phantasies: what matters is that they do not agree with the facts—as shown by their incoherence with our background knowledge and the absence of any positive evidence for them. (More on critical realism in Ch. 15, Sections 2 and 3.)

Note that we have been concerned with truth and efficiency claims rather than with beliefs. (Recall Ch. 2, Section 3.2 on the difference between knowledge and belief.) On the other hand the currently fashionable epistemologies concern belief. In particular the so-called coherence theories (which are really views rather than theories) hold that a belief is justified only if it coheres with all the other beliefs in a given belief system; and the "foundation" theories add that those beliefs that are most certain are to be taken as the foundation of our knowledge. Such epistemologies, which constitute a version of traditional rationalism, are ideally suited to religious and political ideologies, for coherence is the most that such bodies of belief can claim. By the same token those epistemologies are unfit for science and technology: here we are supposed to know (however provisionally) that a hypothesis has a high degree of truth, a procedure a high degree of efficiency, or a machine a high degree of reliability, before we believe in their adequacy. And such knowledge must be an outcome of research, both empirical and theoretical, not just a matter of checking coherence with a body of beliefs.

We hold that the study of belief, though a legitimate concern of epistemology, must be conducted like the study of any other real process, namely empirically as well as theoretically. Belief is mainly a concern of individual and social psychology. An armchair study of belief, yielding an a priori theory, is necessarily poor and dogmatic: poor because it makes no use of experiment, and dogmatic because it does not care for empirical validation. Take for instance the traditional beliefs on the correlation between ideology and social class. No a priori speculation is an adequate substitute for an empirical investigation into the actual correlation between, say, belonging to the working class and sympathizing with socialism, or belonging to the petty bourgeoisie and voting liberal. (See, e.g. Boudon, 1967.) Another example: only empirical investigations have established that (*a*) many popular superstitions (e.g. belief in telepathy) are formed by the natural tendency to conjecture causal relations, even about conjunctions of events that are nothing but coincidences or statistical flukes (i.e. chance accumulation or thinning of events in random sequences), and (*b*) people who are properly trained in probability theory and statistics are less prone to forming such beliefs. (See, e.g. Tverksy and Kahneman, 1977; Falk, 1982.) In sum, only scientific research into belief can yield genuine knowledge of belief: the nonscientific study of belief yields only further belief. And only scientific or technological research can tell us under what conditions it is reasonable to hold, withhold, or reject specific beliefs. In short, first knowledge through research—in particular knowledge about truth and efficiency—then credence.

In conclusion, claims of truth or efficiency must be validated by some evidence. Evidence can be conceptual or empirical. (However, the support a factual theory lends a proposition or a proposal may be regarded as indirect empirical evidence. And an empirical datum is evidence for or against a proposition or a proposal just because some theory suggests so.) Evidence can be positive, negative, or inconclusive. And in principle neither positive nor negative evidence is definitive, if only because further research may show that it is inadequate.

2. Testability and Indicators

2.1. *Testability*

A proposition will be said to be *empirically confirmable* if there is direct or indirect, actual or potential empirical evidence for it; and *empirically refutable* if there is direct or indirect, actual or potential empirical evidence against it. A proposition that is only confirmable, or only refutable by empirical data, will be said to be *testable*. Finally, a proposition will be said to be *strongly testable* if it is both confirmable and refutable, and *untestable* if it is neither. (Concepts are testable for relevance and power but not for truth: only propositions are testable for truth.)

Because empirical data are obtainable by certain means but not others, and only provided the state of the art has attained a certain level, testability is not an inherent property of propositions but is *relative* to the available or conceivable empirical means. It may happen that a proposition is testable by data of a certain kind but untestable by data of another; or that it is better testable by data of one kind rather than another; or that it is more testable than another proposition relative to a given body of data. In sum, testability comes in degrees and is relative to the (empirical and conceptual) test means. So, any theory of degrees of testability would have to elucidate expressions of the forms "The testability of p relative to means m equals t", "p is better testable by m than by m'", and "p is more testable than q with the help of m".

Note that we have made room for potential data or, what amounts to the same, for testability *in principle* alongside actual testability. For example, a historical hypothesis conceived to explain certain data (or their absence) is likely to suggest the search for new evidence of a certain kind, which search may stimulate the invention of new techniques or the application of techniques evolved in other research fields. Another example: it is not always possible to determine, with today's means, whether a certain enzyme is present in a given chemical system or organism, because the substrate-enzyme complexes are unstable and therefore short lived. So, many conjectures in biochemistry, though potentially testable, are not yet actually testable. (According to the well-confirmed Michaelis–Menten hypothesis, a reaction of the form $A \rightleftharpoons B$ catalyzed by C actually summarizes two reactions involving the substrate-catalyzer complex AC. They are: $A + C \rightleftharpoons AC$, which is reversible, and $AC \rightarrow B + C$, irreversible. If the corresponding rate equations fail, we hypothesize that a different or a further catalyzer is at work.)

We have also included *indirect* empirical evidence, i.e. evidence through some intermediary body of knowledge. For example, "There have been dinosaurs" is not directly testable, for there are no living dinosaurs nowadays but only certain fossil bones, eggs, and footprints. The hypothesis itself allows one to interpret such data as evidence for it. (Other historical hypotheses are parallel.) However, far from being a stray conjecture, the dinosaur hypothesis is part of the vast body of evolutionary biology, which in turn is consistent with the rest of biology as well as with geology. Or take the hypothesis of interactionist mind-body dualism, namely "Mind and body are distinct though mutually interacting substances" (Cf. Popper and Eccles, 1977.) This hypothesis is confirmable because it accommodates any conceivable mental event; by the same token it is not directly refutable. However, the hypothesis is indirectly refutable, namely by showing that it is incompatible with physiological psychology and even with physics. Indeed any action of an immaterial mind on matter (e.g. a brain) would violate the principle of conservation of energy. (Cf. Bunge, 1980a.)

Other hypotheses are confirmable but irrefutable in principle, hence only weakly testable. For example, the hypothesis of hypnotic trance is immune to refutation because, if the subject obeys instructions, he is declared to be in such a state, whereas if he does not, he is said not to have been properly hypnotized (Barber, 1970). Another hypothesis of the same methodological type is "All persons seek to maximize their utilities". This axiom of utility theory (and of much of neoclassical economics) is confirmed, never refuted, by whatever any person does: even altruism and suicide can be interpreted as maximizing the person's moral if not economic utilities. Therefore it is weakly testable.

Finally let us exemplify the notion of an untestable proposition. Utter untestability can be due to either imprecision or the positing of inscrutables. Gestalt psychology is an example of a system of propositions that, because of their vagueness, are hardly testable (Claparède, 1934, p. 145). In such cases one does not know exactly what is to be tested. However, some imprecise propositions are worth being exactified and rendered testable. (*Example*: "Curiosity can be smothered or trained".) Note that exactness is necessary but not sufficient for testability. Indeed any proposition positing the existence of inscrutable entities is untestable even if precise. Here is a random sample of untestable propositions of this kind. (*a*) "We are surrounded by things that are unknowable in principle". (Kant's thing in itself was of this kind for him.) (*b*) "Physical space is embedded in a higher dimensional space, but we happen not to have access to the extra dimensions". (*c*) "There are things deprived of energy". (Such things could not possibly exchange energy with ordinary things, such as particles or fields, so they would go undetected.) (*d*) "Good people go to heaven, bad ones to hell".

There can be no empirical evidence for or against vague propositions or for propositions positing inscrutables. The same holds for some counterfactual conjectures, such as "If Einstein had not invented general relativity then someone else would have done it". All such propositions are empirically untestable. However, they may be discarded by the simple philosophical manoeuvre of asking their propounders "How do you know?" or "What evidence have you got?" (Parenthetically this mode of refutation is not available to Popper (1963), who regards such questions as inappropriate and, in general, matters concerning our information sources as unimportant.)

Let us now examine certain kinds of hypotheses whose methodological status seems uncertain: partial differential equations, ad hoc, existence, continuity, possibility and probability hypotheses, and estimates of nuclear weaponry capability. *Partial differential equations*, a standard tool of physicists, astronomers, engineers and many others, have infinitely many solutions. This richness is theoretically valuable but methodologically embarrassing, for it renders such equations testable in too many ways. (*Example*: any differentiable function of the argument $x - vt$, where x and t are independent variables and v a constant, solves the equation $(\partial u/\partial t) + v(\partial u/\partial x) = 0$. The latter may be partially interpreted as "For every entity k of species K, the u-ness that k gains or loses in the course of time is compensated for by a loss or gain in the u-ness of k effected by a change in place". Among others, field-like and particle-like solutions of the equation exist. For a dètailed analysis of a theory generated by that hypothesis see Bunge (1967d).)

In other words, a partial differential equation offers a large target to empirical tests, but the results of these are inconclusive for bearing on the solutions not on the equation itself. (The situation is even worse with regard to the Lagrangian functions that, via extremum principles, generate those equations. Indeed every equation of motion or field equation can be generated by any of an infinity of such functions.) In other words, not the equations themselves together with their infinitely many consequences, but only some of the latter are confirmable by empirical data: see Figure 11.1. If data of one kind fail to confirm a given solution, then data of another may confirm it. Likewise if data of a given kind refute one of the solutions, they may confirm another. So, the original equation itself can hardly be said to be refutable—at least not until all its solutions have been tested. However, in practice the decision is considerably eased by three considerations. One is that a partial differential equation is usually one component of a whole problem system that contains also a number of restrictions such as initial or boundary values, which restrictions narrow down considerably the set of possible solutions. Another is that not every solution is interpretable in suitable factual terms; those which are not so interpretable are just

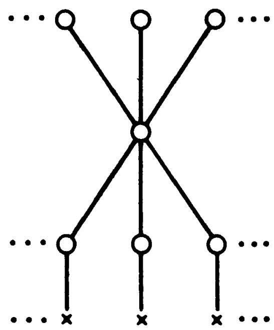


Fig. 11.1. One and the same equation of motion (or field equation) is derivable from alternative Lagrangians, and has different solutions. Data of a certain kind may favor one such solution.

dropped. A third is that the equation entails not only its own solutions but also a number of additional theorems—perhaps conservation laws—some of which are empirically testable. Nevertheless the evaluation of such high level hypotheses is anything but straightforward.

Next in our list of hypotheses that arouse some methodological unease are *ad hoc* hypotheses. An *ad hoc* hypothesis is a particular assumption, i.e. one covering a rather narrow range of facts. We distinguished two kinds of *ad hoc* hypotheses: *bona fide* and *mala fide*. (Ch. 8, Section 4.2.) A *bona fide ad hoc* hypothesis is normally offered to represent facts of a certain kind and it is independently testable. For example, to complete his theory of the circulation of the blood Harvey had to conjecture the existence of tiny vessels, that remained invisible during his lifetime, connecting arteries and veins; this hypothesis was confirmed later on with the help of the newly invented microscope. On the other hand a *mala fide ad hoc* hypothesis is designed exclusively to protect another hypothesis, or a theory, from refutation: it is a cyst. A classical example is Freud's repression hypothesis, which saves the Oedipus complex fantasy in case the individual fails to exhibit such "complex". Another is the parapsychologist's claim that the presence of skeptics inhibits the mind reader and the clairvoyant. *Mala fide ad hoc* hypotheses are to be avoided because they block the test process. On the other hand there is nothing wrong with *bona fide ad hoc* hypotheses. The earth sciences teem with testable hypotheses that, presumably, hold only for our planet, and are therefore both *ad hoc* and *bona fide*.

The *existential hypotheses*, i.e. conjectures of the form "There are F's", are logically modest but ontologically ambitious and therefore methodologically tricky. Indeed they can be confirmed by exhibiting instances but they are hard to refute for, if an effort to find F's fails, one may still hope that more strenuous efforts will meet with success. But of course no such efforts will be made unless there is some evidence, empirical or theoretical, for the existence of F's. Columbus had both when he undertook his first voyage; so had Le Verrier and Adams when they predicted the existence of Neptune, and Hertz when he predicted the existence of electromagnetic waves.

Mere argument neither proves nor disproves existence—except of course in theology. In formal science existence is either postulated or proved from postulates with the help of logic. In factual science and technology existence conjectures are supported or undermined by both theory and empirical evidence. The former suggests, the latter establishes more or less conclusively. And in a science-oriented philosophy existence claims should be treated in like manner: they should be postulated or proved rigorously in the case of constructs, and supported by empirical evidence in case they concern concrete things. No matter how seductive an argument for the existence of deities, disembodied minds, propositions in themselves, and the like, it will cut no ice in scientific philosophy unless it can be supported by empirical evidence.

To be sure, theoretical considerations in science or technology, though powerless to prove or disprove the existence of any concrete thing, can render an existence hypothesis likely or unlikely, hence worthy or unworthy of being investigated. This they can do by showing that the given existence hypothesis is necessary for consistency or, on the contrary, leads to contradiction—as was the case with the aether hypothesis. Or such theoretical considerations may show that, if the entity in question were to exist (or not to exist), certain well-confirmed theories would fail. Thus no physicist can admit the existence of an entity that fails to conserve energy, or the nonexistence of a massive body, even if invisible, at a focus of the elliptical orbit of a celestial body. Still, the most conclusive proof of existence, whether in mathematics, science, or technology, is to exhibit a specimen of the object whose existence has been postulated.

Being empirically irrefutable, existence hypotheses have been declared untestable and therefore unscientific or metaphysical (Popper, 1959; Lakatos, 1978). However, this only goes to prove that testability is not to be equated with refutability. First, if we do find an F we have proved (until new notice) that there are F's. (To be sure such evidence is not final—but then no empirical evidence is.) Second, the refusal to admit existential hypotheses entails giving up some of the most powerful and best confirmed scientific theories. Take for instance the core of classical mechanics (or of its relativistic generalization): "For all bodies and all forces and stresses, at all places and all times, *there exist* reference frames relative to which the rate of change of the linear momentum (or the momentum density) equals the body force plus the divergence of the stress tensor". Finding one reference frame relative to which this equation of motion holds, confirms the hypothesis; such a frame is called *inertial*. And finding any number of frames relative to which the hypothesis fails (e.g. our own planet) serves only to indict the frames as being non-inertial, not the hypothesis as false. Third and most important, the refusal to countenance existence hypotheses just because they are irrefutable, though consistent with conventionalism and pragmatism, is inconsistent with any realistic epistemology, for which the aim of science is to understand existents (and real possibles). This epistemology is inherent in the careful formulation of any scientific theory, where one begins by assuming that the reference class of the theory is nonempty—i.e. that the entities described by the theory *exist* on their own.

Still, could it not be that a theory passes some empirical tests and yet some or even all of its referents do not actually exist? After all there are plenty of historical examples of theories postulating nonexisting objects, such as caloric, aether, and magnetic monopoles. A first answer is that no final certainty is attainable in this case or in any other: the most one can obtain is solid though corrigible evidence. Second, a realist epistemology, one regarding the postulated entities as possibly existing rather than being fictions, will be more helpful than either an idealist or a conventionalist one. For, even if the objects postulated by the theory prove to be ghostly, the search for them is likely to yield some knowledge. (For example, the failure to find magnetic monopoles confirms once again standard electrodynamics.) Thirdly, there are no separate and fool-proof existence tests. In short, existential hypotheses are at the very core of science although, being only confirmable but irrefutable, they are not strongly testable.

Continuity hypotheses are of the form "The function F, which represents the property P of things of kind K, is continuous (or piece-wise continuous) over the domain D". Obviously, no measurement could possibly provide a direct test of such a hypothesis, for only a finite number of values of a function can be measured. So, we must resort to indirect empirical tests, such as those bearing on some consequence of the hypothesis. An example will elucidate this point. Suppose the available data are consistent with the following alternative hypotheses concerning a certain force: (a) the force varies continuously with the inverse of the distance, and (b) the force varies with the inverse of the distance when the latter is a rational number but is zero otherwise. To decide between the two hypotheses we may measure the work done by the force along a certain stretch. The result will be a nonzero quantity in the first case and zero in the second. (The work equals the definite integral of the force over the given distance. The integral is null in the case of the second function because it is defined on a set of zero measure.) In sum, continuity hypotheses are empirically testable though in an indirect manner. Moreover they are not only confirmable but also refutable: indeed any significant discontinuity in the corresponding property of their referents will refute them.

A *possibility hypothesis* is, of course, one stating that facts of a certain kind are possible. (In turn, real possibility equals compatibility with law: see Vol. 3, Ch. 4.) In science and technology possibility hypotheses are stated on theoretical considerations. For example, a spectral line or band is declared possible if the corresponding atomic or molecular transition is compatible with the atomic or molecular laws, and "forbidden" otherwise. And a technological project is declared to be in principle feasible if it involves no violation of any known laws—otherwise in principle unfeasible. Clearly, possibility hypotheses are confirmable but irrefutable on purely empirical grounds. The only possible empirical evidence relevant to a possibility hypothesis is of the positive kind: we prove that X is possible the moment we see X happen either naturally or through human intervention. (If X fails to happen we can still argue that it is too soon to write it off—or that, given the changed circumstances, it is no longer possible.) Actuality is then the only empirical evidence for possibility. (But this does not mean that actuality is the meaning of possibility, e.g. that probabilities mean frequencies.)

As for *probabilistic hypotheses*, they all pose the following problem. If the set of events under observation (i.e. the sample space) is smallish, whatever happens may be regarded as confirming the hypothesis; and if nothing happens the hypothesis cannot be said to have been refuted, for accidental crowdings and thinnings are of the essence of chance. For this reason a number of authors, among them Popper (1959), have regarded probability hypotheses as untestable or nearly so. However, this difficulty disappears as soon as huge masses of events become available: in such cases the probability of any deviation between probability and observed relative frequency approaches zero. This is actually the case with the various probabilistic hypotheses involved in particle and field physics, statistical mechanics, quantum chemistry, genetics, and to some extent social science as well. Thus even if the probability of an individual atomic collision (or genic mutation, or conversion of proletarian into tycoon) is extremely small, collisions (or mutations or dramatic changes in economic power) will happen in large aggregates of individuals or in the very long run.

Randomness is characterized not only by stable (or regularly varying) long run frequencies, but also by short term fluctuations, i.e. occasional high or low scores—e.g. streaks of good or bad luck. Only if the scores are consistently higher or lower than the calculated probability can we infer that the latter has been refuted. Typically parapsychologists ignore this peculiarity of random events when claiming that the psychic powers of their sensitives is "above chance" during certain periods and then—particularly when watched by skeptics—decline. In short, probabilistic (or stochastic) hypotheses are empirically testable by huge masses of like events: a single unfavorable case will not refute them. However, this only shows that we must make a philosophical decision: either we give up some of the deepest and best confirmed scientific theories—such as the quantum theories and genetics—or we reject any methodology that regards probabilistic hypotheses as untestable, and therefore unscientific, just because they are not tested the same way as nonprobabilistic hypotheses.

Let us now discuss the testability of hypotheses concerning the efficiency of weapons or strategies of certain kinds. As is well known, the Nazis subjected Britain to an intense bombing—only to unite and strengthen the Britons as well as to elicit universal sympathy and solidarity with them. Ignoring this teaching, subsequently the U.S. and British air force subjected Germany to an intense bombing in order to destroy the German economy and, in particular, its arms industry. This operation went on for nearly three years without the slightest indication of its effectiveness. Shortly after the war the U.S. Strategic Bombing Survey found that the bombing had been a dismal failure or worse, namely counterproductive. In fact the German arms production increased under the bombing: the production of tanks tripled, and that of planes doubled between 1942 and 1945. (Apparently the military did not relish this finding that refuted one of its most cherished dogmas: see Galbraith, 1981, Ch. 13.) Nowadays we face an even tougher problem. For the first time in history man has produced engines, namely intercontinental missiles with nuclear warheads, that are untestable. Indeed, if any of them were put to the test World War III could start, and presumably there would be nobody left to evaluate the damages. So, nobody can know exactly what those engines are worth—except of course in terms of the impoverishment of our lives. In this case speculating over the effectiveness of such artifacts is preferable to testing them. And in any event it is ironic that technology, with the help of testable and moreover well-confirmed theories, should have produced untestable artifacts.

Let us now compare constructs of different types as to testability. Obviously, such comparison is greatly facilitated if the constructs represent the same aspect of things of the same class, such as radiating antennas. Consider on the other hand the equivalence "For every x, if x is an organism, then: x has genotypic trait G iff x has phenotypic trait P". Since phenotypes are more easily recognizable than genotypes, the LHS of this equivalence is harder to test than its RHS. Equivalents are not necessarily equitestable. (*Methodological moral*: Logic is a poor guide to investigate testability.)

Assuming unlimited empirical means, singular propositions are better testable than universal ones, which in turn are more fully testable than hypothetico-deductive systems (theories). Thus "That antenna is radiating electromagnetic waves of such and such wavelengths in such and such direction" can be checked more fully than any particular formula of the theory of antennas, which theory is in turn even harder to test. Not that testing a singular proposition, i.e. one concerning an individual, is always plain sailing. Think of the extraordinary difficulties met in testing singular propositions of the form "That astronomical object is a black hole", and "This organism has just undergone a mutation". In neither case do observations suffice: in both cases the tests are extremely indirect and their results rather inconclusive.

General propositions are even harder to test than the corresponding singular propositions if the former concern a large or variable collection of individuals, as is the case with any basic law statement. True, in principle a single unfavorable case refutes the claim to universality—provided the evidence is reliable. But such result does not dispose of the generalization. In fact, though false in a given range, the generalization may hold in another, and this can be established only by continuing the tests after the first disconfirmations are in. Never mind the real motives of the scientist or technologist performing such tests: he may wish to confirm or to disconfirm. What matters is that the generalization can be pronounced false if nearly all the tests, over the whole range, are negative, and true (to some extent) in a given range if nearly all the tests in that range turn out to be positive. However, it may also happen that the tests are inconclusive, in which case a new, more precise testing technique may have to be tried.

Paradoxical as it may sound, single generalizations are in general harder to test than generalizations included in theories. The reason is that, whereas an isolated generalization can count only on the evidence bearing directly on it, a hypothesis belonging to a hypothetico-deductive system can also count on whatever evidence favors other components of the system. In other words, a systemic hypothesis can enjoy both direct and indirect empirical support. See Figure 11.2. Such indirect support is multiplied if the theory in question agrees with other theories in the same or different fields. For example, whereas a biological (neurophysiological) theory of mental functions can be supported by neuroscience and, indeed, by the whole of biology, a dualistic theory of mind cannot enjoy such support.

A theory is composed of infinitely many propositions (Ch. 9, Section 1.2). Therefore it can never be exhaustively tested: one must always confine oneself to testing a finite subset of the infinite set of propositions. Now, scientists do not test arbitrarily selected theoretical propositions but only those that interest them most—e.g. because they are the most novel—or that lend themselves more easily to tests. This tends to produce clusters of frequently tested propositions, alongside with pockets of propositions that are seldom if ever checked. There would be nothing wrong with this procedure if all the propositions of a theory had the same truth value, but this is not the case. Even highly successful theories, such as quantum electrodynamics, contain false propositions alongside others which are true or nearly so.

A solution to this problem would be to treat theories the way concrete populations are handled, namely to draw random samples. We might collect all the known and testable propositions of a theory, assign them random numbers, and draw lots. In this way the theory could be likened to

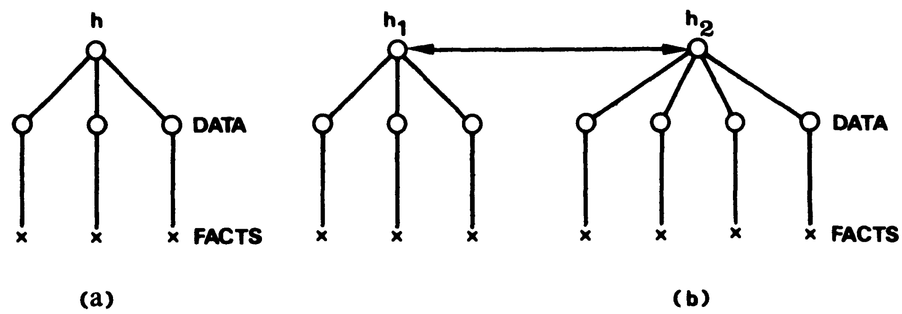


Fig. 11.2. The empirical support of hypotheses. (*a*) A single hypothesis is supported or undermined only by the facts it refers to. (*b*) A hypothesis belonging to a hypothetico-deductive system (theory) is controlled not only by direct evidence but also by whatever data are relevant to the hypotheses to which it is logically related.

a bag containing infinitely many marbles of unknown colors in unknown proportions. In the simplest case there would be only two colors, say white (T) and black (F), in unknown proportions. A test of the theory could thus be likened to the extraction and examination of marbles, one at a time and without replacements—except of course when one has reason to suspect some data and thus repeats the test. What are we to conclude if 100, 10,000 or even 1,000,000 balls turn out to be all white (T) or all black (F)? Strictly speaking *nothing*, for (a) there are still infinitely many unchecked marbles in the bag, all of which could prove to be black (F) in the first case and white (T) in the second; and (b) although the tested sample is a random one, the original population—in turn a finite subset of the set of all the propositions in the theory—was not, for we can know only those propositions in the theory that, for some reason or other, happen to have attracted the attention of theorists. And if the outcome of our test is that w marbles out of a total of n turned out to be white (T) and b black (F), all we can say is that the degree of confirmation of the theory (up to the nth test and with the given means) is $w/(w + b)$. But, for the above reasons, this datum is not very helpful except when the total number of tests is very large and the degree of confirmation is either nearly 1 or nearly 0. In the first case we may conjecture that the theory is true, in the second that it is false. But of course we must keep an open mind, as a further run of tests may force us to change our evaluation. Truth values, like the value of money, tend to depreciate in time.

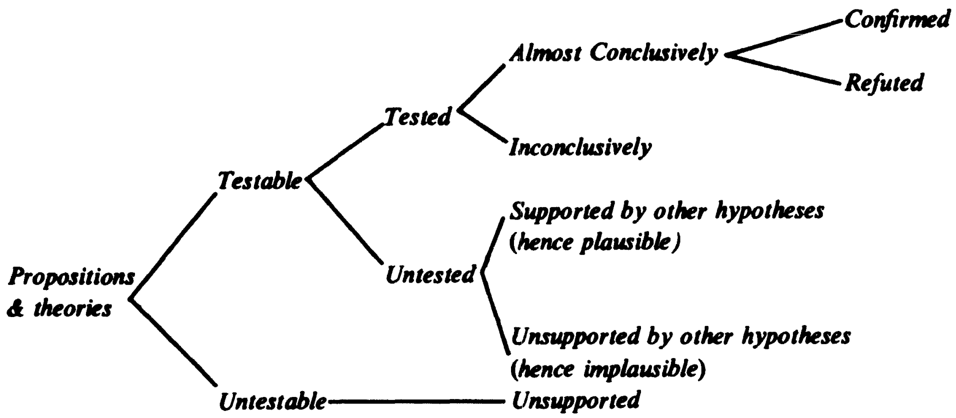
If theories in general are hard to test, the most general among them are the hardest. To be sure, if testable at all they are confirmable, but not necessarily refutable as well. Take for instance the most basic formulas of classical (or relativistic or quantum) mechanics. Undoubtedly they do have some general testable consequences such as the conservation theorems. However, in order to test any basic hypotheses we must adjoin them specific assumptions concerning the precise composition, environment, and structure of the system in question, be it atom, binary star, or machine, for we will contrast the theory with data concerning objects of certain kinds, such as atoms, binary stars, or machines. That is, we must conceive of a model object couched in the language of the theory: one specifying, say, what the components of the system are and how they interact with one another and with the environment. (Shorter: we must specify the Hamiltonian or the forces and stresses, the constraints and the initial and boundary conditions, and the constitutive relations characterizing the type of matter in question.) The outcome of this theoretical activity is a model, or specific theory, representing the object in question. This, not the general theory to which it is bound, is put to the test (Bunge, 1973a).

Since models (or specific theories), not general theories, are the ones that are subjected to empirical tests, the outcome of the latter, if negative, does not tell us unambiguously what went wrong: whether the general theory or the special assumptions adjoined to it. To be sure, uncertainty shrinks if a variety of models using the same general theory either succeed or fail. But even so some uncertainty remains. The only clear cut cases are exceptions: when none of many models grafted on to a general theory is confirmed, we are justified in blaming the latter; whereas if only a few fail, we blame the special assumptions. This methodological predicament is common to all extremely general theories. Thus the theory of evolution is so general as to be confirmable but not refutable. It becomes also refutable, and thus strongly testable, only when the genetic composition and the environment are specified, i.e. when applied to a particular lineage, over a long period of time. (See Bunge, 1978b.) What holds for the extremely general scientific theories holds, a fortiori, for the philosophical ones that concern the real world or our knowledge of it. Only the specific scientific theories based on those philosophical theories are strongly testable (Bunge, 1973b).

Let us remember that every factual theory, whether scientific, technological, or philosophical, has two ingredients, namely the formalism and the interpretation. So either or both can go wrong, and in some cases a correct interpretation can somehow compensate for a defective formula or conversely. Therefore if the theory is disconfirmed we may try altering portions of its formalism, its interpretation, or both. And although this is an eminently theoretical job, it cannot be conducted except in the light of the available empirical evidence. For example, there is consensus that the mathematical formalism of quantum mechanics is essentially correct, but there is disagreement about the correct physical interpretation of the theory. Yet it is possible to choose among the existing rival interpretations on both theoretical and empirical grounds. In particular, we can refute the subjectivistic interpretation of quantum mechanics just by noting that, if it were correct, (*a*) the theory would include psychological concepts (such as those of the state of knowledge and the intentions of the experimenter)—which it does not; consequently (*b*) psychological data (about the observer) would be relevant to both calculations and measurements—which they are not; and (*c*) the outcome of the empirical tests would depend critically on who performs them—which it does not. (More in Bunge (1967c) and (1973a).)

Finally, what about metaphysics or ontology? If defined as an untestable attempt to account for the world, the problem of the testability of metaphysics does not even arise. In this case metaphysics may be granted some heuristic value but it must be denied any truth value. To be sure, most of traditional metaphysics is untestable when intelligible at all. However, metaphysics can be overhauled with the help of exact tools and in such a manner that it becomes continuous with science: see Volumes 3 and 4. For example, it is possible to build a general and exact theory of spacetime compatible with contemporary physics, and a general and exact theory of mind harmonizing with contemporary physiological psychology. Therefore the initial definition must be altered to read: Metaphysics (ontology) is a family of (indirectly) testable theories about the world.

We close this section with a classification of factual propositions and systems of such with regard to their testability.



2.2. *Indicators*

According to radical empiricism scientific theories are data summaries, i.e. they have an empirical (not just factual) content. If this were true, then the testing of any such theory would be a straightforward contrasting of propositions of the theory with the corresponding data. But that is not true: scientific theories have a *factual* reference but, unless they actually describe human experience, they have no *empirical* content.

That scientific theories are not just data summaries, or even mere devices for churning possible data (e.g. predictions), is shown by the fact that they do not refer to the observations, measurements, or experiments aiming at testing them. Thus theoretical mechanics does not describe dynamometers (although it helps design and explain them), and genetics does not describe electron microscopes, which are employed in imaging chromosomes. Instead, scientific theories contain theoretical concepts lacking a counterpart in sense experience, such as those of continuity, free motion, strain, electric field intensity, valence, DNA, neural connectivity, GNP, and theory. As a consequence scientific theories contain propositions without an empirical counterpart. A simple example is that of the "principle" of inertia, a cornerstone of physics and a theorem of classical and quantum mechanics. The "principle" has a factual content since it refers to bodies or particles. But it has no empirical content because it holds only in the absence of any action upon the body of interest, in particular if no empirical tests are performed on it: in short, it does not refer to any empirical situation.

Scientific theories are factual: they concern (authenticated or putatively) real things with properties that are mostly beyond the reach of perception though not of intellection. Therefore they rarely contain empirical concepts. Yet, if they are scientific, theories can be anchored to experience by adding to them links between some theoretical concepts and some concepts having an empirical counterpart. For example, electrodynamics does not deal with pointer readings but gets its support from such operations by virtue of certain formulas that link the field intensity to the torque in a meter. Likewise to test his theories the psychologist looks for behavioral and physiological manifestations of mental phenomena. The physician uses a whole battery of symptoms or diagnostic signs (among them the "vital signs") to evaluate the state of health of his patients. And the economist employs an array of economic and social indicators, such as the steel production and the rate of unemployment, to estimate the state of the economy. All such observables are conjectured, rightly or wrongly, to be lawfully linked to unobservables, so that a measurement of the former allows one to infer (sometimes calculate) the value of the latter. See Figure 11.3.

These observable-unobservable links used to be called *operational definitions* (Bridgman, 1927). But, since they are hypotheses to be tested, not conventions, they had better be called *indicator hypotheses*. An indicator hypothesis is a hypothesis relating an unobservable property of a thing to an observable property of the same thing or of a second thing lawfully linked to the first. The hypothesis may be precise or imprecise, according as it is formulated mathematically or not, but in any case it must

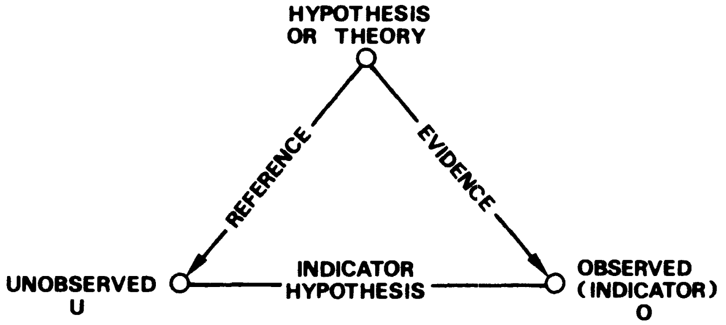


Fig. 11.3. The unobserved-observed (or indicator) relation is expressed by an indicator hypothesis allowing us to infer unobservable things, properties, or events from observation. (Adapted from Bunge (1967a).)

be fully testable and may therefore be improved or replaced with a better one. Far from replacing theoretical concepts, indicator hypotheses are added to factual theories in order to render them testable. Such enrichment of theories in preparation for their empirical tests is sometimes called *operationalization*. A hypothesis or theory that cannot be operationalized, or cannot be logically linked to any operationalizable constructs, is sheer speculation and therefore does not qualify as scientific or technological.

Many, perhaps most of the indicators used in ordinary life and in some professions are imprecise and empirical and therefore unreliable. The cook takes a potato at random from the pot and pricks it with a fork to find out whether it is boiled; but the potato could be rotten, or it could be the only one from the new crop. The naive person takes certain gestures of another as tokens of sincere friendship, while actually they may have been calculated to produce this impression. He is using incorrectly the true generalization "For every x and y, if x is friendly towards y, then x gives y tokens of friendship" to infer friendship from a smile, a kind sentence, or a gift. He is of course indulging in a logical fallacy. A more experienced person would wait for the outcome of further tests; and in future there may be precise philometers.

Typical forms of ambiguous indicator hypotheses are *If U then O*, and *If O then U_1 or U_2*, where O stands for an observable property and the U's for unobservables. In the first case the presence of an indicator (observable) is only a necessary but not a sufficient condition for the corresponding unobservable. In the second case the indicator points ambiguously to two (or more) unobservables. In both cases the indicator–unobservable relation is one–many rather than one–one, and therefore unreliable. Thus the long backlog of a scientific journal may indicate either its prestige, or the scarcity of outlets in its field, or a sudden swell of interest in it, or the poverty of the supporters of the journal, or a deficiency in its administration. Only an independent inquiry may discover the cause(s) of the backlog. Another example: if a political candidate is beaten at the polls, this may indicate that he was not well known or too well known; that he spent too little money or too much; that he was regarded as too dumb or too smart; that he represented the interests of a minority or that most people failed to understand that he did represent them—and so on.

There are two ways of dealing with ambiguous indicators. One is to multiply them, i.e. to employ a whole battery of mutually consistent indicator hypotheses, hoping to reduce the uncertainty. For example, a physician observes symptom O_1, which may indicate causes U_1 or U_2. But he also observes O_2, which never accompanies U_2, so he infers that U_1 is the cause. Even so he may order a few laboratory tests, not only to make sure that U_1 is the cause but also to estimate its strength. Whereas some of these tests are direct, others are indirect, i.e. they yield further indicators. Thus an X-ray plate is direct because it images the suspect bone or organ. On the other hand most of the results of blood tests are indirect.

A second way of reducing uncertainty about unobservables is to conjecture and test one–one indicator hypotheses, i.e. to substitute 1:1 functions for relations. If we suspect or have established that the observable O is a certain 1:1 function f (e.g. a linear function) of the unobservable U, then by inverting f we may compute U from the measured O values, for $U = f^{-1}(O)$. In this case we say that *O measures U*. (Caution: indicators measure unobservables, but only an effective measurement operation can pin numbers on either.) Needless to say, it is desirable, though not always possible, to have different measures of every important unobservable, so that one may check the other.

Here are a few simple examples of functional hence unambiguous indicator hypotheses. (a) The angle of deviation of the pointer in an ammeter connected in series measures the intensity of the electric current in a circuit. (b) The intensity of radioactivity can be measured by the discharge of charged bodies, for this discharge is caused by the ionization of the gas surrounding the radioactive sample. (c) The quantity of ACTH (adreno-corticotropic hormone) released is an indicator of the activity of the adrenocorticotropic system, which in turn is a measure of stress. (d). The time at which two related species diverged is proportional to the fraction of number of kinds of protein that they fail to share: this hypothesis is the basis of the "molecular clock" method for measuring inter-species distances. (*e*) The average length of a bus queue is proportional to the time elapsed since the last bus stopped by.

Whereas some indicator hypotheses have sound theoretical bases and have been abundantly confirmed, others are tentative conjectures with insufficient empirical support. The hypothesis (*a*) underlying all ammeter readings is of the first kind, whereas the hypothesis (*b*), on which the "molecular clock" method is based, is of the second kind. Should rapid speciation events, taking between 5,000 and 50,000 years (like the one reported on by Williamson (1981)), prove frequent, then hypothesis (*b*) is likely to hold only, if at all, for very long periods, of the order of one million years. Another indicator hypothesis that has recently been challenged is the one according to which genotypes are mapped onto phenotypes in a one to one fashion, so that, given a genotype, the corresponding phenotype can be inferred, and conversely. This simple and widely accepted hypothesis is false. First, there are the cases of evolutionary convergence, showing remarkable morphological similarities (e.g. between cetaceans and fish) hiding profound organismic and therefore genetic differences. Second, there are species (or superspecies) within which an extensive karyotic diversity is found underneath little morphological differentiation. (A striking case is that of the spiny rats from Venezuela, the number of chromosomes of which varies between 24 and 62: Benado *et al.*, 1979.) In short, the genotype-phenotype relation is many to one, which renders phenotype a highly unreliable genotype indicator.

Reliable indicators help operationalize a theory, i.e. prepare it for empirical tests, in the following manner. Call $\mathcal{T}$ the general theory to be tested and S the set of subsidiary assumptions specifying particular features of the referent (e.g. the composition, environment, and structure of a system). From $\mathcal{T}$ and S we build the (bound) model $\mathcal{T}'$ of the referent, which is to be subjected to tests. Enter the set I of indicators built with the help of the antecedent knowledge and $\mathcal{T}$ itself. Enter also the set E of available empirical data concerning the referents of the theory and relevant to the latter. Such data are somewhat remote from the theory: for example they may consist of behavioral information, whereas the theory is about mental states. This theory-experience gap is bridged by the indicator hypotheses I: they allow us to "read" behavioral changes in terms of mental processes, benefits from costs and sale prices, or what have you. We may call this a *translation* of the available data into the language of the theory. These translated data E', that follow logically from E and I, are fed into the model $\mathcal{T}'$ to yield the translated model $\mathcal{T}''$. Finally, this result is

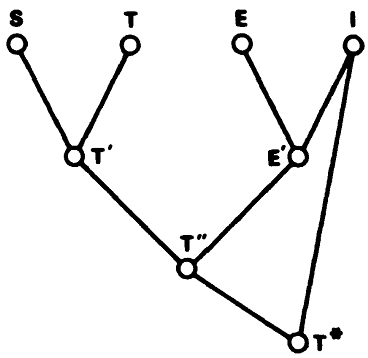


Fig. 11.4. The operationalization of a theory T. T and the specific assumptions S yield the model T' of the referent. The data E are translated into the language of the theory with the help of the indicator hypotheses I: the result is E' which, together with the model T', yield the testable consequences T''. Finally, these are translated back into the language of experimental science by means of the same indicator hypotheses I, to yield T^*, which is ready to be confronted to fresh data.

translated back into the language of experience by means of I. That is, $\mathcal{T}''$ is joined to I to entail $\mathcal{T}^*$, the operationalization of the theory, or rather of the (bound) model $\mathcal{T}'$. Hence not $\mathcal{T}$ itself but some consequences of $\mathcal{T}$ together with the subsidiary assumptions S, the data E, and the indicator hypotheses I, face whatever fresh experience may be relevant to $\mathcal{T}$. See Figure 11.4.

Our view of theory testing is at variance with the received view among philosophers of science. According to the latter a theory is a hierarchy of hypotheses, the highest placed of which would contain theoretical concepts without a directly observable counterpart. The lowest level hypotheses, instead, would be mere generalizations from observable facts and would therefore contain only observational concepts. And there would also be mid-level hypotheses containing concepts of both kinds. Moreover the relation between hypotheses of different levels would be strictly logical: the higher level hypotheses would imply the lower level ones. Therefore, theories would be tested by checking their lower level components, i.e. the logical consequences of their axioms. Such consequences would be in finite numbers because they are allegedly the inductive generalizations that prompted the construction of the theory.

This simple theory of theories is false, for it assumes that theories contain empirical or observational concepts in addition to theoretical ones, whereas in fact empirical concepts occur only in the experimental protocols, and even there combined with theoretical concepts. Also, it is not true that deduction can perform the feat of taking us from the theoretical or abstract to the empirical or concrete. What is true is that the axioms of a theory are seldom independently testable because no axiom contains all the predicates necessary to identify a thing, state, or event. The postulates of a factual theory must be combined with one another, as well as enriched with additional hypotheses, data and indicator hypotheses, to yield predictions concerning specific things in specific states. In short, we must inject some empirical content into a theory if we wish the theory to face experience: by itself it has none.

To conclude, indicator hypotheses are indispensable to test hypotheses and theories. But many indicators used in ordinary life cannot be employed in science or technology because they are ambiguous, imprecise, or both. Not all laughter indicates joy, and not every fluctuation in the Dow-Jones index represents a real change in the economy. The search for reliable indicators is a task both theoretical and empirical. The first because only a theory can tell us whether a given sign does in fact point to a certain unobservable rather than another, by disclosing the objective link between observable and unobservable. And only empirical checks can validate an operationalization procedure, for it involves hypotheses.

3. Data

3.1. *Measurement*

We need objective and precise information about the world, i.e. accurate data, for a number of purposes: to conduct the business of daily life, plan our activities, check our hypotheses and theories, and to explain and predict with the help of the latter. There are two main ways of learning data: producing them oneself or borrowing them from others. And there are three modes of generating data: by observation, measurement, and experiment. Observation, whether direct of with the help of instruments and theories, is deliberate and controlled perception, and it is the basic mode of data generation. We studied it in Ch. 4, Section 2. We shall now examine measurement, which may be characterized as quantitative observation, or the observation of quantitative properties. Experiment, or the observation (and possibly measurement) of changes under our partial control, will be tackled in the next subsection.

All organisms, even unicellular ones, have sensors capable of detecting and even gauging certain changes, in particular gradients and deviations from the optimal values of certain parameters, such as temperature, salinity, and acidity. But only man has invented and constructed, over the past few millennia, a variety of measuring instruments of varying precision. The differences between these and the natural "measuring" devices are as follows: (*a*) every measurement proper presupposes the conceptual operation of quantitation, or assignment of numbers to the degrees of a property—e.g. the conversion of separation into distance; (*b*) some measurements presuppose also the construction of indicator hypotheses—e.g. that the angular velocity of the anemometer measures the speed of wind; (*c*) measuring instruments are equipped with pointers, digital dials, or some other indicators, as well as with scales, that allow one to "read" them; (*d*) the measuring instruments are detachable, they can be exchanged for others, repaired, and improved on. We studied quantitation in Ch. 5, Section 3.2, and indicators in the last section. We shall not study measuring instruments, the subject of instrumentation (see, e.g. Stein, 1965); instead we shall discuss some of the methodological and philosophical issues raised by measurement.

The very first such problem is: *What is measurable*? The usual answer is that facts are measurable for, according to a well-known formula, measuring is "pinning numbers on facts" (S. S. Stevens). What kinds of facts? Surely not things but their properties and changes thereof. And which ones? Obviously only the ones that can be manifested objectively—e.g. generous actions but not (so far) generous feelings or intentions; and prices but not values (in use). Clearly, but what is a measurable property? The short answer is this: A property is *measurable in principle* iff it is (*a*) quantitative (i.e. the corresponding concept has been quantitated) and (*b*) either manifest (observable) or lawfully related (through an indicator hypothesis) to a manifest property. Whether a property is actually measurable at a given time depends on the state of the art, opportunity, and resources.

The two measurability conditions above, (*a*) and (*b*), can be summed up in the phrase 'theory dependence'. This dependence is denied by empiricism, according to which measurement data belong to the theory-free "empirical basis" of knowledge. Experimentalists know better: they know that the very design and operation of measurement instruments depends on theories. For example Rutherford, perhaps the greatest experimentalist in our century, organized as follows the first part of his classic treatise *Radio-Activity* (1904). He devoted the first chapter to a qualitative description of radioactive substances, the second to the *theory* of ionization of gases, and the third to measurement methods (some of which use that theory, and others different theories). This was necessary to explain why the discharge of charged bodies is a reliable indicator of the presence of a radioactive substance (the radiation of which ionizes the surrounding air). In the absence of some well-confirmed theory one may "pin numbers on facts" without understanding what one is measuring. IQ measurement is a case in point: it is not based on theory and it can be put to evil uses. (See Gould, 1981.)

If a theory states that objects of kind K have property P, then the experimenter may decide to devise a procedure for measuring P. (For example, unlike action at a distance theories, the electromagnetic field theories prompted experimentalists to produce and detect electromagnetic waves, and to measure their properties, such as wavelength and frequency.) If the experimentalist fails to detect the theoretically predicted property, then either the theory is at fault or his experimental design is inadequate. Should the theory be successful in a number of other cases, the latter inference is in order, and alternative experimental designs ought to be tried. If on the other hand a successful theory does not state that the objects of kind K have property P, then if the theory is true, P must be regarded as unmeasurable, perhaps because nonexistent.

For example, according to quantum mechanics the three components of the spin of a particle cannot all be measured at the same time. (This is how the noncommutativity among the spin components is usually interpreted. In my own interpretation unmeasurability derives from nonexistence: if one of the spin components is "well defined", i.e. has a sharp value, then the other two are not.) On the other hand, according to a classical or semiclassical theory, where the spin and other dynamical variables are "hidden" (i.e. scatter-free) variables, the particle would possess all three components at the same time, so they would be simultaneously measurable in principle. In short, measurability is critically dependent upon theory.

The central philosophical and methodological question about measurement is of course: What is it? The short answer is poetic rather than technical: Measurement is a synthesis of fact, reason, and action. Let us spell this out. (See Pfanzagl (1959) and Bunge (1967b) for different versions.) Consider a thing possessing a property P that comes in degrees, and call $\mathscr{P} = \langle P, \precsim \rangle$ the relational system formed by the set of such degrees together with its intrinsic factual order $\precsim$ (e.g. weaker than or as strong as). Conceptualize $\mathscr{P}$ by constructing a magnitude the values of which are, in the simplest case, real numbers together with units—e.g. 7 cm sec^{-1}—and call $\mathscr{R} = \langle R, \leq \rangle$ the set of such values together with its order $\leq$.

Quantitation (or numerical quantification) is a mapping of $\mathscr{P}$ into $\mathscr{R}$ preserving (representing) the objective order of $\mathscr{P}$.

Look now at the set M of marks on a meter designed to measure the given property via some indicator hypothesis—e.g. in such a manner that *weaker* corresponds to *to the left of*. Call $\mathscr{M} = \langle M, \lesssim \rangle$ the new factual relational system, where now $\lesssim$ is the conventional order of the marks. The measuring device involves a one to one correspondence between $\mathscr{P}$ and $\mathscr{M}$, and between $\mathscr{M}$ and some subset Q of the rational numbers (fractions) together with the same units as before. Some of the latter numbers will be represented on the dial of the meter. Call $\mathscr{Q} = \langle Q, \leq \rangle$ the conceptual relational system formed by such numbers (together with the appropriate units) in their natural order.

Altogether we have two *factual* relational systems, $\mathscr{P}$ and $\mathscr{M}$, and two *conceptual* ones, $\mathscr{R}$ and $\mathscr{Q}$: see Figure 11.5. These four systems are related as shown in the following diagram:

$$\begin{tikzcd}
\langle P, \lesssim \rangle \arrow[r, "\textit{Quantitation}"] \arrow[d, "\textit{Indication}"'] \arrow[dr, "\textit{Measurement}" near start] & \langle R, \leq \rangle \arrow[d, "\textit{Inclusion}"] \\
\langle M, \lesssim \rangle \arrow[r, "\textit{Scaling}"'] & \langle Q, \leq \rangle
\end{tikzcd}$$

This diagram answers our question, albeit in a somewhat crytic manner, *viz*: Measurement is the empirical operation that maps some of the objective degrees of a property into rational numbers (together with the appropriate units) via indication and scaling.

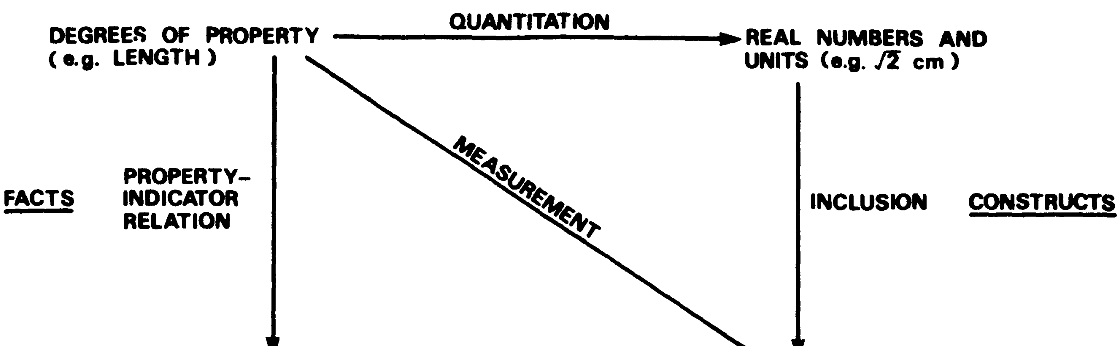


Fig. 11.5. Measurement as a one to one correspondence between the objective degrees of a property and instrument readings. Modified from Bunge (1967b, p. 221).

What is being measured depends not only on the object of measurement but also on the measurement technique, which includes instrument design. Two or more techniques may or may not measure the same magnitude, and it is only when they do that we must require that the result obtained with their help exhibit a strong correlation. The case of speed measurement should make this point clear. An observer riding a train has at least three different methods of measuring the train speed, each of which yields values of a different function (Lévy-Leblond, 1980). First there is the external procedure: the observer counts the mileposts placed regularly along the track, and reads the times shown by the clocks in the railway stations. He thus obtains a value of the *velocity* or rather its average. Secondly, there is the mixed procedure: the observer still counts milestones but measures time on his own watch. What he measures now is a different property, namely *celerity*. A third procedure is purely internal, namely to employ an accelerometer and plot its values against the proper time of his watch. This allows him to measure the train's *rapidity*. For slow motions the values of all three functions are the same, but for fast ones they are very different. To take a simpler example, each aspect of aphasia is studied by a different method even though they are all grouped under the same heading. Thus the sodium amytal procedure tests speech, the tachistoscopic one reading, and the dichotic measure listening (Herron *et al.*, 1979). More on measurement techniques, in particular instrument design, in Bunge (1967b, Ch. 13, Section 13.5).

The point of the above examples was to stress the platitude that different measurement techniques *may* measure different properties. Operationism has stretched this point claiming that different measurement procedures *always* "define" different concepts (Bridgman, 1927)—even if the theory does not make such distinction. (A classical example, still found in books, is the pseudodistinction between inertial and gravitational mass. For a criticism see Bunge (1967c).) But of course only theories can draw conceptual distinctions; and if a theory states that two allegedly different concepts are one, then different measurement methods can only give (different or equal) values of one and the same property.

Measurement, then, does not define concepts, let alone things. Instead, it helps us find out some of the properties of things and in this way identify them correctly or control them efficiently. For example, there are alternative procedures for identifying acids, the litmus paper technique being the most popular. Each such procedure can be summarized into an operational *criterion* (not definition), such as "Anything that turns litmus paper red is an acid". Unlike a definition, such a criterion tells us not what an acid *is* but how to *recognize* it. Therefore operational criteria cannot replace theory; moreover they can be justified only by theory. Of course, in turn theories must be justified with the help of empirical operations, in particular measurements. So, we have a feedback loop or virtuous circle: "There is no measurement without theory and there is no theory without measurement" (Stein, 1965, p. 5).

What holds for properties holds also, a fortiori, for their lawful relations: an effort should always be made to try and check the latter by alternative methods for measuring the same properties, even at the risk of incurring the disapproval of short-sighted administrators who may regard such checking operations as wasteful. A strict operationist might object that different methods bring about different relations among properties, i.e. different laws. This claim has actually been made with regard to the psychophysical law relating the sensation intensity to the stimulus intensity (Wasserman *et al.*, 1979). What may have happened in this case is that differences in method elicit differences in the internal states of the experimental subject, which states are ignored by the law in question. This is the rule rather than the exception. If method 1 allows one to check the functional relation "$y = f_1(x)$", and method 2 "awakens" a second independent variable z and establishes the relation "$y = f_2(x, z)$", then the experimenter is likely to rejoice for having discovered a more inclusive relation. And he will be even more satisfied if he succeeds in showing that, upon "freezing" z (or restricting z to varying within a narrow interval), f_2 comes close to f_1, which is then shown to be but a first approximation to f_2.

A related philosophical problem is whether all measurement creates its object or at least interferes with it and whether, when it does, such interference can be corrected. A measurement technique is said to be *obtrusive* if it changes in any way the state (and a fortiori the kind) of the object of measurement, and *unobtrusive* otherwise. *Example 1*: A current or voltage measurement is slightly obtrusive, for it involves dissipating a fraction of the energy in the meter. But this effect can be corrected for with help of a second measurement, so the net effect is negligible. *Example 2*: Measuring the age of a fossil using the carbon 14 dating technique is highly obtrusive, for it requires burning the object. But alternative dating techniques have been invented which are far less obtrusive. *Example 3*: All interviews and questionnaires used by social scientists and psychologists are somewhat obtrusive, for people change their behavior when they know that they are being observed. Think of questions such as "How often do you batter your children?", "Are you the kind of people who help their neighbor in need?", and "Do you sympathize with communism?"

There is no question but that, the deeper and more accurate a measurement technique, the more obtrusive it is likely to be. However, the history of instrumentation shows that, if we try hard, we are likely to invent means to minimize the effect of the observer's interference or at least to calculate its effect. For example, by lowering the temperature to near the absolute zero, thermal noise is eliminated, and magnetic fields are kept out of metals; and, by using semitransparent mirrors and hidden cameras, the experimental subjects cease to act in a self-conscious or in a hypocritical manner.

Still, there is one important case where it would seem that obtrusiveness is here to stay, namely in the measurement of properties of microthings. For example, if the position of an electron is measured with great accuracy, then—according to the Heisenberg inequalities—it is impossible to measure its velocity at the same time. However, these formulas can be interpreted in several ways (Bunge, 1977c). According to the popular interpretation, the measuring device disturbs the electron in an unpredictable way, imparting it an uncontrollable momentum: this is the *causal* intepretation, which ill accords with the stochastic nature of quantum mechanics. The Copenhagen interpretation avoids causality but introduces a subjectivity alien to the scientific approach: it states that the electron *has* no properties as long as these are not measured, and acquires them the way the experimenter wants.

The realist interpretation is that, in general, the electron *has* neither a sharp position nor a sharp momentum but only distributions of both, as well as the possibility of *acquiring* either a sharp position or a sharp momentum, but not both at the same time, under suitable environmental conditions. The electron position *becomes* measurable only if the electron is forced, by appropriate natural or artificial means, to adopt a state characterized by such property—which it will do only at the price of losing whatever sharp velocity value it may have had before. And if we decide instead to measure the electron velocity we must start by preparing the electron in a state characterized by a sharp velocity value rather than by a whole range of velocities. We cannot measure a property that the electron does not possess to begin with. There is nothing subjective about all this, for the conditions under which an electron gets its position (or velocity) sharpened may be natural or artificial. And, contrary to popular wisdom, the Heisenberg inequalities do not impose any limitations upon our knowledge: they only show that microthings are not particles. (But we should have known this ever since classical mechanics was shown inadequate to describe, explain and predict their behavior.) In short, we certainly alter the state of a microthing when measuring some of its properties, but we do not thereby create the thing at our whim and in violation of the laws of nature. When an experimenter undertakes to measure a property on an object he assumes the objective existence of some objects possessing certain properties and laws.

Still, it might be thought that, since natural things do not come with numbers indicating the values of their properties, the outcomes of measurement are wholly man-made. After all, by shifting the zero of a scale or adopting a different unit we may assign a thing any size or age. Likewise empty space is devoid of milestones, so why not think that it is metrically amorphous, hence that our assigning it metrical properties is purely conventional? The answers to these conventionalist innuendos are as follows. First, the pinning down of numbers on properties is basically no different from our constructing theories about things. Surely Maxwell's equations are not written on the electromagnetic waves that illumine this book, but those equations do fit them quite well. Second, we often handle, and in principle could always deal with, ratios—e.g. relative concentrations, densities, and the like—rather than with absolute quantities, and such ratios are unit-free, hence independent of any unit conventions. Thirdly, although the choice of scale (in particular zero and unit) is conventional, such choice, when adhered to consistently, renders intersubjective comparison and communication possible. Language too is largely conventional, yet such conventions are necessary for communication.

Units occur explicitly in the very formation of dimensional magnitudes such as permeability and productivity. (Cf. Ch. 9, Section 3.1.) All scientists are familiar with the rules for the formation of such concepts, and will reject as ill formed an expression such as 'The mass of body *b* equals number *c*', admitting instead expressions of the form 'The mass of body *b* equals *c* grams', or 'The mass of *b* in grams equals *c*'. (The classical mass concept is formalized as a function from pairs $\langle$body, mass unit$\rangle$ to positive numbers.) Units are defined in conceptual terms, preferably in terms of law statements. For example the newton, a force unit, is defined via Newton's second law of motion. (One newton is the force that causes an acceleration of one meter per second per second to a body possessing a mass of one kilogram.)

In addition to units we need standards for some of them, i.e. their materializations. Such standards are carefully described physical things or processes that can be easily replicated or reproduced as well as improved on. Thus the length standard used to be a meter long platinum bar under certain pressure and temperature conditions. As from 1983 the meter standard is the distance light travels in vacuum in a given fraction of a second. (The speed of light is taken as a base line equal to 299,792,458 m/sec.) Contrary to careless textbook affirmations, standards do not *define* units but *materialize* them. And, unlike units, which are arbitrary, standards are chosen for their stability and reproducibility. Entire governmental laboratories, such as the US Bureau of Standards, are devoted to keeping and replicating standards as well as to investigating ways to improve on them.

Needless to say, improvements in standards and, in general, in instrumentation, require imagination and motivation allied to familiarity with basic theory, electronics, and occasionally also computer programming. Improvements in instrumentation over the past few decades have often advanced at the rate of one order of magnitude (i.e. a factor of 10) every 5 years or so. Fewer and fewer institutions can afford to keep this pace. Hence, unless something drastic be done, experimental research will soon become the privilege of a few giant industrial laboratories—which are not interested in basic research anyway. There is then as much to bemoan as to celebrate in the swift progress of instrumentation.

In addition to favoring an excessive concentration of human and material resources in a few rich laboratories, quick progress in instrumentation may have the following negative side effects: (*a*) an accumulation of detail may make us forget the whole; (*b*) observation, measurement and computation, which are only means, may be turned into ends, and correspondingly scientists into technicians; (*c*) alternative approaches, requiring more modest laboratory equipment, tend to be ignored as the value of research projects tends to be gauged by their cost; (*d*) when too much reliance is placed on instruments, basic questions are hardly asked, and ambitious theories hardly sought. Austerity has its virtues: as Lord Rutherford used to say, "We've got no money, so we've got to think."

Suppose now that all the preliminary conceptual operations have been performed: we have constructed or adopted the magnitude (quantitative attribute) to be measured, and if necessary also its hypothetical (though reasonably well confirmed) relation to an accessible property (indicator).

Moreover we have designed our equipment with the help of a bunch of law statements, and have checked and calibrated it, so that it is ready to measure, to within an acceptable error, what we want to measure. Moreover, suppose we have obtained a suitable preparation—crystal, insect, or what have you—known or suspected to possess the property of interest. (This is no mean task, for we want a specimen or a sample representative of the original population, relatively easy to study, and we wish to minimize any destructive interference with it.) Only now are we ready to perform the measurements and record them, or perhaps to entrust them to a computer-assisted device. Quite frequently, whereas this last stage may be completed in a matter of days, the previous stage may consume months or even years of work sometimes involving dozens of coworkers and technicians.

We now have a set of raw data. They are not sacrosanct: they must be screened, for some of them may be far too weak signals and others plain errors. The remaining data are corrected in some way or other—e.g. reduced to standard atmospheric pressure and temperature. Even this data reduction may be insufficient: usually we want to extract from them some characteristics representing them as a whole, such as the average and the scatter around it (i.e. the standard deviation). The latter may be taken as a good measure of the measurement error. Such error is not necessarily a mistake on our part. Sometimes it exhibits the spontaneous random (e.g. thermal) changes of both object and apparatus. (It can be considerably decreased by working at very low temperatures.) At other times the statistical scatter exhibits the variety of the individuals composing the sample under investigation. (In sum, σ is an ambiguous indicator.) In any case, the correct summary of a run of measurement reads thus: The average value plus or minus one standard deviation.

The calculated average is usually called the "real" or "true" value of the property of interest although actually every one of the values that goes into the calculation is real. Ideally we should keep them all, or at least we should not throw away all of them unless we can make sure that the sample under study is rather homogeneous in the respect of interest. See Figure 11.6 for a case of hasty throwing away of valuable information. In any event, the results of measurements are "cooked" before being compared with those of other runs or with theoretical values. Mathematical statistics and the theory of errors of observation study such data "cooking" procedures and pose a number of genuine and interesting methodological problems that are still awaiting philosophical analysis. Suffice it to mention the ground, or

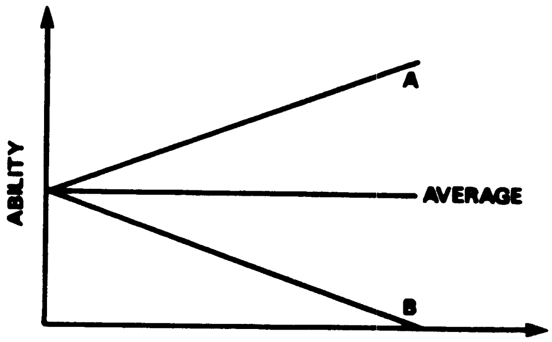


Fig. 11.6. A group of experimental animals is tested for some ability and its variation in the course of time. Those of kind *A* increased their ability while those of kind *B* decreased it. The average showed no change, so if it had been taken as a faithful representative of the group it would be uniformative. Adapted from R. M. Yerkes (1907) *The Dancing Mouse*, p. 273. *New York: Macmillan.*

lack of it, for adopting averages as the real or true values, and for discarding outlying values (i.e. measurement results departing from the average by more than three standard deviations).

We close this section with some remarks on two supposedly extremely general theories of measurement. One of them is what mathematical psychologists call *measurement theory* (e.g. Stevens, 1951; Suppes and Zinnes, 1963), and which others would call 'formation and analysis of scientific (in particular quantitative) concepts'. In fact this theory is unconcerned with the empirical operations everyone else calls 'measurements'—such as the actual determination of durations and electric charges—and deals instead with what it calls 'measurement scales', and the adult sciences call 'quantities' or 'magnitudes'. The theory contains basic mistakes that render it useless. One of them is that it ignores dimensional analysis and even the very notion of a dimension. Another is that it does not contain a proper account of units. (For algebraic theories of dimensions and units see Bunge (1971).) A third is that it ignores intensive quantities, such as density and *per capita* income, which are often primitive in scientific theory. A fourth is that is postulates that all extensive magnitudes (such as length) are additive, whereas in fact some of them, such as mass and entropy, are subadditive. (I.e. the mass of a system is somewhat smaller than the sum of the masses of its components.) The moral is plain: It is not advisable to try and build general theories of scientific concepts with disregard for the factual theories that house such concepts. (For details see Bunge (1973c).)

Another purportedly general theory of measurement is the *quantum* *theory of measurement* or, rather, the family of theories dealing with measurement in general on the basis of quantum mechanics. The aim of such theories is to analyze micro–macro interactions, in particular those between a microthing (e.g. an electron) and a measuring apparatus. Whereas some such theories are strictly physical or objective, others make room for the observer's mind (without however containing a single variable representing mental functions). The main trouble with all these theories is that they are nothing but academic exercises consisting in applying quantum mechanics to extremely idealized situations: they ignore that every actual measurement measures one or more *particular* properties on *specific* things with the help of *special* devices.

There is no such thing as the all-purpose measuring instrument imagined in those theories. Since every type of measuring instrument is quite specific, it requires its own theory, which is usually a classical one and occasionally a mixture of classical and quantum theories. (Think of the enormous difference between optical spectrographs and mass spectrographs, or between thermocouples and scintillation counters.) A general theory of measurement may explain some features of measurement, such as its irreversibility and amplification. But, because of its generality, it can issue no precise predictions and, not being predictive, it is empirically untestable. For this reason none of the many general quantum theories of measurement can ever be checked experimentally. So, the choice among them is a matter of indifference: one may choose either of them or be more parsimonious and ignore them all. On the other hand it is legitimate to try and unravel the main features of measurement and even to build a general theory of experiment involving no particular laws and therefore having no predictive power at all: See Appendix 5.

3.2. *Experiment*

Like measurement and unlike mere perception, experiment is active experience and one blended with reasoning. And, like measurement, experiment can find new facts or test hypotheses. However, experiment can find facts that measurement cannot unless it is part of experiment—namely facts that occur because of the deliberate alteration of the object of study. Indeed a fact finding experiment helps solve problems of the form "What happens to X if Y is done to X?"—e.g. what happens to a frog upon the unilateral destruction of its labyrinth, or to a community deprived of its leadership? Experiments of this kind are more difficult to perform than experiments aiming at testing hypotheses of the form "If Y is done to X, then Z results", for in this case the hypothesis itself directs us to look for effects of kind Z when controlling variable Y, whereas in the former case we must look for every possible difference that Y may make.

Unlike measurement of the unobtrusive kind, every experiment involves controlled changes in the object of study. Indeed an experiment may be defined as a controlled action performed by an animal on some other object, or on a part of itself, with the aim of solving some problem—cognitive or practical—concerning the object of experimentation, and such that the animal is able to receive, record, and analyze the reaction of the object to that action. What is typical of all experiment, in contrast to observation, is that the experimenter controls the object and its environment. In some cases this control is limited, i.e. restricted to the properties thought to be of interest. But in others the control is nearly total, for it includes the production of the thing or event of interest—as is the case with experiments with particle accelerators, with the artificial assembly of macromolecules such as genes, and with experimental social groups.

More precisely, an *experimental device* (or *set up*) is a concrete (material) system with at least three components: the object w of study (which can be replaced from one experiment to the next), an object supplying a stimulus (input) of some kind to w, and an object measuring the response (output) of w to that stimulus. An *experiment* on w consists in (*a*) subjecting w to a stimulus and observing its response; (*b*) observing the output of the same object w or one of the same kind when not subjected to the given stimulus, and (*c*) comparing the responses and finding out whether the difference between them is significant, i.e. due to the stimulus rather than attributable to chance or to idiosyncrasies of the object of study.

Needless to say, the choice of object and stimulus, as well as the design of the set up, in particular measuring device, are guided by hypotheses; and the final comparison (allowing one to draw "conclusions" about the effect of the stimulus) is controlled by statistics. The net outcome of an experiment is summarized in a set of statements, tables, or figures called the experimental *finding*. Sometimes nothing is found except that the experiment yields no evidence for a given hypothesis—e.g. that there are freely roaming spirits. Papers reporting on such "negative" results are regularly published in the scientific literature—seldom in the pseudoscientific (e.g. parapsychological or psychoanalytic) journals.

The advantage of experiment over observation and even measurement is best appreciated by studying some cases. *Example 1*: To find out whether information can be transferred from one animal to another by surgical transplant, Lindauer and his coworkers (Martin *et al.*, 1978) proceeded as follows. A collection of honeybees was divided into two groups, the experimental or donor bees and the recipient ones. The donor animals were fed between 10 a.m. and 12 a.m. for six days in a row, whereas the recipient bees were fed either continuously or a variable times, so that they did not learn any feeding pattern. The mushroom bodies (one on each side of the brain) of the donor bees were then transplanted into the ignorant bees. It was found that after 3 or 4 days the recipient bees behaved like the "learned" ones. The conclusion was that some information gets "engrammed" into the brain, and can be transferred by surgical transplant. *Example 2*: To test the hypothesis that speech in right-handed persons is a function of the left cerebral hemisphere alone, Kinsbourne (1971) performed the following experiment. Three right-handed men who had suffered left hemisphere strokes causing aphasia were given injections of amytal, first in the left carotid, then in the right one. The former had no effect, whereas the injections in the right carotid arrested completely what little speech the patients had been left with. The conclusion was that the right ("minor") hemisphere does participate in speech production. *Example3*: To check the hypothesis of the genetic origin of intelligence (held by A. R. Jensen, R. J. Herrenstein, H. J. Eysenck, and others), Schiff *et al.* (1978) compared the school performance of middle-class children with that of working-class children adopted by middle-class families. (Theirs was an *ex post facto* experiment: the foster children were unwitting experimental subjects.) They found that the children perform in school according to their homes, not their genetic origin: i.e. what counts most for learning are the cultural, not the biological parents.

We are so used to the idea that experiment is a necessary component of science and technology that we tend to forget how recent it is, and take it naively for granted that every widely advertised theory, artifact, or procedure, has passed suitable experimental checks. Experiment, as distinct from intuitive trial, was introduced only in the 17th century. Even in the next century physicists made few experiments; most of the empirical support for the new mechanics came from astronomy. Until the end of the 19th century mechanics was regarded as a branch of mathematics, not of physics. Biological experimentation was not conducted systematically until about 1850. Although medicine is supposed to be experimental, the first experiment to determine the efficiency of surgery to deal with breast cancer was not performed until 1981. The first laboratory of experimental psychology was inaugurated in 1879. Even though psychiatry is supposed to be a branch of medicine, the proposal in 1980 by a committee of the U.S. Congress to investigate experimentally the efficacy of psychotherapy raised an outcry among clinical psychologists and psychoanalysts. The very idea of a social experiment is foreign to most people. Very few have heard about the New Jersey negative income tax experiment (1968–1972). And many scientists still have hazy ideas about key features of experimentation. Thus some (e.g. Abraham and Marsden, 1978) regard computer simulation—a valuable auxiliary to actual experiment but no substitute for it—as experimentation. Others believe that any sample, regardless of size and mode of obtainment, will do; and still others commit the gambler's fallacy, believing, e.g. that the sequence HTHTHT of coin tossings is more probable than HHHHHH (Tverksy and Kahneman, 1971). All of which goes to show that insufficient attention is being paid to the methodology of experiment and, in particular, to its conceptual basis.

The design and interpretation of every experiment presuppose a number of hypotheses, most of which are rarely if ever noted, partly because few philosophers have ever analyzed any experiments. The presuppositions of experiment can be grouped into *generic* (shared by all experiments) and *specific* (characteristic of every type of experiment). See Figure 11.7. The generic presuppositions are in turn of two kinds: philosophical and

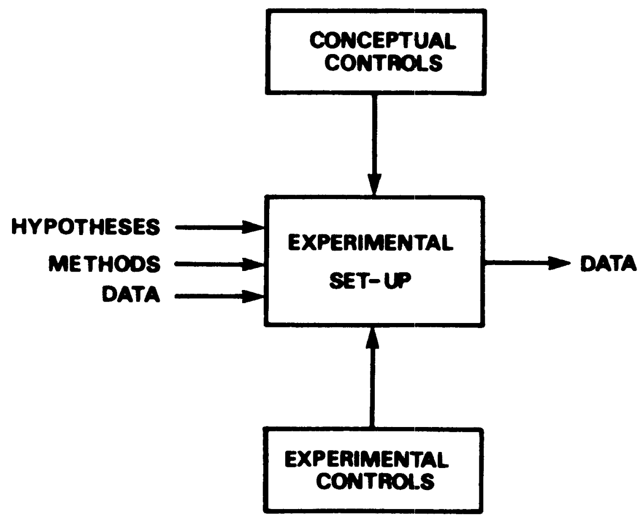


Fig. 11.7. A well-designed experiment has conceptual controls (in particular statistical ones) as well as experimental ones (e.g. of temperature and inflow of air). And in the design and interpretation of the experiment not only data and methods but also hypotheses (philosophical, statistical and scientific) take part.

statistical; and the specific ones consist in particular hypotheses or theories about the nature and behavior of the objects of experiment or the experimental means. Let us examine them quickly.

The main *philosophical* presuppositions of all experiment are these:

(*a*) *Reality*: The members of the experimental and control group, as well as the measuring instruments, exist really although some of the hypothesized objects may be imaginary. (If all the things involved in an experiment were figments of our imagination, then imaginary experiments would suffice.)

(*b*) *Lawfulness*: All the objects involved in the experiment behave lawfully. (There would be no point in performing experiments if nature or society were to give significantly different "answers" every time we pose them the same "question".)

(*c*) *Causality*: All the things involved in the experiment satisfy some form of the causal principle, however weak, e.g. "Every event is the effect (with some probability) of some other event". (Otherwise no deliberate production of an effect and no effective control of variables would be possible.)

(*d*) *Randomness*: All the variables involved in the experiment are subject to some random fluctuation, both intrinsic and due to external perturbations. (Otherwise we would not be able to explain the statistical scatter of results.)

(*e*) *Insulation*: Objects other than the object of the experiment, the experimenter, and his experimental means, can be neutralized or at least monitored for the duration of the experiment. (Otherwise no significant changes could be attributed exclusively to changes in the control variables.)

(*f*) *Disturbances or artifacts*: It is always possible to correct to some extent, either empirically or theoretically, for the "artifacts", disturbances or contaminations caused by the experimental procedures. These are not the deliberate alterations of the object but the unwanted distortions of it or of its image—e.g. the colors and diffuse rings produced by optical lenses, the solvents that change the reactivity of reactants, and the stains that impregnate living tissues with metals. (If such partial corrections were impossible we could not legitimately claim that the thing for us—as it appears to us—resembles closely the thing in itself—such as it is when not subjected to experiment.)

(*g*) *No psi*: It is always possible to design experiments in a manner such that the experimenter's mental processes exert no direct influence on the outcome of the experiment. I.e. the experimenter can be shielded or uncoupled from the experimental set up, so that his bodily and in particular brain processes do not alter the experimental results. Servomechanisms are particularly useful to this end. (Otherwise the outcome of the experiment could be produced at the experimenter's whim, and the experimenter would be testing nothing but his own mental, e.g. psychokinetic, abilities.)

(*h*) *Explicability*: It is always possible to justify (explain), at least in outline, how the experimental set up works, i.e. what it does. I.e. it is possible to form a conceptual model of the experimental device using well confirmed hypotheses and data. (Otherwise we would be unable to draw any conclusions.)

In addition to the foregoing philosophical principles, the following *statistical* principles and rules are supposed to be observed in every experiment, particularly in the biological and social sciences:

(*a*) The objects of experiment should be drawn from as *homogeneous* a population as possible. To this end the original population, if extremely heterogeneous, should be partitioned into equivalence classes. (Otherwise no general patterns may be recognized and correspondingly no generalizations may be conjectured.)

(*b*) When the total population is practically inaccessible (e.g. for being too large), a sample is to be drawn from it in such a way that it be *representative* of the former. Mathematical statistics has come up with techniques for maximizing representativity.

(*c*) The sample to be experimented with should be divided into two roughly equal parts: the *experimental* group (the members of which will be subjected to the given stimuli) and the *control*. The assignment to each group should be done in a random manner, in order to minimize bias in the choice of experimental subjects as well as placebo effects and last, but not least, in order to be able to apply probability ideas. (No randomness, no probability.)

(*d*) Experiments with human subjects should be *double blind* in order to avoid the placebo effect: i.e. neither the subject nor the experimenter should know who gets what. This information should be reserved to technicians or computers and utilized only once all the results of the experiment are in.

(*e*) The experimental design should be *checked* for possible systematic errors (or errors in design), and the accidental (random) errors should be calculated and noted.

(*f*) The *significance* in the differences of behavior between the experimental and the control group should be evaluated with the help of some significance test (e.g. chi-square).

(*g*) The experiment should be evaluated, and if possible repeated, by *independent* groups—not to forestall deception but to correct possible systematic errors.

This incomplete list should suffice to show that, contrary to widespread opinion, statistics should not enter only at the very end, to "process the data", but should be involved throughout the experiment, from design to evaluation. The regulative function of statistics is particularly apparent when the data arrive sequentially rather than in whole batches. In the first case the experimenter can stop gathering or producing data but must do so according to definite rules, for otherwise he may commit the fallacy of optimal stopping, or terminating the experiment as soon as his favorite hypothesis has been confirmed—a habitual tactics of parapsychologists. Such "stopping rules", or prescriptions for deciding when to stop gathering data, are fashioned by sequential statistical analysis.

Some of the above-mentioned statistical safeguards can be ignored in physics and chemistry, which usually work with extremely homogeneous populations (e.g. rather pure chemicals). But the biological and social sciences, which deal with complex systems characterized by an enormous individual variability, cannot ignore them. Yet, although those principles have been taught in standard courses over the past half century, they are still often ignored. *Example 1*: Until very recently studies of mental development and of neurophysiological "correlates" of learning were conducted without adequate controls and sometimes with no controls at all (Denenberg, 1979; Thompson *et al.*, 1972). *Example 2*: Experiments in psychoanalysis are conspicuous by their absence, and in parapsychology notorious for inadequate controls and for a fanciful use of probabilistic and statistical ideas. (Cf. Hansel, 1980; Alcock, 1981.) *Example 3*: The most famous of all experiments in human engineering since the days of Taylor was the Hawthorn experiment (1927–1933) on the effect of human relations on productivity. Its analysis half a century later (Franke and Kaul, 1978) has revealed that it had not been an experiment proper because no control groups had been involved, so that the inferences had been of the *post hoc propter hoc* type. Second, the data were not subject to statistical analysis. Third, between the start and the termination of the "experiment" the Great Depression set in, introducing a major variable which the investigators were not in a position to control, namely the fear of unemployment and the corresponding toughening of management. Fourth, fatigue was reduced by working fewer hours a week.

Finally we come to the *scientific* hypotheses underlying the design, performance, and interpretation of experiments. These hypotheses are of two kinds: whereas some of them refer to the means (in particular the measuring instruments), others concern the objects of experiment and their interaction with the former. We examined the hypotheses of the first type in the preceding section; let us now look into those of the second type. At first sight no such hypotheses are involved in experiments attempting to answer questions of the form "What happens if X is done to Y?" A closer examination reveals that, far from choosing the stimulus X at random, the experimenter is guided by both his information, however scanty, about X and Y and some hunches, however vague, on the possible effects of X on Y. Otherwise he would succeed only by sheer luck, for experimenters do not have an infinite time and things are not susceptible to all possible stimuli. (Try to play Beethoven to a fly. Rock music, on the other hand, might have some damaging effect.)

Every well-designed experiment is inspired or misguided by some specific hypothesis or other. For example, an experiment inspired in stimulus–response psychology controls only behavioral variables. Hence it can draw valid conclusions only if the experimenter has "frozen" (kept constant) all the relevant internal variables, such as drives and motivations. (For example, it has been reported that rats prefer complicated roads leading to food sources, to straight ones. Maybe so, but any experiment to test such curiosity or adventurousness is worthless unless it has been ascertained that both the curious and the greedy rats are equally hungry.) On the other hand an experiment in physiological psychology will control physiological as well as behavioral variables. For example, to find out whether neural system X is "involved" in (or "mediates") behavior Y, block or stimulate the activity of X, and watch for behavior of kind Y. (*Dual*: Modify behavior from Y to Z, e.g. by placing constraints on motion or on perception, and watch for changes in the activity of neural system X.) Another example: it is usually assumed that it is possible to separate hereditary from environmental factors in the study of intelligence by using the twin method. (Each of the monozygotic twins is placed in a different environment, and their performance is compared.) This technique presupposes that one-egg twins are strictly identical, which in turn follows from the assumption that heredity is locked in the cellular nucleus. But Darlington (1970) refuted this latter hypothesis by finding extracellular DNA. Therefore one-egg twins, though possessing identical nuclear DNA, might differ in their non-nuclear DNA. If this difference proved to be significant, then the "identical" twins would not be strictly identical and the twin method would not be fully reliable. But this is still *sub-judice*.

Regardless of the reliability of the twin technique, the methodologically important point is that every experimental technique presupposes some hypotheses and so is just as fallible as these. (For the opposite view see, e.g. Bachrach (1962) or Rapp (1980).) In other words, experimental results are not more reliable or certain than the hypotheses underlying them, and they are not atheoretical anyway. (To be sure, the routine operations that can be performed by technicians once the original experiment has been designed do not require any knowledge of the hypotheses that went into the original experimental design. But this is beside the point: we are concerned with original research.)

Empiricism has extolled the value of the so-called *empirical basis* and its theory-free nature. There *is* no such pure theory-free empirical basis: every worthwhile experiment presupposes a number of hypotheses and, if conclusive, it confirms or undermines some hypotheses or calls for new ones. An experiment calls for a new hypothesis or theory if the existing ideas do not account for its outcome, i.e. do not explain satisfactorily how the experiment works or do not predict well its results. But of course no set of experimental results, no matter how numerous, precise and accurate, will suggest unambiguously a theory. For example, "Thousands of learning experiments with rats have not yielded a set of principles from which the performance of rats can be derived satisfactorily, although a great deal of progress has been made in the 75 years or so since the first rat was introduced into the first maze" (Bitterman, 1975).

Experiment is so essential to science and technology that it is sometimes believed to be all-important, and consequently laboratory work is regarded as a goal in itself. This is a one-sided view because it ignores the many hypotheses involved in every experiment, as well as the impact that experimental findings may have on existing hypotheses or on the construction of new ones. Like other methodological errors, this one has serious negative effects on inquiry. Here are some: (*a*) concentrating interest on variables, details, or components that are easily controllable or measurable; (*b*) focusing on instruments and techniques (e.g. electron microscopy and electrophysiology) to the detriment of the object of study; (*c*) neglecting naturalistic situations and disregarding the environment (particularly in genetics and psychology) as well as experimental "artifacts"; (*d*) underrating theory—which suggests, guides, explains, and often re-places experiment; (*e*) adopting an anthropomorphic view according to which no event happens, or at least matters, unless it is elicited and measured by some experimentalist (as is the case with the Copenhagen interpretation of quantum mechanics). Such negative effects of the exclusive concentration on laboratory work are visible in all the sciences, particularly the less developed natural sciences. The only effective antidote is a strong dose of a mixture of (good) theory and (good) philosophy.

Having warned against exaggerating the importance of experiment, let us now warn against any attempt at curtailing its scope. To be sure, some areas of inquiry are still closed to the experimental method—e.g. astrophysics and ethics. However, the experimental method has made decisive inroads in a number of areas that used to be regarded as impregnable to it. *Example 1*: Experimental astronomy started with the first Sputnik (1957). *Example 2*: Experimental geology, conducted on small scale models, has been in existence for decades. *Example 3*: Experimental palaeontology is on the way. Thus, by working on models of the dermal bony plates along the back and tail of *Stegosaurus*, it has confirmed the hypothesis that they served a thermoregulatory function (Farlow *et al.*, 1976). *Example 4*: Experimental archeology, more than a century old, has succeeded in explaining the use and efficiency of a number of artifacts and techniques, from food storage to earthwork to stone working and pottery manufacture (e.g. Coles, 1973). *Example 5*: Experimental sociology, tried sporadically and on a small scale several times during this century, was launched on a large scale in 1968 with the New Jersey income maintenance experiment. Moral: Never prophesy that a given field of factual inquiry will remain forever beyond experimental control.

4. Concluding remarks

Whereas self-evidence is the gate to self-deception, hard evidence is the seal of adequacy—be this truth or efficiency. Evidence for or against data, hypotheses or theories, techniques, rules or plans, can be conceptual (in particular theoretical) or empirical. In other words, an idea is supported or undermined by other ideas or by empirical tests, from observation and measurement to experiment and action. And the evidence may be intradisciplinary or interdisciplinary (i.e. coming from another field of inquiry).

All empirical tests involve hypotheses and often call for further hypotheses or even entire theories. The hypotheses involved in the design and interpretation of tests are not always explicit: sometimes they are just biases. (Thus different field linguists studying the same population are bound to detect some different phonemes, because each expert expects certain sounds—which he duly records—while he does not expect others—which he may fail to record.) In science and technology there is no purely empirical basis on which to build theories, and no theories that could not possibly be affected, one way or another, by empirical tests. This is not to devaluate data but to remind ourselves that, unlike the anecdotal data of ordinary life, those of science and technology are "contaminated" by theory. Nor is it to revile speculation but to remind ourselves that all speculation must eventually be confronted with hard facts—for facts are, after all, the subject matter of science and technology.

Not all data are equally valid. In fact it has become customary, particularly in the social sciences, to class them into *hard*, *semihard*, and *soft*. Hard data are those provided by measurement or experiment on things of known composition or environment. In general they are repeatable (to within experimental error). Semihard data result from the observation of things of poorly known composition and environment. In general they are nonrepeatable. Statistical data, particularly in the social sciences, are of this kind. Finally, the soft data are occasional or anecdotal: they come from casual observation or hearsay, and they are nonrepeatable. In general, the more and better theory has gone into the production of a datum, the harder it is.

The above classification concerns the source or mode of production of data, not their intrinsic worth (truth and usefulness). In fact a hard datum can be false, or at least imprecise, or it can be useless. (This does not mean that there are no hard *facts*, but instead that our knowledge of them, even when obtained the hard way, may turn out to be false.) And although anecdotal data constitute no evidence, they are occasionally more valuable than many a trivial measurement or experimental result. For example, the observation of caged and free-roaming apes has triggered or supported much important scientific work in antropology, psychology and sociology. The quality of the data source (hard, semihard or soft) is important but not more so than the output: we judge the reliability of data sources by their performance, not conversely. (For the contrary thesis, that the plausibility of propositions depends upon the reliability of their sources, see Rescher (1976).)

The best data are those that constitute evidence for or against some hypothesis or theory and those which are precise, accurate, and varied. We will not insist on the importance of the theoretical relevance of data: suffice it to note that nobody collects data for their own sake, but always to some purpose, such as trying to discern a pattern or to check a hunch. As for repeatability, essential to minimize error, it is not always obtainable, particularly in the historical sciences. Variety, on the other hand, is in principle always obtainable because every property is lawfully related to some other properties. And it is desirable to minimize error. Thus palaeobiologists do not rely exclusively on fossils and the theory of evolution: they also make use of geological data and of information about living organisms.

The standards or criteria for evidence have become increasingly exacting in all fields of inquiry, from science and technology to the humanities and the law, since the beginning of the Modern Era. Thus hearsay and circumstantial evidence are no longer accepted, either in the laboratory or in the court of law, except as hints pointing to further search for solid evidence. Nor, of course, is a mere statement of belief or authority accepted: instead, evidence for truth or efficiency, as the case may be, is required. This dramatic progress in the standards of accuracy can be explained not only as an aspect of intellectual advancement but also by external motives. As science and technology increase in cultural and economic value, it becomes increasingly important to know to what extent they work and in which respects they are still deficient. And as the struggle for economic, political and cultural rights proceeds, the law is required to observe them more faithfully, to which end it must substitute objective evidence for denunciation and confession.

The data generated by observation, measurement or experiment may discharge several functions. They may pose problems and illustrate hypotheses. They may call for, and partially suggest, new hypotheses or theories. They may "activate" known hypotheses or theories by filling some blanks in them (i.e. assigning values to constants or parameters). Or the data may be used to assess the worth of our ideas: the truth of hypotheses and theories, the precision and accuracy of techniques and instruments, or the efficiency of rules, proposals, and artifacts. Let us study next the latter operation, namely evaluation.

CHAPTER 12

EVALUATING

All cognitive activities are motivated. We seek to find, think to solve problems, plan to attain goals, talk and listen to communicate, and so on. And we evaluate all the conscious activities, whether purely cognitive or using some knowledge. We evaluate problems and their solutions, propositions and proposals, theories and designs, methods and artifacts. We evaluate some of them for truth and others for usefulness: these are the supreme values in matters of cognition. Any other values, such as simplicity and beauty, are subsidiary with regard to knowledge. To be sure, a false hypothesis and a useless machine may be valuable in some regard—e.g. heuristically in the former case and commercially in the latter—but ultimately they are to be discarded.

Those two main values, truth and usefulness, are quite distinct: so much so that certain false propositions are valuable (for evil purposes) whereas some true ones have no practical use. This does not entail that truth and usefulness are always unrelated. Modern technology involves both, for it employs theories that have passed truth tests, and does so in order to guide the production or maintenance of (supposedly) useful things. What the distinction does entail is that truth is no substitute for usefulness or conversely, whence the search for truth is not the same as the search for usefulness. In particular, science is not identical with technology. In a way the latter is more demanding than the former, for it uses and produces both truth and usefulness.

Although truth and usefulness are the supreme values of cognition, they are quite elusive: they are difficult to define and recognize. To be sure, there are precise definitions of logical and mathematical truth, but they are inapplicable to factual propositions. And anyone can draw a list of useful items, but few can say what value is. We shall outline a program for a correspondence theory of truth, and another for a theory of value (Section 1.)

Being seldom manifest, truth and usefulness are often hard to pinpoint. As in the case of other unobservables, we need indicators. I submit that there are both empirical and conceptual indicators of truth and usefulness. The foremost empirical indicator of truth is confirmation, and that of usefulness efficiency (Section 2). And the main conceptual indicator of either is compatibility with our background knowledge (Section 3). We come up then with a battery of truth indicators and another of usefulness indicators. Each battery allows one to assign, at least temporarily, degrees of truth or of usefulness, as the case may be. I wrote 'assign' not 'disclose' or 'discover', because—and this is a central thesis of this chapter—neither truth nor usefulness are intrinsic and eternal properties: they are contextual and changeable. As von Uexküll (1928, p. 1) said, scientific truth is today's error, and the progress of science is a process of moving from gross to fine error. Likewise we may say that usefulness is today's waste, and the progress of technology is or ought to be one of decreasing waste.

1. Values

1.1. *Truth*

We all value adequate knowledge: adequate propositions (in particular data and hypotheses) and adequate proposals (in particular rules and plans). Our notion of adequacy embraces two concepts, those of truth and usefulness. An adequate proposition is said to be true (to a considerable extent), whereas an adequate proposal is said to be useful (to a considerable extent). Thus the proposition that crème caramel is nourishing, is true, whereas a recipe for baking a good crème caramel is useful. In this section we shall study truth, leaving usefulness for the next.

Let us begin by recalling briefly some of the ideas on truth discussed at length in Vol. 2, Ch. 8; after that we shall have a new look at the problem. To begin with we stipulate that truth (total or partial) is a property of propositions that have passed certain tests. This statement raises immediately two questions: Why should propositions, rather than sentences, be the truth bearers, and why should propositions lack a truth value unless they are first tested? The answer to the fist question is this. Firstly, not all sentences are meaningful, hence not all of them could be assigned a truth value. For example, 'This book squeezes my anvil', though grammatically correct, has no cognitive content and therefore does not designate a proposition. Second, the language in which a proposition is expressed is important only for purposes of communication. Thus the sentences 'Russell was a famous thinker' and 'Russel fut un penseur célèbre', as well as their renderings in American Sign Language, express the same (true) proposition.

As for our view that propositions are not born with truth values but are assigned truth values on the strength of tests, it is at variance with tradition. According to this tradition, which probably originated with Plato, truth (or falsity) is possessed by a proposition whether anyone knows it or not. This view is still found, though usually unexamined, in much of contemporary epistemology. In particular epistemic logicians usually equate "s knows p" with "s knows that p is true". Yet there are two different concepts here: subject s may know or understand p without having the slightest idea about the truth value of p. For example, a theorist who conjectures or infers a hypothesis p will not know whether p is factually true until p has been subjected to empirical tests—and even then some uncertainty may remain. In short, a proposition p must be distinguished from the metastatement "p is true (or false)", which in turn can only be a result of an investigation of p.

We adopt then the constructivist thesis that propositions, and in general constructs, exist (formally) if, and only if, they are knowable. In particular, truth values are acquired or lost as a result of tests for truth. This does not prevent us from *feigning*, for the purposes of logical or mathematical work, that there are constructs, in particular propositions, detached from the material world and, in particular, from thinking brains (cf. Bunge, 1981). We adopt this as a useful fiction and as tantamount to saying that, for certain purposes—such as logical analysis and mathematical calculation—it does not matter who thought up the propositions concerned or under what circumstances he did so. Pretending that propositions exist in this manner is radically different from claiming that they exist in and by themselves, the way Plato, Bolzano, Hegel, and Popper held. Our view is then compatible with the naturalist ontology formulated in Vols. 3 and 4.

Our constructivist thesis is superficially similar to that of operationism, according to which physical objects have no properties as long as these are not measured. The analogy is superficial, for physical objects happen to exist by themselves, and every existent has some properties. On the other hand constructs must be constructed by someone in order to exist. It is we who, by fashioning them—i.e. thinking them up—endow them with properties, in particular truth values—or abstain from doing so. Table 12.1 summarizes our view so far.

The kind of test that may determine the truth value of a proposition depends on the kind of proposition. Thus the (formal) truth of the Pythagorean theorem is established by deduction within Euclidean geometry, whereas its factual adequacy is investigated by measure-

TABLE 12.1
Truth not inborn but acquired or lost

Methodological viewpoint	*Semantical viewpoint*	*Pragmatic viewpoint*
p has not yet been investigated, or it has been studied and no conclusive results have been obtained.	p has no truth value.	Suspend judgment about the worth of p: neither believe nor disbelieve p.
p has been investigated and found coherent with both background knowledge and new data.	p is true (to some extent).	Accept p *pro tempore*.
p has been investigated and found inconsistent with background knowledge and new data.	p is false (to some extent).	Reject p *pro tempore*.

ment—which shows it to be approximately true of physical space. There are then, as Leibniz argued, truths (and falsities) *of reason*, and truths (and falsities) *of fact*. We count the logical, mathematical and semantical among the former, and the ordinary, scientific, technological, ontological and epistemological among the latter. A truth of reason is recognized by its coherence with some body of knowledge, i.e. by purely conceptual operations. On the other hand a truth of fact is recognized not only by its matching a body of (factual) knowledge but also by its passing certain empirical tests showing its adequacy to concrete or material reality. See Figure 12.1.

Because formal and factual truth are predicated of propositions of radically different kinds, and on the strength of operations of diverse types, each of them requires its own theory. That is, we need a *coherence* theory to elucidate the notion of formal truth, and a *correspondence* theory to explicate that of factual truth. The claim that a single theory of truth can perform both tasks is consistent only with either an idealist ontology or a radical empiricist epistemology. In the former case reality, or the actual world, is just one more model of some abstract structure. In the latter case all knowledge, whether formal or factual, is acquired only through sense experience: the task of reason would be only that of ordering or systematizing such knowledge, never generating or validating it. Since we accept neither an idealist ontology (cf. Vols. 3 and 4) nor an empiricist

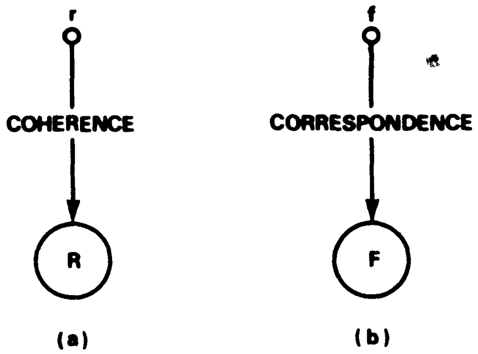


Fig. 12.1. Two kinds of truth. (*a*) Formal truth: proposition r coheres with body R of propositions. (*b*) Factual truth: proposition f corresponds to facts F. Since R and F are disjoint, the relations of coherence and correspondence are different.

epistemology (as is clear from this volume), we reject the monistic view of truth, which conflates formal and factual truth, and we undertake to find one theory for each kind of truth (or falsity).

Now, the coherence theory of truth is available: it is part of model theory, or the semantics of logic and mathematics, and it is usually included in logic. On the other hand the correspondence theory of truth is so far little more than a program. We shall not be concerned here with formal truth and its theory, i.e. model theory, of which more in Vol. 2, Ch. 8, and Vol. 6. We shall grapple instead with the idea of truth as correspondence with fact. Our investigation will proceed in two stages. In the first we shall study what the correspondence consists in; in the second we shall investigate the actual process of truth value assignments, or the problem of truth criteria.

Our first problem, then, is that of clarifying the sense of 'correspondence' or 'adequacy' in the scholastic formula *Veritas est adaequatio rei et intellectus* (or *intellectus ad rem* in a more realistic version). How can a proposition, which is a construct, be adequate to (or match or fit in with) a fact in the world of things? One answer is that there is an isomorphism between a proposition and the piece of reality it refers to: this idea has been upheld by naive realists and Gestalt psychologists. However, 'isomorphism' is here as little a technical term as 'adequacy' in the scholastic formula. Indeed, isomorphism can exist only between sets, and neither a proposition nor the concrete thing it refers to are sets. This difficulty is seemingly solved by postulating that sets of propositions, such as theories, can correspond to collections of facts. But then truth would be a collective or emergent property possessed by a theory, not by its constituents. Besides, sets of facts have rarely a definite structure. Systems do have definite structures, but not every thing is a system. Besides, it is easy to exhibit systems that are not isomorphic with their theories—e.g. a system composed of two particles gravitating around each other is not isomorphic to any theoretical model of it, which model contains infinitely many propositions.

We seem to be up against a Gordian knot, so let us cut it. Let us not pretend any longer that the truth relation holds between propositions and facts. Let us admit instead that propositions can match other propositions, and facts other facts. And let us assume that the correspondence we are seeking is one between mental facts of a certain kind and further facts, whether mental or not. Further, in line with psychobiology we identify mental facts with brain processes occurring in plastic neural systems (Ch. 1). This strategy will allow us to characterize (though not define in general terms) the concept of true partial knowledge of fact; once in possession of this notion we shall proceed to define the truth of a proposition.

Consider a thing a internal or external to an animal b endowed with a brain capable of learning. Call e an event (change) in thing a, and e^* the corresponding perceptual or conceptual representation of e in the brain of b. Then we say that b has gained *true partial knowledge of fact* e if, and only if, e^* is identical with the perception or conception of e as a change in thing a (rather than as a nonchange, or as a change in some other thing). The true partial knowledge that b has acquired of event e is the neural event e^*; and the *correspondence* involved in it is the relation between the events e and e^*.

Consider next the case where the object of knowledge is a binary relation between events, such as the temporal succession of two flashes of light. Let now e_1 and e_2 denote changes occurring in a thing a internal or external to an animal b capable of learning, and call e_1^* and e_2^* the perceptions or conceptions that b forms of e_1 and e_2 respectively. Further, let e_1 be R-related to e_2, i.e. Re_1e_2. We stipulate that b has *true partial knowledge of the fact that* Re_1e_2 if, and only if, (*a*) b recognizes e_1 and e_2 as changes in thing a (rather than either as nonchanges or as changes in something else), and (*b*) b perceives or conceives of e_1 and e_2 as being R-related, i.e. if $e_1{}^*$ and e_2^* are R-related. The true partial knowledge that b has gained of Re_1e_2 is $Re_1^*e_2^*$, and the correspondence involved is the relation between the event pairs $\langle e_1, e_2\rangle$ and $\langle e_1^*, e_2^*\rangle$. In general, the brain correlate of Re_1e_2 will be $R^*e_1^*e_2^*$, where R^* is similar to but not identical with R.

Consideration of the above two cases, of the single event and the pair of events, suffices for our present purposes. Let us now move from thoughts (brain processes) to propositions (constructs). Recall that we have defined a construct as an equivalence class of thoughts (Ch. 1, Section 2.1.) Consider the class of thoughts e^* elicited in animals of a given species by event e, and form the subset of e^*s that happen to constitute true (though partial) knowledge of e. Call this class $[e^*]$. No two members of $[e^*]$ may be identical—for they are thoughts of a given animal at different times or thoughts of different animals, and in either case they differ in some respect or other. However, they are all *equivalent* in that every one of them constitutes partial true knowledge of e; i.e. for every member e^* of $[e^*]$, e^* happens if, and only if, e is the case. We identify the proposition $p = \ulcorner e$ is the case$\urcorner$ with that equivalence class of thoughts, i.e. we set $p = [e^*]$. And we stipulate that *p is true if and only if e happens.* Likewise in the case of pairs of R-related events discussed above we form the equivalence class of thoughts $[\langle e_1^*, e_2^*\rangle]$ and identify it with the proposition $p = \ulcorner Re_1e_2\urcorner$, stipulating that *$Re_1e_2$ is true just in case $R^*e_1^*e_2^*$ is the case.* In short, the correspondence relation that holds between a mental fact and some other (mental or nonmental) fact carries over to propositions in relation with facts. In other words, we have analyzed the fact-proposition correspondence into the relations shown in the following diagram:

$$\begin{tikzcd}
\textit{Facts } E \arrow[r] \arrow[dr] & \textit{Thoughts } E^* \arrow[d] \\
& \textit{Equivalence class } E^*/\sim \textit{ of thoughts (proposition)}
\end{tikzcd}$$

In sum, we have identified factual truth with adequate knowledge of fact: we say that the proposition "e is the case" is true if, and only if, some animal has adequate knowledge of e. On this view truth and falsity are *primarily* properties of perceptions and conceptions, and only *secondarily* (vicariously) properties of those equivalence classes of thoughts we call 'propositions'. Hence a faithful perception or representation of a human figure can be said to be true, whereas a hallucination can be said to be a false perception. Moreover we identify the organ of truth and falsity as the plastic brain. It is a problem for neuropsychological research to try and localize, within the human brain, the neural systems that attribute truth and falsity.

Note that we have dealt only with simple (atomic) propositions, not with complex (molecular) ones such as disjunctions and generalizations. It might be thought that the latter can always be built out of simple ones, so that the truth values of the former can be evaluated on the basis of the truth values of the simple propositions together with a set of semantical truth conditions. This belief is false, as shown by any of the differential equations occurring in theoretical science. Thus a basic equation of Maxwell's electromagnetic theory reads: "For every electromagnetic field and the current that generates it, and every point in spacetime, the d'Alembertian of the vector potential of the field is proportional to the current density". This formula cannot be construed as an infinite conjunction of simple statements, one for each field and its corresponding source, and for each point of spacetime. Indeed the points of spacetime constitute a continuum, and logical conjunction is not defined for a nondenumerable set of propositions. The semantic truth conditions fail us here and we must resort to entirely different truth conditions, supplied by experimental physics and bearing on the testable consequences of the axiom in question.

Note also that we have dealt only with individual propositions not with hypothetico-deductive systems. One reason for this restriction is that an entire theory is unthinkable for it contains infinitely many propositions. We can think only of a few statements (postulates, definitions or theorems) of any given theory. When we think of a theory as a whole, in point of fact we select a handful of typical formulas of it. Therefore attributing truth or falsity to a theory is making an inductive leap on the basis of some evidence. (More in Section 2.2.)

It will be noted also that in the above discussion we spoke of *partial* knowledge of fact. Equivalently: factual propositions represent only some (not all) features of things, or they are not totally true but rather half true. Total truth and falsity, which are the only admissible ones in logic and pure mathematics, are hard to come by elsewhere. In applied mathematics, science, technology and the humanities, no less than in ordinary life, most statements are at best approximately true. In most cases we must settle for half truths in the hope of replacing some of them with three-quarter truths. Think of the rounding off of figures, the method of successive approximations, the errors of measurement, and the simplifications made in any mathematical model. Everywhere except in logic and in pure mathematics we deal with approximations and do our best to improve them. Therefore it behoves the philosopher to study error and its complement, partial truth. See Appendix 3.

Twelve centuries before Descartes' *Cogito, ergo sum*, Augustine wrote: "If I err, I am. For he that has no being cannot err, and therefore mine error proves my being. Which being so, how can I err in believing in my being?"

(*The City of God* Bk. XI, Ch. XXVI). Error is our constant companion, hence we should learn to live with it. We do so when learning from error and learning to correct it—which, by the way, is less frequent than learning from success. All observation, measurement or experiment is subject to error, and so is every nondeductive—in particular inductive and analogical—reasoning. However, without such operations we would hardly gain new knowledge. The rule therefore is not "Avoid error", good only for Superman. It is, rather, "Be not afraid of error but of refraining from exploring for fear of erring, and of being unwilling to correct error and learn from it". Moreover we should cultivate controlled error, or what passes for such, by questioning once in a while our most cherished beliefs and modes of thinking, to see whether they need alteration or, on the contrary, can be trusted for another while.

As there are two kinds of truth so there are two kinds of error: of reason and of fact. An *error of reason* is a mismatch among thoughts (or the corresponding propositions), whereas an *error of fact* is a mismatch between thoughts (or the corresponding propositions) and their factual referents. (However, both are brain processes and in this sense factual.) Each kind of error falls in turn into one of the following categories: of *omission* (overlooking propositions or facts) and of *commission* (misconception, misperception, or misdeed).

The most common errors, whether of omission or of commission, are of the following kinds:

(*a*) *erotetic*: asking the wrong question;
(*b*) *postulational*: assuming the wrong premises, be these presuppositions or explicit hypotheses or data;
(*c*) *dialectical*: advancing the wrong argument;
(*d*) *methodical*: choosing the wrong method or technique;
(*e*) *design*: designing the wrong operation, measurement, experiment, plan, or artifact;
(*f*) *computation*: making calculation mistakes;
(*g*) *sampling*: inferring from unrepresentative samples;
(*h*) *axiological*: wrong evaluation of, e.g. a proposition or a goal.

We say that an error is *wrong-headed* if it combines two or more errors of the above kind—e.g. the wrong problem with the wrong approach. (*Example*: Asking at what time the soul enters the human embryo, and looking for the answer in a theological treatise.) Characteristically, ordinary knowledge is plagued with errors of all the above kinds, often to the point of being wrong-headed. Worse, ordinary knowledge errors tend to be self-perpetuating (in popular myth) rather than self-correcting (as in science and technology). In particular, every time we announce a generalization from a hasty personal sample we commit a sampling error, often compounded by the contrary statement suggested by an equally nonrepresentative sample.

Design errors deserve a special comment. In measurement or experiment they are called *constant, systematic*, or simply *biases*. They are mistakes in the design of the operation, e.g. lack of adequate controls, or electrical leaks. Once detected they may be corrected, often completely. Such constant errors must not be confused with random errors, such as typing errors and the fluctuations around the average of a set of measured values. These other errors are really random: they have a regular statistical distribution (usually bell-shaped or gaussian). Although they can often be reduced, they can never be completely eliminated because they are rooted in objective randomness—e.g. the random thermal motions in both the measured object and the measuring instrument. (For this reason they should not be called *random errors* but perhaps *random scatters*.) Since chance is universal, random errors can be studied regardless of the nature of the measurement or experimental set-up. In fact they are studied by the mathematical theory of errors of observation, an application of the calculus of probability. On the other hand, since the constant (systematic) errors depend upon the nature of the components of the experimental set-up or its manipulation, there can be no general theory of constant (or design) errors.

Luckily we possess error detection mechanisms and have invented error correction procedures; moreover, both are perfectible with learning. The human brain seems to be equipped with error detecting and correcting systems—sensory, motor, and conceptual. The efficiency of such systems improves with (successful) practice and deteriorates in pathological conditions. For example, under the action of neurotropic drugs, or in the presence of brain disease or injury, such error detectors fail, and consequently the performance of mental activity becomes suboptimal; the electrical stimulation of such detectors affects them likewise (Bechtereva, 1978, pp. 134ff.)

Wiener (1948) conjectured that every error-correcting mechanism is cybernetic. To understand this recall that every error is a discrepancy or deviation, e.g. from the truth, or from some desired or normal value. Thus, in the case of a numerical variable, if the actual value is a and its correct (or optimal) value c, then the error in that variable is the absolute value of their difference, i.e. $\varepsilon = |a - c|$. If this discrepancy in the output of the system concerned overtakes a tolerable threshold, the control system is activated, and adjusts the input so that the error will decrease. This cycle is repeated until the (intolerable) error vanishes. We have become accustomed to thinking that such negative feedback mechanisms are common to inquiring systems and artificial control systems.

No doubt, errors in the performance of sensory-motor tasks and elementary intellectual operations are likely to be corrected (if detected) in this manner, i.e. by activating feedback loops in existing neural systems. However, the recognition and correction of bulkier errors, such as choosing the wrong problem, the wrong approach, or the wrong world view, may require far more than an existing feedback mechanism. Such errors may call for a radical reshuffling of neural connections—i.e. a self-reorganization—and the corresponding emergence of new conceptual systems.

When rendered conscious, the error correcting processes take the form of *methods* such as the dialectical method, the hypothetico-deductive method, the method of successive approximations, the statistical method, and the experimental method. One uses Socrates' (not Hegel's) *dialectical method* when investigating a set of premises, such as hypotheses and data. If they turn out to be utterly irrelevant or false, one discards them; but if they are relevant and one finds a grain of truth in them, one attempts to refine them, render them consistent with the antecedent knowledge, or adjust some of the latter to the new finding. (This procedure is not dialectical in Hegel's sense because it does not involve concepts that "contain their own negation"—whatever this may mean—and does not advance from contradiction to contradiction but from ignorance to knowledge, from falsity to approximate truth, from stray bits to systems, or from shallowness to depth.) The *hypothetico-deductive* method, or method of hypotheses, is a particular case of the dialectical method: it consists in advancing hypotheses, investigating their consequences, and adjusting or eliminating the former if such consequences turn out to be false.

The *method of successive approximations* is another particular version of the dialectical method. In mathematics it usually works as follows. One poses a certain problem and tries to solve it. If the problem cannot be solved exactly (or in closed form) one obtains a first approximation, which serves as the basis for constructing a second order approximation, and so forth, until a sufficient approximation, or even an exact solution, is reached. In factual science and technology the method of successive approximations works similarly but not quite, for in this case there are no algorithms for advancing securely from one hypothesis to the next: one proceeds in zig-zag until hitting on an acceptable hypothesis—or on none. However, one does follow a prescribed sequence of steps such as: Initial data–hypothesis 1–data 2–hypothesis 2–. . . –hypothesis n, or some other alternate chain of conjecture and evidence.

The *statistical method* is a particular case of the method of successive approximations, in which the hypothesis, which concerns an unknown population, is suggested or checked by investigating a random sample of it, and rechecked by resampling. Finally, the *experimental method* is the same thing, except that experiment is explicitly involved. All five methods are error correcting (or rather minimizing) procedures. And the last three—the method of successive approximations, the statistical and the experimental method—presuppose that truth is usually partial rather than total.

Error is the complement of truth: the smaller the former the greater the latter. In symbols, $V(p) + \delta = 1$, where $V(p)$ is the truth value of proposition p, and δ the relative discrepancy or error of p relative to some other proposition taken as a base line. The latter may be a datum if we are checking a hypothesis, or a law statement if we are checking a datum. In writing the above formula we have assumed that truth values are real numbers comprised between 0 (utter falsity) and 1 (complete truth). That is, we have assumed that V is a function from a set P of propositions (those that can be assigned a truth value) to the unit interval [0, 1] in the real line. We have also assumed that the truth value of a proposition is relative to some other proposition, which is not questioned when evaluating the former although it may of course be questioned in a different context.

TABLE 12.2
Truth value of proposition p assuming base line q is true, i.e. $V(p) = 1 - \delta(p,q)$

Discrepancy $\delta(p,q)$	*Truth value* $V(p)$	*Vernacular*
0.0	1.0	p is true relative to q (rel. q)
$0.0 < \delta \leq 0.2$	$1.0 > V(p) \geq 0.8$	p is approximately true (rel. q)
$0.2 < \delta \leq 0.4$	$0.8 > V(p) \geq 0.6$	p is half true (rel. q)
$0.6 < \delta \leq 0.8$	$0.4 > V(p) \geq 0.2$	p is three quarters false (rel. q)
$0.8 < \delta \leq 1.0$	$0.2 > V(p) \geq 0.0$	p is nearly false (rel. q)
1.0	0.0	p is false (rel. q)

Therefore our initial formula should be generalized to read

$$V(p) = V(q) - \delta(p, q).$$

Table 12.2 exhibits a few typical cases.

The discrepancy $\delta(p,q)$ may be equated to the *reliability index*, a widely used measure of the theory-data discrepancy. This index is defined as follows. Let p and q be propositions of the form "The value of property M for thing a is r", or "$M(a) = r$" for short, where r is a real number. If both p and q refer to the same thing, and q is adopted as the base line, then the discrepancy is defined as

$$\delta(p,q) = \frac{|m - m'|}{|m'|},$$

where m and m' are the numerical values of the property M according to propositions p and q respectively, i.e. $p = \ulcorner M(a) = m \urcorner$, $q = \ulcorner M(a) = m' \urcorner$.

The above reliability index or discrepancy can be generalized to sets P and Q of propositions, such as a set of theoretical formulas and the corresponding set of empirical data, by summing over the corresponding values:

$$\delta(P, Q) = \frac{\sum |m - m'|}{\sum |m'|}.$$

In particular, this formula covers both the case of checking theoretical calculations against observations, and of checking observations against calculations. But it applies only to a smallish sample of all the propositions of a theory, namely those that are actually subjected to tests. Moreover, that sample is not a random one, so if the theory-data discrepancy is small (large) we cannot safely infer that the theory itself (together with the subsidiary assumptions and the data necessary to derive testable consequences) is true (false). Finally, the above formulas apply also to the confrontation of theoretical propositions with other theoretical propositions, and of empirical data with other empirical data, provided the confronted pairs of propositions share a sense and a reference. For more on the way (partial) truth values are assigned in science see Vol. 2, Ch. 8, Sections 2.2–2.4. For theories of partial truth see Appendix 3.

Two warnings are in order. One is that, in general, there is one discrepancy value for each value of the variable(s) under experimental control. Hence as the experimenter explores different parts of the theoretical curve the discrepancy may well vary, i.e. increase or decrease.

Moreover this variation is *lawless*, because there is no logical or physical necessity linking the experimental data to the theoretical predictions.

The second warning is that experimental accuracy depends not only upon the quality of the instrumental, e.g. the laboratory equipment, but also upon the mathematical form of the formulas being checked—which form is to some extent conventional. A simple example will suffice to make this point. Suppose the task is to check a theoretical formula of the form "$y = ax/(b+x)$". This task is complicated by the fact that a is the asymptotic value of y, i.e. $y(\infty)$: see Figure 12.2. To facilitate the task, one performs the simple trick of transforming the given formula into a linear equation by means of the variable changes $x = 1/u$ and $y = 1/v$, which ensues in "$v = (1/a) + (b/a)u$". Now a is the reciprocal of the ordinate value at the origin, usually an accessible number.

Inductive logicians have proposed a number of acceptance and rejection rules for propositions, that are independent of the concept of truth. Scientists appear to abide, albeit tacitly, by a different set of rules, namely

*R*1: Accept (*pro tempore*) all and only the (sufficiently) true propositions.

*R*2: Reject (*pro tempore*) all and only the negates of the (sufficiently) true propositions.

*R*3: Suspend assent or denial of all and only the propositions that you neither accept nor reject (*pro tempore*).

These rules can be unified and somewhat exactified by introducing three functions from the set of propositions to the set of decisions: A (acceptance), R (rejection), and S (suspension). The last two functions can be defined in terms of the first, namely thus:

$$\text{For any proposition } p,\ R(p) =_{df} A(\neg p),\ S(p) =_{df} \neg A(p) \,\&\, \neg A(\neg p).$$

The above rules reduce then to the single rule

R: *For all propositions p*, $A(p)$ *iff* $V(p) > l - b$, where b is the tolerance or maximal discrepancy allowable given the state of the art.

We close with a batch of assorted remarks. First, Wittgenstein (1953) and his followers have claimed that the concept of truth is dispensable: that every proposition of the form "p is true" can be replaced without loss with "p". This no-truth thesis about truth is logically and factually mistaken. It is a logical mistake because "p is true" is a metastatement, not a statement. (In other words, $p \neq \ulcorner V(p) = 1 \urcorner$.) The thesis (known as "the absolute theory of truth") is factually erroneous because scientists and technologists want to know whether their hypotheses are true, and this is why they check whether they "fit" the data. (The very notion of goodness of fit involves the concept of partial truth.)

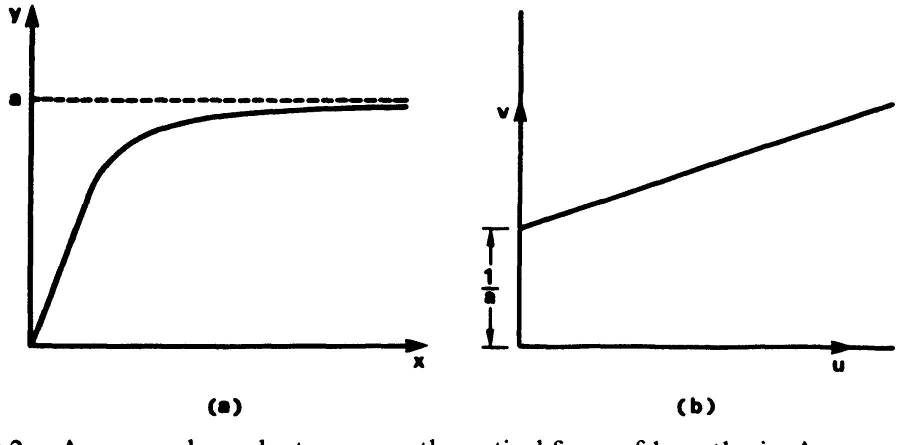


Fig. 12.2. Accuracy dependent upon mathematical form of hypothesis. A mere change of variables transforms the arduous task of determining the asymptotic value a into the far easier one of measuring the ordinate $1/a$ at the origin in (b).

A related doctrine is conventionalism and, in particular, fictionism, according to which the scientist does not seek truth because he investigates phenomena not reality. "He seeks system, simplicity, scope; and when satisfied on these scores he tailors truth to fit" (Goodman, 1978, p. 18). Let us gloss over the unfortunate fact that the author of this quote does not explicate the notions of simplicity and scope: suffice it to say only that, if scientists and technologists did not care for truth, and could make it up, they would not devote so much time to checking their information and testing their hypotheses for truth.

It is usually held that truth values do not really change in the course of time: what may happen is that sometimes we fail to discover their real truth values. This view presupposes that truth values are intrinsic or absolute rather than relative to some body of (empirical or theoretical) evidence. The obvious question is: How does anyone know that a given proposition has a certain truth value before it has been tested (or perhaps even formulated)? As long as no satisfactory tests have been performed we had better abstain from assigning truth values, i.e. from formulating metastatements of the form "$V(p) = v$". Factual truth values are assigned on the strength of tests and, since these are fallible, truth values are transient. This does not entail that truth, like fashion, is quite arbitrary: it does entail instead that truth, like fashion—and like everything else in matters of knowledge—is changeable.

Does the transience of truth values mean that factual knowledge is only probable in the technical sense, i.e. that truth is the same as probability? Clearly, truth differs from objective (physical) probability, or propensity.

Thus consider a thing that can be in any of n different states with equal probability $1/n$. The truth value of "The thing is in state m", where m is one of the n states, is either 0 or 1. This example suggests that there is no lawful relation between objective probability and factual truth.

What about subjective probability? Could we speak of the chance of a proposition being (totally) true, and equate that chance with the subjective probability of the proposition? This is what some philosophers have proposed. Since they take probabilities to measure intensities of beliefs, it turns out that the chance of a proposition being true equals the intensity of our belief in it. In other words, they propose the formula *p is true for subject s if, and only if, s believes strongly that p.* Thus truth would be just as subjective as belief. Consequently science and technology would have no more claim to truth than myth and religion. True, most proponents of subjective or personalist probability do not draw this consequence: they are inconsistent. But we insist on evaluating philosophical views by their fruits.

Other philosophers have proposed to substitute intersubjectivity for subjectivity, advancing the slogan *True is what the majority of researchers believe.* This, the consensus view, has been defended by Fleck (1935), Polanyi (1958), Kuhn (1962), Ziman (1968), and Rorty (1979). True, consensus is an indicator of truth; but it is not the same as truth: remember the fable of the Emperor's clothes. Factual truth is not just a matter of belief but of evidence. So, the above slogan would be adequate if in turn one could prove that in all cases the majority of researchers believe only the propositions supported by plenty of evidence. But this is a matter for social psychology. In epistemology we are interested in finding out whether or not there can be such thing as objective truth estimated on the strength of tests and quite independently of the strength of the beliefs of individuals or even entire communities. This is why we care for confirmation (Section 2.1).

1.2. *Usefulness*

An item of knowledge, such as a theory or a plan, can be valuable cognitively, practically, or both. Obviously we prefer our knowledge items to be valuable in at least one way. And it is also clear that this desideratum is not satisfied by every cognitive item. Thus counting the number of pebbles on a given beach is likely to be just as worthless as devising a technique for catching ghosts. In sum, not all things are worth knowing—just as not all actions are worth performing.

Cognitive and practical value are deceivingly transparent. Actually they are rather difficult to define and attribute. Let us start by examining the general notion of value, which will be studied in detail in Vol. 7. There is, first, the naive notion of *absolute* value, such as it occurs in the question 'What is the value of this datum (or that proposal)?' This notion is imprecise, for every time we assign a value to an object we do so in some respect or other: an item may be good for something but worthless or even disvaluable for something else.

The notion we want is that of *relative* value, or value in some respect, such as it occurs in the statement that cosmology is cognitively valuable though practically worthless. We are often in a position to make reasonable qualitative value judgements such as "The value of item x in respect R is great (or small or nil or negative)". And we can analyze such a global value statement in some such way as this: "The R-value of x for thing (e.g. person) a in circumstance b and in view of desideratum c is great (or small or nil or negative)" (Bunge, 1962b).

An object that is valuable may be so in itself or as a means for attaining some other object possessing a value in itself. We speak accordingly of *intrinsic* and of *instrumental* value as two varieties of relative value. (Intrinsic value must not be mistaken for absolute value: whatever is valuable is so in some respect or other—in particular as a means or as an end.) If we could quantitate value we would say that the total value of an object equals the sum of its intrinsic and instrumental values.

(It may be possible to quantitate value, i.e. to assign a definite number to the value of every object of some kind in some respect. I.e. one might try to postulate that, for a certain kind K of objects, in a certain respect R, there exists a function V_R from K into the double unit real interval $[-1, 1]$. Hence one could write, e.g. $V_R(x) > 0$ for "x is valuable in the respect R", $V_R(x) = 0$ for "x is worthless in the respect R", and $V_R(x) < 0$ for "x is disvaluable in the respect R". This possibility will be explored in Vol. 8.)

A realistic way of looking at valuation is to do so in terms of the *relative and comparative* notion of value, occurring e.g. in the question 'Which of the items x and y is the more valuable in the respect R?' Indeed it is often possible to rank objects of a given kind (e.g. hypotheses) in a certain respect (e.g. testability), much in the way we grade students with respect to originality and industry. We shall assume that, at least in principle, every homogeneous set K of cognitive items can be ordered in a given respect, such as truth or practical usefulness. (The homogeneity condition means that we compare hypotheses to hypotheses, proposals to proposals, and so on.) In other words we postulate that $\langle K, \geq_R \rangle$, where K is a homogeneous set of cognitive items, is a partially ordered set, so that, for any x and y in K, $x \geq_R y$ or $y \geq_R x$. (I.e., $\geq_R$ is assumed to be a simple ordering of K.)

We are now in a better position to clarify the notions of cognitive and practical value. We stipulate that an item of knowledge is *intrinsically valuable* if it is, or is likely to be, a (sufficiently) true proposition. In other words, intrinsic value is, in matters of knowledge, identical to truth and, like truth, it comes in degrees. Secondly, we stipulate that an object is *instrumentally valuable for inquiry*—or *instrumental for inquiry*—if it is an epistemic operation—such as questioning or measuring—or part of one, likely to yield an intrinsically valuable (i.e. sufficiently true) piece of knowledge. Thirdly, we stipulate that a knowledge item is *instrumentally valuable for action*—or *useful* for short—if it is, or is likely to be, a means for attaining a practical goal (whether or not goal itself is valuable in a given respect). Note the cautious clause "is likely to be" in all three definitions: it is a caution not to write off a piece of information, or an epistemic operation, just because it looks worthless at first blush. It may well be that, on a more careful investigation, or under new circumstances, it turns out to be valuable.

From the above definitions it follows that truth, though always (intrinsically) valuable, may not be instrumentally valuable, i.e. may lead nowhere and, in particular, it may not be practically valuable, i.e. useful. It also follows that, for an object to be practically valuable (useful), it need not be intrinsically valuable. In particular, untruths and even lies may be useful for certain purposes—particularly though not exclusively evil ones. Finally, the cautionary "is likely to be" reminds us that, even though we evaluate every cognitive item, we should not regard such valuations as certain and definitive. To proceed otherwise is to be short-sighted.

A couple of examples will help grasp the above notions. Unveiling the stratigraphy of a given site is intrinsically valuable, for it contributes to our knowledge of our planet. And it may also be instrumentally valuable, for both cognitive and practical purposes. Indeed, knowing the kinds of rock layers and their order is instrumental to dating whatever fossils or tools may be found in the site, and it is useful to identify minerals or search for them. In other words, geology is intrinsically valuable (as a science in itself), it is valuable to other fields of inquiry, and it is also useful to the economy. Our second example is epistemology: it is intrinsically valuable for contributing to our knowledge about knowledge. But epistemology is also instrumentally valuable for inquiry, if only for clarifying certain key methodological terms such as those of taxonomy and theory. Finally, epistemology can be useful in designing science and technology policies, e.g. by elucidating the very concepts of basic research, applied research, technology, and technical service (such as the practice of engineering or medicine), and by showing that, on the whole, it pays to support the former in order to make progress in the latter.

2. Empirical Value Indicators

2.1. *Confirmation*

Truth is not manifest but there are truth symptoms or indicators. One of them is empirical confirmation or corroboration. We shall deal here with the empirical confirmation of hypotheses and theories. To begin with, we stipulate that a hypothesis p is *empirically confirmed* (or *supported*) by a datum e if and only if (a) e is empirical evidence for or against p (Ch. 11, Section 1.2), and (b) e is roughly as true as p or some logical consequence of p.

The concept of empirical confirmation can be considerably exactified in case both the proposition and the datum in question are quantitative statements, such as

p: The theoretical or calculated value of property M of object x in state y equals t. Briefly, $M_{\vartheta}(x,y) = t$.

e: The measured value of property M of object x in state y equals u plus or minus the relative random error σ. I.e., $M_{\varepsilon}(x,y) = u \pm \sigma$.

In this case we stipulate that p and e are *empirically equivalent to within the error* σ if and only if the difference between the theoretical value t and the average u of the measured values is smaller than the relative random error, i.e.

$$p \sim_{\sigma} e =_{df} |t - u| \leq \sigma.$$

Obviously, p and e are empirically *inequivalent* with respect to the error σ if and only if they are not empirically equivalent to within σ.

It is tacitly agreed, in all factual sciences, that empirical equivalence amounts to empirical confirmation, and inequivalence to disconfirmation. Therefore we adopt the postulate that a hypothesis is *confirmed* (or *supported*) by all the data that are empirically equivalent to it, and *disconfirmed* (or *undermined*) by all those that are empirically inequivalent to it. It is also tacitly agreed that, the more numerous and varied the empirical evidence favoring a hypothesis, the more strongly it is confirmed.

A number of philosophers have attempted to quantitate the concept of degree of confirmation (or corroboration) of hypotheses in terms of their probabilities or, alternatively, improbabilities. In fact an entire industry has developed out of those proposals. But, since the assignment of probabilities to hypotheses is arbitrary, such measures are arbitrary and therefore useless at best, misleading at worst, and in all cases alien to the ideal of objectivity. In science and technology probabilities are assigned only to random *events*, never to propositions. What is true is that probabilities of random events are sometimes used to evaluate hypotheses holding that certain events are not random. In this case the probabilities of events, not of hypotheses, are compared. An explanation of this point may be in order.

Consider the process of self-assembly of a macromolecule, such as a gene, out of its precursors or components. If one models such components as point particles colliding at random, one is able to estimate the probability that the macromolecule got assembled by chance. Since the resulting number is extremely small, one concludes that the precursors did not assemble by chance. If one believes in miracles, one assigns the formation of the molecule to supernatural agencies; otherwise one explains it in terms of interatomic and intermolecular forces such as hydrogen bonds. This kind of reasoning is done all the time in psychology and other sciences, and it has been polished by mathematical statistics. The key words are 'null hypothesis' and 'significance level'. A quick reminder may be welcome.

Suppose we are to test a hypothesis of the form "B depends upon A" e.g. "A causes B". The thing to do is to compare the test results with the rival hypothesis according to which the occurrence of B is a chance event independent of A: this is the *null hypothesis. Example*: An animal can press either of five keys, every one of which has a different letter painted on it; the experimenter wishes to test the hypothesis that the animal discriminated among them and has a definite preference for one of them. The null hypothesis is of course that the animal presses the keys at random, so that the probability of each letter being pressed is 0.20: this is the so-called *chance level.* If the animal does press any one letter with a frequency significantly greater than 0.20, i.e. *above the chance level,* the null hypothesis has been refuted and, by the same token, its rival has been confirmed: i.e. the animal has in fact learned. The significance level (p) is a measure of the "significance" of the above-chance result and so of the rigor of the test. More precisely, p is the probability of observing a difference with respect to the chance level when actually there is none. The value of p is adopted before the experiment is run and is usually required to be 0.05. This means roughly that, if the null hypothesis (chance) is true, then the data will exhibit a difference (with respect to the chance level) only once in every 20 times.

One point to note here is that neither p nor any other probability involved in the above test is the probability of a hypothesis or the degree of confirmation of it. Another point is that the experimenter, far from trying to disprove his own hypothesis, *attempts to refute its rival*, i.e. the null hypothesis. In so doing he inverts Popper's injunction. But of course he takes precautions: by adopting a low significance level, such as $p = 0.005$, the experimenter makes sure that it is unlikely that he will adopt his own hypothesis when in fact the null hypothesis is true. So, the test he gives his pet conjecture is severe although he makes no attempt to refute it: he aims for a *rigorous confirmation*.

At first sight a reasonable measure of the degree of confirmation (or disconformation) of a hypothesis by a set of data would seem to the ratio between the difference and the sum of the number of confirmers and that of refuters of the hypothesis, i.e. $c = (C - R)/(C + R)$. The value of this function is 1 for $R = 0$, $\frac{1}{2}$ for $R = C/3$, 0 for $C = R$, $-\frac{1}{2}$ for $R = 3C$, and -1 for $C = 0$. However, since these values obtain regardless of the total number $C + R$ of tests, as well as of their variety and rigor, they are hardly significant. In conclusion, the above formula is useless. The alternative formulas proposed by inductive logicians fare even worse.

Scientists, usually eager to pin numbers on almost anything, are wary of quantitating the degree of confirmation of hypotheses. Thus nobody in his right mind would say, e.g. that the degree of confirmation of the Schrödinger equation is $\frac{3}{4}$. All we find in the scientific literature is a ranking of empirical evidence strength: *very strong* (or *compelling*), *strong* (or *definite*), *weak* (or *presumptive*), *very weak* (or *indecisive*), and *nil* evidence for or against a hypothesis. Thus the hypothesis that terrestrial life emerged about 3,000 million years ago is judged to be very strong; that mental functions are brain processes, strong; that the neutrino has a nonvanishing mass, weak; that intelligence is inheritable, very weak; that there are superluminal particles, nil; and that acquired characters are inherited, very strong negative.

The weight of a body of empirical evidence depends on the status of the

TABLE 12.3
Impact of new empirical evidence on evaluation of hypothesis

Cases	*Intrinsic value*	*Effect on scientific community*
e confirms established *h*	Minute	Nil
e undermines established *h*	Medium	Weak
e confirms new unrivalled *h*	Great	Strong
e undermines new unrivalled *h*	Small	Weak
e confirms new rival *h*	Great	Medium
e disconfirms new rival *h*	Medium	Weak
e inconclusive for or against established or new *h*	Nil	Nil

hypothesis it is relevant to: it is not the same for an established conjecture as for a new one. And the impact of new evidence on the scientific community depends not only on the intrinsic value of that evidence but also on a number of cultural factors. For example, the scientific community does not hail as enthusiastically as the mass media reports of the refutation of the theory of evolution, or of the confirmation of the hypothesis of telepathy. Table 12.3 exhibits the main situations with regard to both intrinsic value and social impact.

As can be seen from this table, the value of an empirical finding depends crucially on its relation to the hypothesis to which it is relevant. If the results are inconclusive one way or another, it is advisable to alter the experimental design so as to enlarge the sample, or render it more representative, or reduce the constant and random errors. The findings are of maximal value if they support a new unrivalled hypothesis or a new rival. They are far less weighty if they confirm or undermine an established hypothesis. In particular it takes more than a few unfavorable data to bring down a previously confirmed hypothesis. One does not rush to proclaim its demise but tries again, taking extra precautions or even altering radically the experimental design. This is particularly so if the hypothesis happens to belong to a theory enjoying empirical supports of several other kinds. In contrast, it takes only one conclusively negative finding to refute, at least for the time being, a new rival to an established hypothesis. (The latter may enjoy such an enormous prestige that any rival to it may be regarded as a crank not worthwhile testing—which is just as well in the majority of cases though unfortunate in a few.) And it takes many positive tests to conclude to the truth of a hypothesis.

How decisive can empirical findings be? In particular, are there crucial experiments? If by 'crucial' is meant a conclusive and therefore definitive verification of a given hypothesis or theory, then the answer is in the negative, for future alternative hypotheses or theories might account for the same, as well as further, empirical results at least as well. But if by 'crucial' is meant a finding allowing one to *reject* a given hypothesis or theory, or to *prefer* one of them to its known rivals, then there *are* crucial experiments (and measurements and observations). Since existence hypotheses are best proved by exhibiting examples, let us proceed to do so.

Uncounted observations and measurements have refuted conclusively the hypothesis that the Earth is flat; by the same token they have confirmed conclusively the hypothesis that it is (roughly) round—so much so that we call this a fact. Likewise uncounted observations have refuted conclusively the hypothesis that the biospecies are fixed; and, *eo ipso*, they have confirmed conclusively the hypothesis that biospecies have evolved—though the exact manner of evolution is still *sub judice*. The cases of the hereditary transmission of acquired characters, of demonic possession, of the biological inferiority of certain human groups, of the immutability of the human mind, and of the permanence of all social institutions, are parallel: they have all been refuted conclusively and, by the same token, their negations have been confirmed conclusively. (However, note that it is far easier to confirm a negative proposition than a positive one. Thus the conjecture that the reader of this page is exactly 30 years old is likely to be false though perhaps not widely off the mark. Most readers will be instances of the statement that any present readers are not exactly 30 years old.) In short, the radical skeptic is wrong: empirical evidence *can* be found to refute hypotheses in a conclusive manner and thus to confirm conclusively their negates. It is not true that all hypotheses are equally wobbly and stay so forever, so that nothing is ever settled. For better or worse we do not have to start from scratch every morning.

As with hypotheses so with hypothetico-deductive systems: there *are* observations (or measurements or experiments) capable of tipping the balance, in a decisive manner, between two rival theories. One case will suffice to prove this existence hypothesis. Hertz's experiments on electromagnetic waves destroyed once and for all the action at a distance theories of electromagnetism existing in his time and showed the enormous superiority of Maxwell's field theory over them. This is as close to a proof as one can get in factual matters: it proved that theory T_2 (Maxwell's) was *truer* than theory T_1 (e.g. the Gauss and Weber theory)—without however proving that T_2 was true. But it so happened that, no sooner was T_2 accepted as *the* true theory of the electromagnetic field, than Planck found a flaw in it (blackbody radiation) and Einstein another (photoelectric effect). This showed that T_2 was in need of basic corrections. These were provided by quantum electrodynamics (T_3), which is not only more accurate but also far richer than T_2, and consistent with quantum mechanics to boot. Still, T_3 is known to have certain flaws (the so-called divergences) that call for the construction of an even truer theory T_4 which is still due. In conclusion, although no experiment (or measurement or observation) can conclusively verify any theory, some experiments allow one to make *decisive choices* between given rival theories.

Why cannot theories (hypothetico-deductive systems) be confirmed in a conclusive manner: why must we settle for choosing the best confirmed member of a pair of theories? One reason is of course that, unlike a hypothesis, a theory is a system of infinitely many statements. (Recall Ch. 9, Section 1.2.) We could not possibly check every one of them but must select, for test purposes, a finite number of hypotheses in the theory. So, even if all of these statements were to pass the empirical tests, there would still remain infinitely many untested propositions in the theory. The second reason for the impossibility of confirming conclusively (i.e. verifying) a theory is that, for it to be checked, it must be enriched with data and indicator hypotheses (Ch. 11, Section 2.2) and, possibly, with subsidiary hypotheses as well. In other words, it is not just the bare theory but the theory together with all such additional propositions—some of which are data and others hypotheses—that is confronted with the new empirical evidence. Consequently we may blame any unfavorable outcome of the test on some of the additives to the theory, and may propose to revise some of these. And if the test is favorable to a particular theory-data-indicator hypotheses-subsidiary hypotheses combination, we must still try further such combinations with the same theory. Should all of them succeed we may hazard to diagnose the theory as (sufficiently) true. However, strictly speaking no theory can be assigned a truth value, for such assignment would require checking its infinitely many formulas.

Popper (1959) has held that confirmation is worthless because it is easy to come by, whereas refutation requires severe tests and can be decisive. A number of cases confirm this view, but there also arguments in favor of the thesis that confirmation and refutation are equally cheap or costly depending on the background knowledge used to design and interpret the tests as well as on the severity of the latter. Let us first offer three cases that seem to lend inductive support to Popper's anti-inductivist methodology.

First case: "My dog keeps the ghosts away by wagging his tail" seems to be confirmed by the nonexistence of ghosts in his vicinity. (Actually there has been no experimental test of the hypothesis, for the dog has not been prevented from wagging his tail, and there has been no independent check on the ghosts hypothesis.) *Second case*: "Of the two hypotheses, creationism and evolutionism, the former is by far the better confirmed, for whatever happens is in the last resort the will of God". (Likewise no controls have been set up. Moreover it would be considered blasphemous to try and control or even monitor in any way the divine thoughts and deeds.) *Third case*: "There is plenty of evidence for witchcraft, such as the hundreds of confessions signed by witches during the 16th and 17th centuries, not only under torture but also spontaneously, and all of which read very much alike—a likeness that cannot be mere coincidence". (Needless to say the Inquisition did not conduct any experiments on the presumed witches, nor were these examined by 20th century psychiatrists to find out whether they were psychotics.) This case is of great methodological interest because it shows the crucial role played by the "interpretation" of data in the light of our background knowledge. Indeed, it appears that the said confessions were cast in the language of the demonology concocted by Dominican priests on the basis of pagan myths and Christian theodicy, much as our contemporaries undergoing psychoanalytic treatment talk, feel and dream as dictated by their therapists. Thus "the mythology created its own evidence, and effective disproof became ever more difficult" (Trevor-Roper, 1969).

The above examples confirm, then, Popper's thesis that empirical confirmation is insufficient for it can be had too easily for any hypothesis. But they also show what is wrong with his thesis that confirmation is unnecessary and that only refutation, or unsuccessful attempts at refuting, count. In the first place, all three cases discussed above were examples of observation, not experiment: if the proper controls had been incorporated into the observations, the results would have been different or, rather, there would have been no favorable results. Second, all empirical operations are dependent upon our background knowledge: data do not come bare but wrapped in presuppositions. By altering some components of this wrapping—some of the hypotheses or data included in it—we may be able to change a confirmation into a refutation or conversely. Thus if we believe in witches we shall tend to accept reports on witchcraft, whereas if we disbelieve in witches we shall discard them out of hand—or shall inquire into the mental health or the level of education of our informant. The point is that unfavorable empirical evidence is just as vulnerable as positive one to the falsities in the background knowledge against which it is evaluated.

There are further arguments against refutationism. One is that it is far from easy to confirm a precise theoretical prediction, such as that the 3489 Å (ultraviolet) line in the spectrum of lithium results from a transition between two bound excited states of the negative lithium ion (Carlos Bunge, 1980). Checking this prediction requires constructing a device for producing negative ions in a certain state and using ultraviolet spectrometry techniques. A second argument is that it is psychologically and methodologically false to pretend that scientists value only attempts to shoot down their favorite hypotheses. To be sure, one will examine critically his theories but, once built and checked, it is only human to wish to see them reconfirmed. Moreover, when convinced of their truth one will tend to disregard criticism. Thus when the noted experimentalist W. Kaufman announced in 1906 that his experiments had refuted the special theory of relativity, Einstein and Planck did not budge. Ten years later it was discovered that the experimental results had been vitiated by a leak in the vacuum system (Holton, 1973). Luckily neither Einstein nor Planck were falsifiabilists. They were only fallibilists.

As for the methodological need for confirmation, it is just as clear: there is no other empirical indicator of factual truth. This is why physicists are still withholding judgment concerning the hypothesis of the existence of gravitational waves, black holes, and magnetic monopoles until someone detects them. No theoretical argument and no criticism can substitute for clear-cut—even if revisable—empirical confirmation.

The above reasons suffice to reject refutationism (deductivism) as well as confirmationism (inductivism). Section 3, on the nonempirical indicators of truth value, will yield further arguments against those two doctrines. The view we adopt instead can be summarized as follows. First, confirmation and refutation usually come in degrees such as nil, very weak, weak, strong, and very strong. Consequently factual truth itself comes in degrees. Second, because hypothetico-deductive systems contain infinitely many propositions, and some of them (namely the data, indicator hypotheses, and subsidiary assumptions) originate outside the systems, these can never be fully tested. However, if two rival theories are testable at all then we can, at least in principle, find out which of them is the best confirmed by a given set of data. Thus a choice among theories is possible. Third, imprecise hypotheses and theories withstand empirical tests far better than precise ones. In particular the negation of an exact hypothesis—such as "$a \neq b$"—is in general easy to confirm and therefore of little use except as a guide in the search for alternatives. Negation is cheaper than affirmation.

Our fourth principle is that failure to refute a hypothesis may not be of much value, particularly in the case of existential hypotheses such as those affirming that there are black holes, vector bosons, and extraterrestrial intelligent beings. Indeed failure to find the thing conjectured may be blamed on unsuitable equipment or search tactics. Fifth, although confirmation is insufficient, it is indispensable for assigning (temporarily) truth to some degree. Sixth, every scientist subjects his conjectures to critical examination and asks his peers for comments and criticism on them, but he also wishes to defend them from destructive criticism if he believes them to contain some grain of truth. In criticizing a theory we should be rather generous, in defending it rather parsimonious: the former because we do not want to nip possible advances in the bud, the latter because we do not want to condone error. Seventh, the value of confirmation and refutation depends critically upon the quality of the antecedent knowledge employed to produce and interpret the data concerned. Eighth, in actual scientific practice hypotheses and theories are evaluated not only in the light of data but also in that of nonempirical criteria, such as consistency. This last point will concern us in Section 3.

2.2. *Efficiency*

Cooking and medical recipes, technical rules and moral norms, as well as instructions and programs, are not propositions but *proposals*. And they are not true or false but efficient or inefficient. (On the other hand a proposition of the form "Rule r is efficient" may be true or false.) However, the adequacy of proposals is (or ought to be) assigned on the strength of tests. Not tests for truth, to be sure, but tests for efficiency. Thus we want to find out whether a given prescription for curing a certain illness is efficient, and whether a certain social program for eradicating a given social plague works.

In principle, any given proposal may be efficient to some degree, or inefficient, or even counterproductive. In some cases we may quantitate such efficiency, i.e. we may assign a proposal an efficiency value comprised between -1 (maximally counterproductive) and 1 (maximally efficient).

And the efficiency of a course of action ruled by the given proposal, in attaining its goal, can in turn be set equal to the product of the efficiency of the proposal by the value (or utility) of the goal itself.

Practice, which is not a test of truth, is the ultimate test of efficiency. Hence efficiency tests must be given independently of truth tests. This is not, of course, the view of pragmatism and dialectical materialism, for which only practice counts. But this view is wrong, as shown by the uncounted theories in basic science that are practically useless and therefore neither efficient nor inefficient, as well as by the uncounted myths that have been highly successful—e.g. in recruiting believers and activists—without being true.

In general, then, efficiency is not equivalent to truth, let alone identical with it, and the two predicates are not related in any regular way. There is, however, one important case where such relation does exist, namely when a proposal is based on scientific laws. For example, if we have ascertained that events of type *A* are always followed by events of type *B*, and estimate that we can control or produce events of type *A*, then we can set up the proposal "In order to get *B*, do *A*", or *B per A* for short. As a matter of fact the same law justifies also trying the dual proposal "In order to avoid *B*, prevent *A* from happening", or *non-B per non-A*. That is, one and the same set of laws may be used as a foundation for a pair of proposals, one for attaining a goal and the other for avoiding it. We call such proposals *nomologically grounded* (or *justified*). And we stipulate that technology fashions and recommends only nomologically grounded rules, norms, and proposals. (More in Bunge (1967b).)

Note that basing a proposal on a set of laws bears on efficiency, not morality: thus the proposal might be the efficient killing of innocent people. Hence showing that a proposal is based on a set of laws justifies it only partially. A full foundation of a proposal to use means *A* to bring about goal *B* must include a moral justification of both means and goal. Likewise if we propose to prevent *A* from happening in order to avoid *B*, we must justify morally our not doing *A* as well as avoiding *B*. More in a while.

The differences and relations between a law and the proposals based on it are summarized in the comparison between the truth table of the former and the efficiency table of the latter: See Table 11.3. In it we have assumed, for the sake of simplicity, that the law statement "If *A* then *B*" is completely true rather than only partially true. (This form fits both causal and probabilistic laws. In the latter case the consequent reads "*B* with probability p".) And we have assumed that the efficiency of the correspond-

TABLE 12.4
Truth table of law and efficiency tables of rules based on law.

A	*B*	*If A then B*	*B per A*	*non-B per non-A*
1	1	1	*f*	—
1	0	0	0	—
0	1	1	—	0
0	0	1	—	*f*

ing proposal can be gauged by a number f. We have not set f equal to unity because a law statement usually refers to an idealized (in particular simplified) model of things of some kind. Such ideal conditions can often be approximated in the laboratory. On the other hand real life, which is the domain of practical proposals, is messy. Hence the efficiency of a proposal may be less than maximal even if the underlying law statement is extremely accurate. This does not entail that practical proposals can dispense with law statements. On the contrary, they have to involve further law statements to account for the complexity of real life situations.

Note the following additional differences between proposals and their underlying law statements exhibited by Table 12.4. To begin with, whereas the law statement "If A then B" has a truth value for all four combinations of antecedent and consequent, the corresponding proposals have efficiency values only in half of the cases, namely when the means A is in fact applied. In this case, if the goal B is achieved to the extent f, we infer that the proposal works to precisely this extent. But if A is not implemented then we cannot infer anything with regard to the efficiency of the rule.

Finally, whereas law statements are value free, proposals are not, since they all concern goals that are more or less valuable. Moreover the means as well as the goals may be the subject of valuation, not only economic but also moral. Therefore mastering a law of the form *If A then B* does not warrant jumping without further ado to the rules ***B per A*** and ***non-B per non-A***. We must also evaluate both A and B, and we should declare explicitly what our preferences are. Our conduct should then be guided by the following maxim: Try ***B per A*** if, and only if, (*a*) "If A then B" is a law statement, (*b*) A is attainable or feasible, and (*c*) B is desirable and moreover outweighs A by far. The dual role, *non-B per non-A*, is subject to similar conditions. Condition (*a*) is epistemological, (*b*) is technical, and (*c*) is axiological (possibly moral).

The above maxim starts with the word 'try' because the conjectured efficiency of a rule should be checked rather than being taken for granted. As suggested before, a proposed rule may have to be altered (in particular complicated) to cope with reality. Or it may have to be replaced with an alternative rule, in particular one involving a different means to the same goal. And different rules are likely to have different efficiencies. The truth of a law statement suggests the efficiency of the associated proposals but does not guarantee it nor, in particular, does it guarantee maximal success. Nor does success warrant truth: it only poses the problem of explaining why the given proposal works. Thus the apparent success of acupuncture in decreasing pain poses the scientific problem of uncovering the neural mechanism whereby acupuncture raises the pain threshold.

In conclusion, there is no simple relation between truth the efficiency. Consequently the relations between theoretical knowledge and practical knowledge, science and technology, and knowing and doing, are involved. In particular, technology does not flow automatically from science—nor the other way round. (Obvious moral for science and technology policy: Cultivate both.) Another consequence of the complex relation between truth and success is the inadequacy of pragmatism as a philosophy of either science or technology for, by conflating truth and efficiency, pragmatism accounts for neither.

3. Conceptual Value Indicators

3.1. *External Consistency*

Empirical confirmation, though necessary, is not a sufficient indicator of factual truth, and this for the following reasons. Firstly, in principle any number of hypotheses and a large number of theories can cover the same data. I.e. two or more inequivalent hypotheses or theories may enjoy the same inductive support—as was the case in the 16th century, though not in the next, with the sun-centered and the earth-centered planetary astronomies. Secondly, the zeal in fitting the data may be overdone. Thus by increasing the number of uninterpreted (phenomenological) parameters one can improve indefinitely the fit to any given set of data, though at the price of losing understanding and predictive power: overfitting does not pay.

In short, data are insufficient grounds for choosing among competing hypotheses or theories. Nor is internal (logical) consistency sufficient in the case of theories. Though necessary, this condition too is insufficient, for one can spin consistent theories at odds with the facts. This being so, there are two possibilities: either nothing can help us choose between two consistent and equally well-confirmed theories, or something can. To find out which is the case we must examine the way scientists and technologists proceed—rather than, say, consult some philosophical authority. This section and the next will be devoted to such examination.

When an investigator sketches his research plan—in modern parlance when he 'writes his grant application'—he makes sure that, no matter how novel his ideas may be, they do not clash with the whole of antecedent knowledge. To put it positively, our researcher takes it for granted, or attempts to show, than even his most heterodox ideas and procedures are congenial with our background knowledge or, rather, with the bulk of it. For example, if he proposes to investigate the hypothesis that inoperant ("selfish" or "junk") genes are fossil ones, i.e. nonfunctional vestiges of long past evolutionary stages, he will not question current mathematics, physics, chemistry, or even the bulk of genetics, let alone evolutionary biology.

We may say that the scientific investigator strives to keep or attain *systemicity* and, in particular, *external consistency*, or the compatibility of his new views with the bulk of antecedent knowledge (Bunge, 1967b). He may well be a revolutionary in one department but he cannot hope to upset the whole of science at one stroke—unless he is a pseudoscientist. The same holds for technological research, design, and planning, which are increasingly being based on applied science. True, the technologist may occasionally defy and refute the odd scientific theory, as was the case with the first aircraft designers, who had to ignore the primitive aerodynamic theory then prevailing. But, unless he is a bogus technologist, he will not go against the grain of mathematics and science; moreover, he will avail himself of as much applied science as he can lie his hand on. (Not doing so can be ruinous, as Edison learnt when he ignored the experts who had told him that, according to theory, alternating current was more efficient than direct current for transmitting electric energy.) In short, the technologist too values external consistency, and does so increasingly.

Why external consistency? Why could we not question everything at the same time? My answer is as follows. First, we *need* not question the entire body of knowledge: we question items of antecedent knowledge only as we find fault with them. Second, what we do need is to extend and deepen the existing knowledge, i.e. to face new problems. (Sure enough, as we proceed we are bound to revise some of the very body of knowledge that we used as our starting point.) Third, we need a launching pad, however wobbly it may be, for no problem can be posed in a vacuum. Every problem has presuppositions—which, of course, may be questioned provided we take some other propositions for granted. (Recall Ch. 7, Section 4.1.) Fourth, by using—at least temporarily—some of the antecedent knowledge we are assured of a fair amount of heuristic guidance and indirect empirical support. For example, if a new hypothesis implies or is implied by another hypothesis that has already passed empirical tests, then any direct support for each of them is indirect support for the other: see Figure 12.3. Fifth, even if we wanted to revolutionize the entire body of antecedent knowledge we *could* not do it, not only because every problem has presuppositions, but also because the evaluation of new ideas cannot be done in a vacuum and must not be done arbitrarily. Thus we shall be justified in preferring one theory to another only if the former accounts better for data or hypotheses that are not questioned for purposes of evaluation. Every well-grounded preference judgment is of the form "A is preferable to B with regard to (or in view of, or to attain goal) C", where C is not questioned during the comparison although it may eventually become an object of criticism. For all five reasons, although science and technology are in permanent transformation they cannot suffer total revolutions. We shall return to this point in Ch. 13, Section 3.2.

Let us watch the principle of external consistency at work. *Example 1*: Wundt (1879) rejected spiritism not for the dearth of empirical data in its favor but because it contradicts the best established scientific laws. (He also noted that magicians and mediums are better equipped than scientists to disclose the tricks of their colleagues, partly because scientists are used to acting in good faith and do not suspect any trickeries.) Hebb (1952) used likewise what he called the "external criteria" of physics and physiology, which "say that ESP is not a fact despite the behavioral evidence that has been reported". *Example 2*: Every time a new theory is proposed in place of an established one, it is judged not only as to internal consistency and empirical support, but also as to its harmony with the bulk of antecedent knowledge. Moreover the new theory is required to yield, in some limit or other, roughly the same correct results as its rival. Physicists call this methodological rule the *correspondence principle*, and most of them refuse even to consider any theories violating this requirement. And for a good reason, namely that external consistency, and in particular "correspondence" with a reasonably good theory, is a weighty truth indicator.

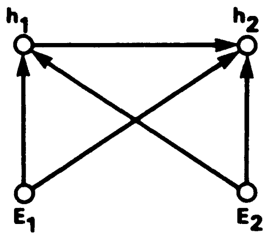


Fig. 12.3. If the hypotheses h_1 and h_2 are logically related, then every confirmer (or refuter) of one of them confirms (or refutes) indirectly the other.

Of course there is risk in applying the criterion of external consistency. By invoking it we may occasionally thwart a valuable piece of heterodoxy—but then error is the unavoidable hazard of inquiry. The principle serves not only to evaluate scientific hypotheses and technological proposals: it also spares us uncounted disasters by weeding out wrong-headed (ill founded) research projects and entire pseudosciences. To give it up would be to condone bogus science and deprive the serious investigator or inventor of any base line to start his inquiry, as well as of a guide to evaluate research plans and outcomes. It is no accident that Feyerabend (1975, Ch. 3) rejects the principle of external consistency and places magic, religion and pseudoscience on the same footing as science.

To prevent the principle of external consistency from enshrining dogma and stifling research it suffices to demand that (*a*) research produce new results—a standard requirement, and (*b*) our fundamental principles be subjected to critical scrutiny and systematization once in a while. The latter task is called *foundations research.* It is an established branch of mathematics but it still has to win acceptance in factual science, where observation and calculation are far more highly esteemed than discussion of basic principles.

The neglect of foundations research is mistaken, for unexamined principles may hide error or may need the company of further principles. And dismissing such research as "mere philosophy" betrays superficiality, for some fundamental principles of science and technology are in fact philosophical. For example, every fundamental physical hypothesis is required to comply with general philosophico-scientific principles such as those of antecedence ("Effects cannot precede their causes") and invariance with respect to observer changes as well as changes in the observer's standpoint or reference system. (For further philosophical principles of science and technology see the Introductions to this volume and to Vol. 3.)

Our view on external consistency as both a necessary requirement and a truth indicator is at variance with three popular philosophies of science: confirmationism (inductivism), refutationism (deductivism), and epistemological anarchism. The first exaggerates the value of examples (confirming instances), the second that of counter-examples (disconfirming instances), the third cares for neither ("Anything goes"), and all three views ignore the matter of external consistency. Yet even the most cursory examination of scientific practice shows that (*a*) data are not compelling by themselves (inductivism) or accepted by convention (deductivism) but only provided they cohere with some previously accepted data and theories; (*b*) a theory is not accepted just when it enjoys a strong inductive support (inductivism) or when all attempts at refuting it have failed (deductivism), but only when these two conditions have been met and, besides, the theory is consonant with some other theories (particularly theories about lower level entities); (*c*) the slogan "Anything goes" may be justified in matters of taste (though even here there are some restrictions) but not in matters of truth (science) or efficiency (technology).

In short, we postulate that every new proposition or proposal be compatible with the bulk (never the whole) of our antecedent knowledge. Moreover we submit that modern science and technology in fact abide by it. This postulate has the following consequences. First, failure to comply with it is a clear indication of pseudoknowledge—in particular bogus science or technology. Second, even though science and technology are characteristically changeable, there can be no total revolutions in them: every transformation, however deep, is local. Nearly total revolutions were possible before the emergence of modern science and technology: from then on every epistemic revolution has been and will be partial because it is a new growth in the midst of a vast system of knowledge. Third, because every body of background knowledge has philosophical (in particular ontological and epistemological) components: (*a*) the evaluation of every radically new proposition or proposal involves or presupposes philosophical considerations; (*b*) there are no frontiers between (good) philosophy on the one hand and the rest of knowledge (in particular scientific and technological) on the other; (*c*) the search for a demarcation criterion between philosophy on the one hand and science and technology on the other is futile (Bunge, 1973b); however, (*d*) since most of philosophy is still at variance with or at least remote from contemporary science and technology, only a deep and sweeping revolution in philosophy or an equally deep and sweeping counter-revolution in science and technology can bring about an overall consistency between philosophy on the one hand science and technology on the other.

3.2. *Other*

Internal and external consistency are not the sole nonempirical indicators of the value of propositions and proposals. Several others, some reliable and necessary, others less reliable and less desirable, or even ambivalent, have been proposed by a few scientists and philosophers. We shall list them with the help of an earlier study (Bunge, 1967b, Ch. 15, Section 15.7). Let us start with the reliable indicators, labeled '*R*'.

In the first place we have three *formal* or logical indicators of the value of scientific or technological hypotheses, theories, or proposals:

R1 Well-formedness: the propositions and proposals should be well-formed rather than gibberish. This condition excludes expressions such as 'The length of *b* is 3′, where the mention of a unit is missing, and 'That method is good', which fails to mention the goal.

R2 Systemicity: it is desirable that every construct be a system (classification or theory) or a component of it. This excludes strays, which are hardly intelligible and cannot be evaluated for truth or efficiency.

R3 Internal consistency: propositions and theories should be logically consistent. This excludes contradictions and demands that we reconstruct any theories containing them.

Next comes a *semantic* indicator:

R4 Interpretability: some mathematical formulas in a scientific or technological theory or proposal should be susceptible to interpretation in factual (though not necessarily empirical) terms. To this end we must join it to semantic assumptions determining what the various predicates refer to and represent. (For semantic assumptions see Ch. 9, Section. 2.2.)

In the third place we have a rather numerous collection of *epistemological* indicators, other than coverage and predictive power, which may be regarded as empirical:

R5 External consistency: i.e. compatibility with the bulk of antecedent knowledge: see Section 3.1.

R6 Consilience: "the evidence in favor of our induction [theory] is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis [. . .] No accident could give rise to such an extraordinary coincidence" (Whewell, 1847, Bk. XI, Ch. V, Section 11, p. 65).

R7 Unifying power: capacity of a theory to embrace previously separate theories—e.g. electromagnetic theory of light, neurophysiological theory of mind—or of a plan of action to bring together previously detached courses of action—e.g. a social program aiming at both job retraining and public works aimed at alleviating unemployment. Clearly, unifying power entails consilience.

R8 Depth: deep theories and techniques, involving fundamental mechanisms, are preferable to shallow ones. In science because deep theories give more than coverage, namely explanation; in technology because deep proposals expose the roots and allow us to control or alter them.

R9 Stability: the theory should not fail at the first unfavorable evidence, and the plan should not founder at the first practical obstacle, but both should be susceptible to repairs. However, stability has a limit. We do not want to save a theory by evasive manoeuvres such as adding further epicycles, for it may then become impregnable to disproof. Nor do we wish to save a plan by making so many compromises that it will not attain its goal.

R10 Heuristic power: new theories and techniques should suggest and guide research rather than just summarizing it, let alone blocking it.

R11 Originality: bold (yet not wild) theories and methods, enabling one to explore old territories in new ways, or to discover new ones, are preferable to well-tried and safe ideas. After all, the goal of research is to gain new knowledge or new power.

Now come two *pragmatic* indicators, particularly desirable in the case of applied science and technology:

R12 Methodological simplicity: the theory should not be so complicated that any calculations with its help, or any empirical tests of it, would be so time consuming that in practice it would remain fallow.

R13 Feasibility: techniques, designs and plans should be capable of being implemented by real people in real time rather than being suited to ideal people and situations.

Finally come three *philosophical* indicators:

R14 Compatibility with a science-oriented ontology: i.e. one of changing things rather than either unchanging entities or ghostly ones. This disqualifies the hypothesis of telekinesis and the proposal of solving the energy crisis by powering machines by psychic power.

R15 Compatibility with a science-oriented epistemology: i.e. one learning from science and thus capable of stimulating the search for true theories and proposals based on objective knowledge. This condition spares us the attempts to gain truths of fact by purely rational means, or by purely empirical ones.

R16 Compatibility with an ethics: enshrining human rights, in particular the right to know, and human duties, in particular the duty to teach and enlighten.

With two exceptions all of the above conditions are mandatory, hence they are good indicators of truth or usefulness. The exceptions are *R11* (originality) and *R12* (methodological simplicity), which are desirable but not absolutely. Moreover, occasionally they must be dropped altogether. For example, building a theory of the iron atom would not shake the foundations of physics but would still be a very distinguished achievement because of the many subsidiary hypotheses and the enormous computational difficulties involved. On the other hand building a theory of the precise way the nucleic acids synthesize proteins would constitute a revolution and it would probably be a theory difficult to apply as well as to test, just as general relativity was and still is.

A number of alternative nonempirical value indicators have been proposed from time to time, which I submit are unreliable and sometimes clear indicators of disvalue. Here are the most popular, each labeled '*U*'.

U1 Coherence: the articulation of a conjecture with other, previously accepted propositions, is the paramount criterion of truth. This is the thesis of the so-called coherence theory of truth (Blanshard, 1939; Rescher, 1973, 1979). This theory is correct in formal science, where it has been exactified and has become part and parcel of model theory (cf. Vol. 2, Ch. 8, Section 2.1). Moreover it has an important grain of truth with regard to factual statements for, after all, when weighing the truth of a proposition we do so by comparing it with other propositions. (In other words factual truth is always relative to some base line that, in turn, may be questioned as long as a further base line is adopted however provisionally. See Vol. 2, Ch. 8, Section 2.2.) However, internal coherence is insufficient, as shown by a number of consistent doctrines proposed by madmen and pseudoscientists. In the case of factual propositions we need also agreement with facts (correspondence) and external consistency, i.e. agreement with adjoining bodies of knowledge. In short, coherence is *a* truth indicator not *the* truth criterion.

U2 Simplicity: some kind of simplicity, logical, psychological, or practical, has been claimed to be the seal of truth. (*Examples*: the medieval slogan *Simplex sigillum veri*, Mach's *Denkoekonomie*, and the conventionalism of Philip Frank and Nelson Goodman.) To be sure we delight in simplifying, particularly in practical matters. But this is not always possible and it is usually undesirable in cognitive matters because the universe we wish to unveil happens to be complex. This is why the mainstream of scientific and technological progress has been a course of increasing complexity in all regards. Compare contemporary theoretical chemistry, based on quantum mechanics, with Lavoisier's. Besides, high speed computers can handle computational complexities that were formerly intractable: this has brought about a devaluation of simplicity. (For more see Bunge (1963).)

U3 Beauty: theories and methods should appeal to our sense of beauty. Yes, but what is beauty and why should it be related to truth or usefulness? A logically well organized (i.e. axiomatized) mathematical theory is likely to look beautiful to a mathematician or a philosopher, ugly to one interested only in applications, and revolting to a data collector. As with simplicity, beauty is welcome but uninvited.

U4 Longevity: the longer a theory or a technique has "survived" (i.e. been in use), the more adequate or better adapted it must be—so the misplaced evolutionary argument proposed by Peirce goes (see Rescher, 1977). True, some scientific theories (e.g. classical mechanics) and techniques (e.g. microscopy) have proved to be extraordinarily long-lived—but only because they have been refined and assigned more modest roles. If survival were a genuine truth or efficiency indicator, then we should accept as true or efficient all the ancient superstitions that are still with us. No, longevity is not necessarily a mark of truth or of usefulness: it can also be an indicator of ignorance. If anything, the older a cognitive item the sooner it is bound to be altered or even forgotten.

U5 Consensus: the goal of research is to win the consensus of the experts (Ziman, 1968. 1979). This is a double-edged idea. On the one hand it suggests complying with the criterion *R5* of external consistency, but on the other it collides with *R11* (originality) and therefore invites discarding heterodoxies, some of which are likely to carry the seeds of necessary revolutions. Fortunately consensus is not universal in basic science or technology, where controversy flourishes. Even the most widely accepted data, hypotheses, methods, designs and plans meet with criticism. (On the other hand applied science and textbooks are usually free from controversy.) Surely the dissenters are always in a minority: so are the innovators who are eventually proved right.

So much for the nonempirical indicators, both the reliable and the unreliable ones, of truth or usefulness. How are research projects and plans of action *actually* evaluated? What criteria does a supervisor, referee or client actually employ to encourage or discourage a given line of research or development? And how do scientists and technologists evaluate their own findings and those of their peers? There is no consensus on this matter. True, internal and external consistency, originality, testability, predictability (in particular serendipity, or the ability to forecast unexpected facts), feasibility, profitability, and a few other indicators are normally taken into account. In short, evaluation is done on the strength of a whole battery of tests, some empirical, others conceptual. (Only philosophers tend to believe that a single factor, such as simplicity, explanatory power, or predictability, is involved.) However, the entire battery is rarely if ever used explicitly: some indicators are not used, and none is assigned a definite rank, let alone weight. As Kuhn (1978, p. 331) says, they "function not as rules, which determine choice, but as values, which influence it".

Worse, in many instances extracognitive factors play a role in evaluation. Among such factors we may mention personal sympathy or animosity, school affinity or rivalry, excessive depth or originality (incomprehensible to the judge), and even ideological bias. All but the last are easy to understand though hard to forgive. The last factor, namely ideological bias, was highlighted by the Lysenko affair: as is well known the quack agronomist succeeded in beheading Soviet biology by accusing it of not complying with Marxism. Less well known is the case of classical economics, in particular Ricardo's: though at variance with many facts and lacking in predictive power, it dominated the British academic and business communities for an entire century because, among other reasons, it explained social injustice and discouraged social reform (Keynes, 1936, pp. 32ff.). Nowadays neoclassical economics, in particular monetarism, though equally at variance with reality and therefore powerless to predict anything, is upheld because it abhors state intervention and planning. In short, ideological factors do play a role in the evaluation of scientific and technological propositions and proposals. We shall return to this point in Ch. 14, Section 4.2.

The intervention of noncognitive criteria, as well as the intervention of false philosophies of science and technology, may explain the frequent failure of the peer review method. (The method consists in asking experts, whose names are not disclosed, to evaluate the projects and accomplishments of their peers in the same field.) The peer review method is double-edged. On the one hand it screens out much fraud and low grade stuff, particularly most of pseudoscience and pseudotechnology. But on the other hand it tends to perpetuate whatever views the referees happen to hold; in addition, anonymity gives them even the chance to exact revenge. That the method does not work to perfection has been shown by two experiments, which have shown that (*a*) the prestige of an author and his institution are decisive factors in the evaluation (Peters and Ceci, 1980), and (*b*) the fate of a particular grant application is half determined by apparently random elements (Cole *et al.*, 1981). Double-blind refereeing solves some of these problems but not all. In any case no better method of evaluation has been proposed, so we should try and improve on it. One way of achieving this goal is to educate scientists and technologists in the principles of the methodology and philosophy of science, so that they may abandon some of the grossest errors underlying valuational mistakes—such as that data are all that really matter, so that all measurements are worth doing, and no theories are worth considering unless they summarize data or help get more data.

4. Concluding remarks

A proposition may be accepted (believed) only if, when subjected to suitable tests, it proves to be sufficiently true. And a proposal ought to be adopted (implemented) only if, on the basis of sufficiently true propositions, it promises to be useful to solve certain problems to a reasonable and good end. But truth and usefulness are seldom manifest: therefore we must resort to indicators.

There are many value indicators, some conceptual and others empirical. (The former may be assessed before any empirical tests are given: such preliminary screening spares much time and effort.) The value indicators may be grouped as follows:

Logical: well-formedness, systemicity, and internal consistency.

Semantical: interpretability in factual terms.

Epistemological: external consistency, coverage, accuracy, consilience, unifying power, serendipity, depth, stability, originality, and heuristic power.

Methodological: testability—i.e. confirmability and, when applicable, refutability as well.

Pragmatic: feasibility (in the case of techniques, plans and designs), methodological simplicity (i.e. simplicity of application), efficiency, and profitability.

Philosophical: compatibility with an ontology of changing things, a realistic epistemology, and an ethics of human rights, in particular the right to inquiry.

Such multiplicity of value indicators renders evaluation delicate but makes for responsible evaluations. In any case that multiplicity ruins the simplistic philosophies of science and technology, such as those summarized in the slogans "*A* theory is only as true as its predictions (regardless of its depth, accuracy, etc.)", and "*A* plan is only as good as its deliveries (regardless of the morality of both means and ends)". The actual process of evaluating propositions and proposals in science and technology is far more complicated than suggested by such simplistic philosophies, but it seldom takes advantage of the full battery of tests, to concentrate only on such properties as accuracy, originality, and usefulness. No wonder, for the scientists and technologists who evaluate the work of their peers have seldom given any thought to the principles of evaluation—principles that they may dismiss contemptuously as being "merely" philosophical.

Unfortunately philosophers retaliate by not doing much better when it comes to evaluating their own ideas. In fact the current standards of excellence in philosophy seem to boil down to smart argumentation (even if it involves ignorance of basic facts), triviality of the problem, possibility of compression into a simple slogan, and prestige of the philosopher's institution. I submit that there is a better way to evaluate a philosophical view, namely to subject it to the following three batches of tests:

Historical: Does the view have any sound historical roots, i.e. does it make contact with the best philosophical tradition? And is it original?

Logical: Does the view constitute a system, and if so is the system internally consistent?

Epistemological: Is the view compatible with the current mathematical, scientific and technological knowledge? Does it solve any interesting problems? Does it have any heuristic power?

When subjected to the above tests, the most popular philosophical views of the day turn out not to be systems, not to harmonize with science and technology, not to possess any heuristic power, and often not even to handle (let alone solve satisfactorily) any outstanding problems. This is a very sad state of affairs which calls for a revolution in philosophy. But the matter of revolutions in knowledge deserves another chapter.

PART V

VARIETY AND UNITY

CHAPTER 13

EPISTEMIC CHANGE

Knowledge, like food, can be stored—for a while. Some of it becomes stale and is eventually recognized as useless, or is no longer of interest to anyone. And whatever does stay is bound to become incorporated into more comprehensive and deeper bodies of knowledge. (Even the immortal Pythagorean theorem has become a particular case of a more general theorem in Riemannian geometry.) Therefore the investigator must be always on the run if he wishes to stay in the same place.

Knowledge is gained and lost in a number of ways: empirically and theoretically, gradually and through revolutions, by disclosing ever finer details and by joining entire fields of research. The more we know the more and harder problems are we able to pose and solve. And the less helpful the existing body of knowledge proves to be to solve new problems, the more it invites its enrichment or replacement.

Like everything new, new knowledge emerges from pre-existing items. And like any other novelty, new knowledge is elicited from within and in favorable external circumstances, and it must overcome resistance. The inner driving force of knowledge is curiosity and, particularly, disciplined curiosity. The external driving force is need and, in particular, the needs of industry, trade, and welfare. And the obstacles to research, hence to novelty in knowledge, are multiple: economic (dearth of resources), political (hostility or indifference to learning on the part of government officials), and cultural (inertia and ideology).

Given the historicity of knowledge, the epistemologist would be ill advised to ignore the history of science, technology, the humanities, and their respective bogus counterparts. And given the social conditioning of research, as well as its impact on modern society, he would be equally ill advised to ignore the sociology of knowledge. However, we cannot find out how knowledge evolves along society unless we know what knowledge is. Therefore epistemology cannot be replaced with the history and sociology of knowledge although it must learn from them.

1. Cognitive novelty

1.1. *Cognitive Kinematics*

Recall a few landmarks in the history of human knowledge since the beginning of civilization a few millennia ago: the invention of writing and its use as a thinking tool as well as to record social and natural facts; formal education; the invention of time keeping and astronomy, of mathematics and logic; the substitution of history for myth; the secularization of at least part of the intellectual culture and the accompanying tolerance for some innovation and some dissent; the establishment of scientific and technological bodies, such as academies and observatories; the invention of scientific theory and controlled experiment; the emergence of hundreds of scientific and technological disciplines; and the practical applications of science, i.e. the conversion of arts and crafts into technologies. No doubt, we have come a long way in a period that is but a small fraction of the total time since the human species emerged.

However, by far the greatest number of mathematical, scientific and technological knowledge we possess today has been acquired since the beginning of the Modern Era, which began only five centuries ago. We are all familiar with the exponential growth of mathematical, scientific and technological knowledge over this period. Indeed specialized knowledge has been doubling in size every decade or so, so that its growth may be likened to that of capital at a compound interest of about 7 per cent per annum. The analogy is apt in so far as in both cases the total size at a given moment depends on both the rate of growth and the total size at an earlier time. This highlights the importance of background knowledge, i.e. of the general assumptions and the fund of knowledge. Moreover, we also have the cognitive analogs of bankruptcies and crashes, namely those caused by depressions, wars and invasions, by mistaken science and technology policies, and by irrationalist movements. The exponential growth of knowledge, like that of the world population, is a modern trend, not a law. (Knowledge grows only provided there are people willing and competent to work on new problems and to try new ideas and methods.) Like any other trend, the exponential growth of knowledge may be altered. In fact there are already signs of decline: see Section 4.2.

Mutability is an essential mark of mathematics, science and technology, just as stasis is one of ideology and pseudoscience. This holds for all branches of science and technology, even the classical ones. A look at the

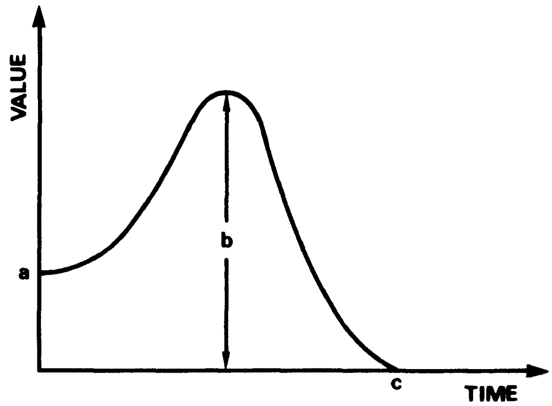


Fig. 13.1 Change in value of a cognitive innovation. Initial value = a; maximal value = b; life span = c.

scientific literature shows that new results keep cropping up even in classical mechanics and thermodynamics, as well as in plant morphology and genetics, in learning theory and human history. It is not only that new data and new hypotheses are found, or new applications are made: some of the very principles of such classical disciplines are modified once in a while. In the case of the classical physical, chemical and biological theories, such renewal is often associated with a modernization of the mathematical formalism. This change is so profound that Newton and Euler would be unable to read a contemporary advanced textbook in classical mechanics.

The value of data, hypotheses and techniques is time dependent. It may increase for a while after inception, reach a peak from which it may decline because the item has been superseded by more accurate information, more comprehensive or deeper theory, or more powerful method—or simply because it has ceased to attract interest. See Figure 13.1. Such decline in value is due to the very nature of inquiry, which consists in investigating problems with the aim of learning something new, i.e. of making original discoveries or inventions, some of which are bound to correct or even eliminate bits of knowledge gained earlier. Obsolescence, not perennity, is the mark of scientific findings and technological artifacts—just as discovery and invention is that of original scientific and technological research and development.

Originality, not only ability to learn much and fast, is the mark of the investigator, be it in science, technology, or the humanities. Originality, feared and repressed until a few centuries ago, is nowadays institutionalized, at least in mathematics, science, and technology. This institutionalization takes the forms of recognition of authorship or patent, and

TABLE 13.1
Phases of normal maturation of science

Phase	*Theoretical aspect*	*Empirical aspect*
0 *Prescience*	Unchecked speculation	Unchecked data
α *Protoscience*	Hypotheses formulated verbally, no theory	Observation and occasional measurement, no experiment
β *Deuteroscience*	Hypotheses formulated mathematically, no theory	Systematic measurement and experiment on perceptible traits of perceptible things
γ *Tritoscience*	Mathematical models (specific theories)	Systematic measurements and experiments on perceptible and imperceptible traits of perceptible and imperceptible things
δ *Tetartoscience*	Mathematical models and comprehensive theories	Precise and systematic measurements and experiments on perceptible and imperceptible traits of perceptible and imperceptible things

academic or pecuniary reward (cf. Merton, 1973). Only in the humanities are endless repetitions of ideas and scholastic commentaries on them, instead of the exploration of new ideas, still tolerated.

We may distinguish five main stages in the normal development of any field of factual inquiry: see Table 13.1. Note the qualifier 'normal': not all fields conform to our schema. For example, cosmology and economics, though mathematically very advanced and well stocked with precise (though insufficient) data, are still dominated by untested speculation; so, they exhibit some signs of maturity together with others of immaturity. (More in Vol. 6.) Also, some fields that teem with unbridled speculation and unchecked data, and so belong in phase zero, are sometimes advertised as scientific although they are clear examples of pseudoscience. (More in Ch. 14, Section 14.1.)

Different fields of research advance at different paces. Thus since mid century biology and history have made great strides, physics and sociology have advanced a fair stretch, psychology and economics only a little. Can the volume and speed of knowledge be quantitated and measured? Several measures have been proposed, such as the number of papers published and quoted. There is even an entire new discipline, scientometrics, with a journal of its own, that purports to gauge scientific and technological advance. However, counting papers and citations gives only an inkling of popularity, not of value.

There is no reliable measure, and no accurate measurement, of anything unless there is some underlying theory. So far we have not got a theory of the growth of science and technology. We only have a handful of rather vague principles, such as the following:

(i) The pace of inquiry in any given field is proportional to (*a*) the fund of knowledge accumulated in the same field, (*b*) the number of fields from which it draws background information, (*c*) the number of workers engaged in research, and (*d*) the number of problems it tackles.

(ii) Research in every field of inquiry can be likened to a river: (*a*) the *mainstream* formed by new lines of research and their continuation, (*b*) the *sidestream* constituted by routine investigations—or, to change the metaphor, it is made up of efforts to fill holes in a terrain the main features of which are already known, and (*c*) the *eddies* composed by barren controversies and heterodoxies (i.e. novel ideas clashing with the bulk of the fund of knowledge).

(iii) The power of any cognitive innovation (datum, hypothesis, method, or design) is proportional to the number of old cognitive items it displaces, and to the number of new cognitive items it renders possible.

(iv) The resistance to cognitive innovation is proportional to the degree of success of the extant body of knowledge which it seeks to displace, as well as to the originality and power of the new item. If there is no rival there is no resistance. If there is, the first reaction to the proposed innovation is to ignore it; the second, to try and find fault with it; the third, to attempt to assimilate it, i.e. to account for it in traditional terms; the fourth, to admit it; the fifth, to turn it into dogma. Resistance to innovation is maximal in stagnant fields. Thus economic theories are often accepted and repeated uncritically, and the economic "establishment" tolerates innovation "of the additive and amending type" but rejects radical novelty (Myrdal, 1973).

(v) Technical innovations are the most readily accepted, the better they satisfy deeply felt needs and wants, and provided they do not harm any powerful vested interests or call for the renunciation of whole life styles.

1.2. *Conceptual Change*

Understandably, philosophers are more interested in theoretical change than in any other type of change: they are interested in the emergence, development, and decline of theories, as well as in their replacement by rival theories. And they are interested in all the aspects of such changes—logical, semantical, epistemological, and pragmatic.

Theories are variously described as ideal objects, systems of changeable meaning and truth value, growing bodies of knowledge, or prescriptions for doing things. These characterizations, though very different, are mutually compatible. In fact from a logical point of view a theory is an ideal object of a certain kind, namely an infinite set of propositions closed under deduction. That one and the same theory may be variously interpreted, and may be attributed different truth values on the strength of different bodies of evidence, is obvious. From an epistemological point of view theories are not static but they grow in certain directions (as more theorems are proved and more applications discovered) and shrink in others (as regions of falsity appear). Finally, from a pragmatic viewpoint theories, even mathematical ones, can be used as rules or prescriptions for computing or for designing experiments, much as musical scores are used as instructions for performing. Since theories are all four—logical objects, semantic systems, growing bodies of knowledge, and prescriptions—according to the viewpoint that is chosen, there is no incompatibility among the four descriptions.

From a logical point of view theories are "already there in one piece" From this vantage point there is no difference between an axiom and the process of hypothesizing it, a theorem and the process of proving it, or a definition and the process of introducing it. Not so from a psychological or a historical point of view. Here definitions are not static identities but either inventions or discoveries of previously hidden identities. Here theorems are not just consequences of a set of premises: they are usually first guessed, then proved. And an axiom is not just an initial assumption that had always been "there" (in the theory): it is a hypothesis, usually tried long after a number of theorems are known. From a logical point of view every result obtained when working on a theory is a discovery not an invention: we only discover occult implication relations. From an epistemological viewpoint there is both discovery and invention: one makes new posits, introduces new concepts, and tries new strategies for constructing proofs.

From an epistemological point of view, a theory is not a self-existing ideal object but a potential body of knowledge that can be partially actualized by working on it. At any given moment we know only a finite subset of all the statements of a theory. This finite and variable collection of statements can be defined as follows. Call T a theory and s a knowing subject. The *part of the theory T known to s at time t* is the subset of the total set of formulas of T that s happens to know at t. And the part of the theory *known to mankind* at time t is of course the union of all such fragments for all the subjects familiar with T. In obvious symbols, $\Theta(s, t) = \{x \in T \mid s \text{ knows } x \text{ at time } t\}$, and $K(t) = \bigcup_{s \in H} \Theta(s, t)$. Perhaps this, the known part of a theory, is what McMullin (1976) means by a 'dynamic theory'.

A second concept of philosophical interest is that of the family of specific theories built on a given general theory—or, to use the term introduced in Ch. 9, Section 1.2, the family of bound models of the theory. (*Examples*: every quantum-theoretical model of an atom or a molecule is a bound model; likewise every model of the evolution of a biospecies, or rather a biopopulation, is a bound model of the theory of evolution.) Call T a general theory and S_i a set of specific hypotheses and data consistent with T and couched in the language of T. Then the set of consequences of the union of T and S_i is the ith T-model, or $M_i(T)$. And the set of all T-models will be called the *T-family*, or $F(T) = \{M_i(T) \mid 1 \leq i \leq n\}$. Perhaps this is what Sneed (1979) has in mind when he states (wrongly) that a theory is composed by its hard core and the set of its applications.

Finally consider a historical sequence of theories T_k, each proposed at the corresponding time t_k over a period τ, and each dealing with a given domain (or reference class or universe of discourse) D_k. Moreover assume that the intersection $D = \cap D_k$ of these domains is nonempty. Then the *historical sequence of theories about D* over the period τ is $H = \langle T_k \mid t_k \in \tau \;\&\; k \in \mathbb{N} \;\&\; \cap D_k = D \neq \varnothing \rangle$. This seems to elucidate Lakatos's (1970) hazy notion of a research programme. But of course in factual science and technology theories are only part of the evolution of inquiry: there are also empirical investigations.

Broad and deep theories, such as classical mechanics and the theory of evolution, are likely to trigger a landslide of research based on them or inspired by them. Such theories are applied to uncounted new situations and they become paragons to be imitated—in short, they are regarded as paradigms or exemplars (Kuhn, 1962, 1970, 1974). Criticism, whether theoretical or empirical, is unlikely to stop the multiplication of models bound to such generic theories: victorious theories, like victorious politicians, are impatient with critics. Thus hardly anybody listens today to the correct charges of inconsistency made against quantum electrodynamics, or to the objection that "big bang" cosmology rests on flimsy empirical data. The paradigm becomes a paradogma. And, if criticism continues to be unheeded, the paradogma turns into dogma.

Eventually every theory is shown to be defective in some respect or other: in its mathematics, interpretation, accuracy, coverage, or all four. People formerly blinded by the successes of the theory start to admit that it is not perfect, and alterations are tried. Such changes range from minor adjustments to revolutions. In some cases the mathematical formalism is corrected or even thoroughly overhauled, as was recently the case with classical mechanics and classical electrodynamics (Truesdell and Toupin, 1960). In other cases the formalism is kept but the interpretation is altered, i.e. the semantic hypotheses are changed, as is the case with the objective reinterpretation of quantum mechanics (Bunge, 1967c). In still other cases the theory is replaced with another in the same style, as was the case with the change from classical to relativistic mechanics. Finally, in a few cases a whole new style of theorizing is proposed—as when Faraday and Maxwell invented field physics, Darwin and Wallace proposed their theory of evolution, and Lashley and Hebb revived physiological psychology. In such cases learning a new theory is not just adding knowledge but reorganizing from scratch what fund of knowledge one had and adopting a new mode of thinking.

From a semantical point of view theories can be compared as to meaning and truth. According to our semantics (Vol. 1 and 2) meaning is composed of sense and reference. The former is determined by the totality of logical relatives (implicants and implicates) of the construct of interest, whereas the reference of the latter is the class of (actual and potential) entities to which it applies (truly or not). The difference in sense or in reference between two constructs can be taken to equal their symmetric difference. Hence the total *difference in meaning* may be defined as the ordered pair: $\langle$Difference in sense, Difference in reference$\rangle$ (Vol. 2, Ch. 7, Section 3.3). In particular, differences in reference can be ascertained rather easily, particularly if the theory has been axiomatized. For example, field electrodynamics refers not only to the currents dealt with by action at a distance electrodynamics, but also to the fields accompanying such currents.

Another respect in which theories are comparable, and are actually compared, is truth. However, such comparison is far more difficult than that of meaning, for it cannot be performed a priori: in fact it involves the calculation of numerous examples and the corresponding empirical tests. Either operation is likely to be more difficult in the case of a rich theory than in that of a poor one: the number of variables and constants is likely to be greater, their interrelations more complex, and they are bound to be less readily accessible to measurement. Thus for several decades Einstein's theory of gravitation was judged superior to Newton's on the strength of only three "effects" in excess of those predictable by the latter. It took several decades of patient calculation of new solutions of Einstein's field equations, as well as ingenious new observations and experiments, to confirm that first impression. Still, the Newton–Poisson theory is so much simpler to apply, and it is such a good approximation, that it is used in the vast majority of astronomical and geophysical calculations. (This fact may come as a shock to the philosophers who believe that Einstein's theory "overthrew" its predecessor.)

In conclusion, rival theories are compared as to meaning and truth; they are also compared as to computational simplicity. Such comparisons are used as a basis for a rational decision among them. Hence it is mistaken to assert that rival theories built in entirely different styles (or involving "Gestalt switches") are mutually "incommensurable", so that the choice among them cannot be a completely rational act (Kuhn, 1962; Feyerabend, 1975). After all, two theories could not possibly compete with one another unless they yielded different answers to equivalent problems—such as alternative explanations of the existing variety of biospecies. And in order to share some problems the two rival theories must share some concepts and, in particular, they must have some common referents.

2. Change mechanisms

2.1. *Usual Mechanisms*

Russell asked at one point: How comes it that we are able to know so much even though our individual experience is so limited? Chomsky (1975, p. 7) replied in a Platonic vein: "we can know so much because in a sense we already knew it, though the data of sense were necessary to evoke and elicit this knowledge". That is, experience would be nothing but the Socratic midwife that helps the innate knowledge come to light. There is of course no empirical evidence for this hypothesis. And there is plenty of negative evidence from the neurophysiology and psychology of development: it all points to the thesis that we are born ignorant.

The correct answer is that we are able to know so much because in principle there is no limit to what we can learn by sensing, thinking, and doing. And we can learn so much because our brains are plastic and because we inherit a tradition and learn from our contemporaries. As Bernard of Chartres said, we can see farther than the ancients because we are like dwarfs mounted on the shoulders of giants. That is, we do not have to start our inquiries from scratch because we can make use of the knowledge acquired by millions of people over thousands of years—and because we need not learn it all before tackling new problems. We are born ignorant though with the ability and drive to learn, and we are not born into a social vacuum but into a society that keeps some learning tradition or other. If this tradition happens to be one of inquiry rather than one of reverence for myth, we may learn as much as our genetic endowment and social status will permit.

Next to the innate knowledge thesis comes the combinatory view, according to which every new idea is the result of combining old ideas. There is plenty of evidence for this doctrine, which explains why the more we know the more we can find out. Indeed the more items are available for combination the greater the number and variety of combinations. (Thus if the number of items doubles from 5 to 10, the total number of binary combinations rises from 10 to 45; and if the former increases from 10 to 20, the possible binary compounds rise from 45 to 190.) But of course the combinatory view does not explain the origin of the first ideas or the emergence of radically new ideas. How did the Greeks hit on the idea of a proof: what elements did they combine in order to form it? How did Marx come to believe that human history is nothing but the history of class wars: did he merely mix up some ideas of Hegel's and Feuerbach's? The combinatory view holds only a grain of truth. In particular it applies to the original combination of ready-made parts to build a machine, and to the merger of theories—although it does not explain why some people conceive of such combinations whereas most don't. But combinatorics fails to account for the emergence of radically new ideas, such as those of atom, chemical reaction, gene, and social structure, which do not originate in the senses.

How then is the growth of knowledge, when it happens, to be explained? There are four traditional and influential views on the sources or movers of scientific, technological, and humanistic innovation: empiricism, rationalism, psychologism, and sociologism. According to *empiricism*, knowledge grows spontaneously as experience accumulates either by observation or by chance discovery. Theories would come, if at all, at the end of empirical inquiry and only as data summaries or at most to suggest further observations. Since empiricism regards data as irrefutable and as equally valuable, it imagines the growth of knowledge to be cumulative: it would proceed by the steady accumulation of data. No doubt, the production of empirical data is a necessary component of all factual inquiry; but the invention of theories and methods is another. Nor is it reasonable to doubt that there is some accumulation of data; but many data are in need of revision whereas others, even if accurate, cease to be of interest. In short, empiricism gives a one-sided description of the growth of knowledge: it fits at most the empirical side of protoscience and prototechnology.

On the other hand *rationalism* describes the theoretical side of the most advanced stages of science and technology. According to it, knowledge is self-constructed: it grows by inventing and criticizing ideas. Data would be only occasions for, and checks on, theorizing. The main movers of the inquiry process would be internal: the impulses to question, generalize, systematize, and explain. The growth of knowledge would be a sequence of ever better theories. Once in a while a bold new theory appears that kills its old rival. Knowledge advances thus by fits spaced by uninteresting routine activities. No doubt, this view holds an important grain of truth, but it fails to account for the entire process of research. For one thing fact finding is just as important as theorizing and it engages the vast majority of researchers. For another, new methods, particularly if allied to new and powerful instruments, can be just as revolutionary as new theories: think of the galvanometer, the mass spectrograph, the electron microscope, and the microelectrode. Thirdly, not all of the new and better theories kill their older rivals; thus relativistic quantum mechanics has not displaced the nonrelativistic approximation. In sum, rationalism has nothing to say about laboratory and field research, and gives a distorted view of theoretical progress.

Whereas empiricism and rationalism are popular among philosophers, psychologism and sociologism are often embraced by historians of knowledge. According to *psychologism*, knowledge is always the result of inquiry sparked off by either curiosity or need. Consequently the growth of knowledge must be explained exclusively in terms of mental or biological mechanisms. It would be foolish to deny that psychologism does tell part of the story. But obviously it does not explain why inquiry proceeds swiftly in certain periods and morosely in others, or why it flourishes in certain societies and is repressed in others. *Sociologism* makes up for these shortcomings of psychologism. In its moderate version it reminds us that every inquiry proceeds within a social matrix, so that, far from being a purely individual affair, it engages whole communities of inquirers. In its extreme version sociologism states that every bit of knowledge is a reflection of, or a response to, social conditions: that science, technology and the humanities are the products and mirrors of society. We have no quarrel with moderate sociologism even though it focuses on only one of the aspects of the inquiry process. On the other hand we find extreme sociologism unacceptable: after all, it is individuals, not communities, that perceive, think and act. There is no such thing as a collective brain or mind, just as there is no such thing as a Robinson Crusoe scholar. And a thousand mediocre researchers cannot do the work of a single genius—just as the genius would have nowhere to start from were it not for the dedication of uncounted modest researchers.

The most cursory look at the history of any research line in science or technology shows that each of the four views holds a grain of truth but is otherwise either incomplete or somewhat mistaken. There is no unique prime mover of inquiry: in some cases progress is made by performing new observations, measurements, or experiments; at other times by inventing new methods or new designs; at still other times by inventing new concepts, hypotheses or theories; occasionally also by criticizing or reorganizing known ideas; and always by contrasting ideas with facts, plans and artifacts with values, and so on.

Since each of the above partial views on the growth of knowledge is adequate in some respect, they should be consolidated into a fifth view free from the errors or exaggerations of the other four. This view acknowledges the roles of both experience and reason, as well as of the individual brain and its social matrix. This view results from the *biopsychosociological* approach to inquiry favored in the present book, and it suggests an integrated vision of the history of knowledge. Thus an adequate account of Harvey's work on the cardiovascular system must include such items as Harvey's medical experience and his familiarity with Galen and Vesalius; his acquaintance with the incipient mechanical world view, as well as with some mechanical contraptions, in the first place the pump; his ambition to understand the *humani corporis fabrica* described by Vesalius; his inquisitiveness and audacity (which cost him the loss of many patients); and his living in an age of discovery and invention in a society that tolerated both and was on the way to rewarding them. In other cases the intervention of formal organizations, such as universities, academies, churches, and political parties, must not be forgotten.

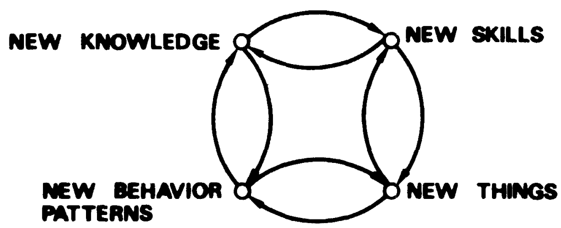


Fig. 13.2. Connections between epistemic and social innovations.

New knowledge sometimes causes social change, and at other times social innovation makes the acquisition of further knowledge possible. Moreover both epistemic and social innovation may result in increasing plant production, which ensues in greater food supply, which makes increase in population possible (not necessary), which causes the growth of cities, which stimulates the enrichment of culture (in particular the invention of new crafts), which favors the evolution of manufacture, trade and services, which in turn offer new opportunities for agricultural development. Likewise, from the 17th century on scientific innovations have sometimes triggered technical changes—such as electrical engineering and industrial chemistry—which have in turn revolutionized production, which has caused cultural and political changes, which in turn have stimulated advances in knowledge. See Figure 13.2. (For the chain-like character of technical and social innovation see Bourke (1978).)

Are such chains necessary? In particular, are discoveries and inventions inevitable? And as a consequence do science and technology have a momentum of their own, i.e. are they unstoppable? All we can say is that every epistemic accomplishment makes further gains in knowledge *possible.* Thus quantum mechanics made it possible to build modern solid state physics, which in turn made it possible—together with electronics—to design high speed computers, which in turn have revolutionized communications, industry and commerce. Without favorable social conditions none of these developments would have happened. They were epistemically possible and socially desirable: this is all we can say.

Yet, it is often stated that simultaneous discoveries and inventions by independent individuals *prove* that they are *necessary*: they seem to be "in the air" and emerge "when the time is ripe". They look as necessary outcomes of earlier work with no need for either genius or chance, or even promise of social reward. So much so that Merton (1973, Ch. 17) holds that what needs explaining is not independent multiple discoveries, for they are the rule, but singletons, which on closer examination should prove to be multiples, as is the case with the rediscoveries of unpublished or poorly circulated work. Merton exhibits an impressive array of data in support of his hypothesis, but does not discuss counterexamples such as Archimedes' statics, Netwon's dynamics, the electrodynamics of Faraday and Maxwell, or Einstein's general relativity. The latter deserves a special comment.

According to his coworker Leopold Infeld, Einstein believed that the special theory of relativity could have been invented by someone else, for a number of people—among them no less than Lorentz and Poincaré—were on the right trail. Not so the general theory. We should believe him, because nobody was interested in gravitation theory at the time: it seemed to pose no problems. Gravitation theory became a hot spot only thanks to Einstein. Even now the observational and experimental "effects" that only Einstein's theory explains and predicts are very small for the most part. Had it not been for Einstein's theory they might have passed unnoticed or, if noticed, they might have been explained in terms of perturbations, experimental errors, or some other *ad hoc* hypotheses.

We should not underrate the roles of genius and of chance. True, an isolated and misunderstood genius will work in vain; still, no scientific community will make great strides unless it contains some exceptionally talented and nonconformist leaders. True, discovery and invention do not spring out of nothing but require some background knowledge; but the researcher may come across some of it by chance—e.g. browsing in the library or meeting a stranger on a plane. In short, new knowledge comes about in ways that are biologically and logically necessary but socially accidental. (See Taton (1955) for the role of chance in scientific discovery.)

What holds for scientific innovation holds also for technical innovation: here too necessity combines with chance, and individual talent and pluck with social opportunity. The importance of the latter is clear from the fact that no industrial or social innovation is the automatic outcome of invention. For invention to fructify in innovation it must be practical and must be understood as useful by those who have the economic or political means to have the blueprints translated into artifacts or social programs. This is why only very few inventions are ever put to use.

To conclude this section: there are as many mechanisms of epistemic change as there are types of epistemic operation. Some workers go out into the field whereas others stay at the laboratory; some make observations and others experiment; some classify and others calculate; some draw blueprints whereas others build theories; some solve problems with the help of existing theories, and others criticize the latter; some delight in specificity, others in pattern; some split and others lump—and so on. The growth of knowledge requires that all of these and more epistemic operations be carried out concurrently in the scientific and technological communities. Breakthroughs may result from any of them. But none will occur unless society tolerates the free search for truth. We shall come back to this theme in Section 4.2.

2.2. *Reduction and Synthesis*

The rise of modern science was accompanied by the substitution of a mechanistic world view for the prevalent organicist one. The new model was the clockwork, not the organism; the new paragon was mechanics, not biology; the new judge was experiment, not dogma. The program of reducing every process to motion triumphed almost everywhere, though not without some philosophical opposition such as Leibniz's. It was not until 1850 that mechanism showed signs of exhaustion, particularly with regard to field physics, evolutionary biology, and social science. Mechanism is now dead. Nowadays only the odd philosopher believes that the world is made up only of particles. We know that there is no particle without field, that there are fields with no associated particles, and that in addition to physical objects there are chemosystems, biosystems, and sociosystems possessing emergent properties. (See D'Abro (1939) for the decline of mechanism in modern physics, and Vol. 4 for the supraphysical levels.)

Mechanism is dead but *physicalism*, or the program of trying to reduce every high level theory to physics, or perhaps to physics and chemistry, is still very much alive. Physicalism is both an ontology and an epistemology. The ontology of physicalism boils down to the thesis that concrete things differ only in complexity; its epistemology, to the thesis that every property of any whole can be understood in terms of the properties of its components. Such reduction would indicate the direction that the advancement of knowledge must take: "the number of principles needed to explain the totality of phenomena becomes ever smaller (...) Consequently, the number of explanatory principles used may serve as a measure of the level of knowledge attained, the highest being that which gets along with the fewest explanatory principles that are not themselves susceptible of further explanation. Thus the ultimate task of knowing is to make this minimum as small as possible" (Schlick, 1925, p. 13). On this view the progress of knowledge would consist in a progressive reduction of the number of ideas accompanied by a ceaseless multiplication of data.

According to the proponents of the reductionist program, only reduction can attain the unity of science, doubtless a desideratum. Thus the social sciences would be reduced to biology, which would reduce to physics: "each science is a special case of the one that precedes it (...) Biology is not 'just' physics and chemistry, but a very limited, very special and profoundly interesting part of them. So with ecology and sociology" (Medawar, 1974, p. 62). "In its simplest form the *unification program* consists in the unification of all branches of science by means of successive microreductions of the dynamic theories of these branches to one unified theory" (Causey, 1977, pp. 4–5).

How has the reductionist unification program fared? The history of science, technology and the humanities over the past century exhibits not only reductions but also mergers, not only unification but also diversification. The number of theories has become ever greater, and the number of branches of knowledge—even within physics and chemistry—has not ceased to grow. But at the same time the connections among theories and entire fields of research has increased. In short, we are heading towards the unity of knowledge through both reduction and synthesis, and in any case through enrichment not simplification, through interaction, not isolation. Thus we hope soon to be able to understand language not by reducing linguistics to physics or by keeping it detached from all the other branches of learning, but by integrating it with neuroscience, psychology, and social science. (More in Vol. 6.)

Why has the program of unification through reduction met with only limited success, so that we must look for a complementary mechanism—namely integration—to achieve the unity of knowledge? One reason is, no doubt, that the world is far more varied than imagined by physicalism: far from being flat it has a complex level structure: See Figure 13.3. A second reason is that a science is not a theory but a system composed of theories, methods and data, so that, even if the successive microreductions of theories were possible, this would not lead to the reduction of all sciences to one. A third reason is that, in most cases, the reduction of one theory to another is only partial, in that it requires the adjunction of premises not contained in the reducing or basic theory (Ch. 10, Section 3.1). Thus, to account for the very existence of a molecule we need not only quantum mechanics but also a set of subsidiary hypotheses; for example, the theory of the hydrogen molecule is not indebted to that of the hydrogen atom. (More in Vol. 6.)

In addition to the above conceptual obstacles to full reduction there is

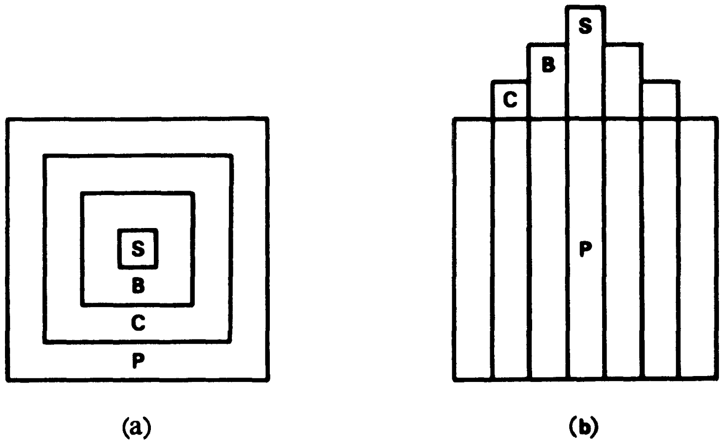


Fig. 13.3. (*a*) Ontological reductionism: reality is a system of Chinese boxes. Higher level things are just special cases of lower levels. (*b*) Ontological emergentism: reality is a telescopic system. Every higher level is composed of things that possess some emergent properties in addition to some of the properties characterizing lower level things. '*P*' stands for the collection of physical things, 'C' for chemosystems, 'B' for biosystems, and 'S' for sociosystems.

one of a practical nature, namely computational difficulty. Take for instance the problem of calculating *ab initio* the possible energy levels of the chromosome of *Escherichia coli*, the best known cell and a comparatively simple one. Since the chromosome consists of a double-stranded DNA molecule with a molecular weight of about 2,800,000,000, in order to account for every single interaction among its subatomic components we would have to set up and solve a wave equation with about 10^8 terms, a task beyond human capacity. It is therefore reasonable to give up such a reductionist attempt and analyze the molecule into manageable modules. In short, reduction cannot always be performed, be it for conceptual or for practical reasons.

Still, the unity of science is a worthy goal, and one that is being attained through both reduction and integration. Why is such unity desirable? First, because the world happens to be one even though immensely rich both quantitatively and qualitatively. This unity suggests that many of the barriers among the various research fields are the product of ignorance or accident, hence unjustified. Such barriers are in our brains not in the external world. Second and consequently, the *rapprochement* and eventual fusion of different fields of inquiry is bound to be fruitful if they study the same objects (the case of ethology and neuroscience) or if one of them studies the components of the systems investigated by the other (the case of psychology and sociology). By getting together with its neighbors, each research field illuminates and checks the other. (Recall the requirement of external consistency or coherence of every body of knowledge with its nearest neighbors: Ch. 12, Section 3.1.)

A fourth reason for wishing to attain or rather strengthen the unity of knowledge is from the limitations of specialization. The division of labor, though necessary to obtain accurate and deep knowledge, may reach a point when it becomes an obstacle to further progress. As Adam Smith noted two centuries ago, narrow specialization blocks technical innovation. The same holds for scientific research: the narrow specialist tends to lose sight of the scientific enterprise as a whole, to the point that he may become an eminent technician ignoring everything that goes on around him. As a result he does not enjoy the benefits of the flow of information and the suggestion of analogies from one field to another, and he is unlikely to be able to cope with new problems requiring an integrated approach. By specializing in excess he has fallen into routine, i.e. the very opposite of innovation. This is one reason that many a scientific, technological and humanistic revolution has been the work of marauders free from the fetters of disciplinary tradition. For example, physiological psychology was founded by two physicists, Helmholtz and Mach; and the Mach bands, which we now attribute to lateral inhibition, were intensively investigated by Békesy, originally a communications engineer. Molecular biology was largely the creation of physicists and chemists: Watson was the only biologist among the fathers of the new discipline. And operations research, or large scale management, resulted from the joint efforts of physicists, mathematicians and engineers—not of administrators or strategists.

We have begun to understand that the current fragmentation of science, technology and the humanities hinders their advancement: that breakthroughs in all fields of inquiry can be obtained only by a judicious combination of specialism with generalism. The model to be imitated is neither the assembly line worker nor the handy man, but instead the specialist with a general background and outlook, who keeps updating and diversifying his knowledge, who keeps abreast of advances in adjoining fields as well as tries his hand at some problems in them, perhaps to the point of changing professions once or twice in life. In short, the best expert is the generalist with specialized knowledge—or, if preferred, the specialist with general interests. It is people of this kind who contribute to creating new research fields or to merging different specialties, and in general help redraw interdisciplinary boundaries. It is also people of this kind whom we seek as our family's doctor: people able to take care of the whole, not by ignoring the findings of the various specialists but by integrating them. If this strategy works to keep us whole, why not use it more often to improve the health of our badly fragmented intellectual culture?

3. Evolution and revolution

3.1. *Paradigm*

Every human being is born into a culture, and every culture includes one or more fields of knowledge, some of which are rather closed belief systems (e.g. religions) whereas others are open research fields (Ch. 2, Section 4.1). Every one of these epistemic fields includes one or more conceptual frameworks. Each such conceptual framework is composed in turn by a general outlook or philosophy, a body of background knowledge, and an accepted thought style which includes certain methods for handling problems of a given type. In mature research fields a few such frameworks dominate at a given moment whereas the competing frameworks are marginal. The dominant conceptual frameworks, or *paragons*, have been variously called *thought styles* (Fleck, 1935) and *paradigms* (Kuhn, 1962). In developing research fields there are no such dominant conceptual frameworks, thought styles, or paradigms. Thus psychology and sociology are still in search of their paradigms, whereas chemistry has its own. (Contrary to widespread opinion, a mature science need not have a single paragon. Thus a chemist may use, in the same research, classical chemical kinetics, a spoke-and-ball model of molecules, and quantum chemistry—three paradigms altogether.)

Those of us who become professional inquirers—scientists, technologists, or humanists—do so by grasping the main features of the paragons in at least one research field, usually in two or more. We learn mostly by studying model cases or *exemplars* (Kuhn's apt term) of problem solving. And we make original contributions when posing or solving new problems within the existing framework, or when introducing some important and viable alterations in the latter. In the first case we conduct, to employ another of Kuhn's favorites, *normal* research. In the second case we engage in *extraordinary* research, which may result in an epistemic breakthrough or even revolution. All this has been known to scientists, technologists, and historians, for quite some time: Kuhn's (1962) modern classic had the merit of bringing it to the fore. What remain problematic are the very notions of a conceptual framework and of a paradigm, and of a revolution in it. Neither of these notions has been elucidated carefully, either by Kuhn or by his followers or critics. (See, e.g. Lakatos and Musgrave, 1970.) Let us apply our tools to clarify those notions.

In Ch. 2, Section 4.1 we characterized a field of inquiry as being formed by a material framework and a conceptual one. The former is constituted by an inquiring community (or rather a community of inquirers), the society supporting (or at least tolerating) it, and the sort of thing that the inquirers study. (In the case of the formal and humanistic disciplines the "things" being studied are conceptual objects, so the expression 'material framework' is somewhat misleading: a better name is wanted.) A *conceptual framework* in any given epistemic field $\mathscr{E}$ was characterized as a septuple $\mathscr{E}_c = \langle G, F, B, P, K, A, M \rangle$, where

G = *general outlook* or philosophical background,
F = *formal background* (logical or mathematical presuppositions),
B = *specific background* (body of borrowed knowledge),
P = *problematics* (collection of problems that may be investigated in $\mathscr{E}$),
K = *fund of knowledge* previously obtained by members of the inquiry community,
A = *objectives* or goals of the inquiry, and
M = *methodics* (collection of methods of $\mathscr{E}$).

Except for the occasional impostor, every member of a research community is engaged in designing or implementing one or more research projects. A *research project* in a research field characterized by a conceptual framework $\mathscr{E}_c = \langle G, F, B, P, K, A, M \rangle$ may be construed as a septuple $\rho = \langle g, f, b, p, k, a, m \rangle$, where every component is a subset of the corresponding component of $\mathscr{E}_c$. Two or more research projects are said to *compete* with one another it they deal with roughly the same problems in different ways, e.g. using different special methods. For example, at one time physicists were divided into corpuscularians and plenists (or continuists); and nowadays sociologists may be classed into holists, individualists, and systemists. There is no competition if the aims are different—e.g. theoretical in one case and practical in the other.

An *exemplar* may be defined as a research project that, (*a*) having proved successful in the past, (*b*) is imitated (taken for a model) for the conduct of further research work. Lakatos (1970) proposed his own notion of a *research programme*, conceived as a sequence of theories in themselves (i.e. dwelling in the platonic realm of ideas), as an "objective reconstruction" of Kuhn's notion of a paradigm. Actually it is an adulteration of it, because for Kuhn (*a*) research is not restricted to theorizing, and (*b*) theories do not hover above social circumstances. The thrust of Kuhn's view is that inquirers do not work in a social vacuum but in a research community. Much the same can be said of the analysis proposed by Stegmüller (1976)—which, moreover, rests on the analysis of theories due to Sneed (1979), criticized in Ch. 9, Section 1.2.

We define *normal research*, be it in science, technology, or the humanities, as the implementation of a research project within an existing conceptual framework and in imitation of some exemplar or paradigm. *Extraordinary research*, on the other hand, is that which may result in a radical innovation in some conceptual framework, such as a substantial change in general outlook, problematics, or methodics. If successful, the new conceptual framework generates new paradigms which inspire a new run of normal research.

Normal research is the bread and butter of investigators, and much of it is exciting. (Even those few revolutionaries who succeed in constructing a new conceptual framework do normal research when they adopt the new paradigm to investigate problems other than those that gave rise to the new framework.) Normal research is often predictable in outline but need not be always so. In fact it sometimes proves our intuitions and expectations wrong, as when Maxwell discovered that the viscosity of a gas is unrelated to its density. (For a good selection of surprises in the course of normal research in contemporary theoretical physics see Peierls (1979).) Moreover most breakthroughs are effected within existing conceptual frameworks. *Examples*: the 18th century work in mathematical analysis (in contrast with the invention of the latter in the previous century); Laplace's work on probability (*vs.* the earlier work); Frege's contribution to logic (*vs.* that of Boole and De Morgan); the axiomatization of set theory (*vs.* its creation by Cantor); modern solid state theory and quantum chemistry (*vs.* quantum mechanics); and even molecular genetics (*vs.* classical genetics) according to Maynard Smith (1972).

Extraordinary research involves a change in thought style and therefore it elicits a reorientation of research. If the outcome is a substantial advance, it constitutes an epistemic revolution—what Bachelard (1938) called a *rupture épistémologique*. Moreover, a successful research project will be said to be an *epistemic revolution*, relative to a given conceptual framework $\mathscr{E}_c$, if and only if (*a*) it involves radical departures in some (not all) of the components of $\mathscr{E}_c$, or (*b*) it opens up a whole new research field (without however cutting ties with all of the existing ones). Clear cases of epistemic revolutions were those effected by Newton, Darwin, Marx, and Cantor. They changed the prevailing modes of thinking in a deep and lasting way.

However, extraordinary research need not be revolutionary: it may ensue in an epistemic counter-revolution, i.e. a partial return to an earlier conceptual framework. (There are never total returns.) More precisely, a research project may be said to be an *epistemic counter-revolution*, relative to a given conceptual framework $\mathscr{E}_c$, if and only if it involves (*a*) giving up, for no good reasons, substantial portions of any of the seven components of $\mathscr{E}_c$, or (*b*) returning to ideas or procedures that had proved to be inadequate in the past and moreover were superseded by $\mathscr{E}_c$. Contemporary cognitivism, with its obsolete mentalism, its disregard for biology, and its lack of concern for experiment, is a rather clear case of counter-revolution; the romantic revolt against positivism (e.g. Feyerabend, 1975) is another. In still other cases extraordinary research results in a mixture of revolution and counter-revolution. An instance of such mixture is behaviorism, with its scrupulous methodics, narrow problematics, and almost total renunciation of theory. Another mixture of revolution and counter-revolution is transformational linguistics, with its formal rigor and exciting new problematics allied to traditional mentalistic psychology and scant concern for empirical test.

Some philosophers, notably Popper (1970), take normal research to be a matter of routine and even dogma, hence full of dangers. This is not so. As we saw a while ago important and surprising breakthroughs can be made within existing conceptual frameworks. Moreover, every epistemic revolution has its roots in some conceptual framework or other. What is true is that normal research is not as glamorous as extraordinary research, and therefore does not often come to the attention of philosophers. And it is also true that clinging to any given conceptual framework, no matter how fruitful it may have been, may end up in dogmatic rigidity: in the refusal to try new theories or proposals, and occasionally also in the refusal to admit defeat in the face of empirical evidence. This point deserves a new paragraph.

Kuhn's most important contributions to methodology are perhaps his ideas that in every science there is a permanent tension between tradition and change, and that negative evidence is treated differently by normal research and by extraordinary research (Kuhn, 1977). The former idea, though obvious, bears hammering, and anyway it is alien to both gradualists (who view the history of knowledge as cumulative) and catastrophists (who focus on revolutions and ignore normal research). The second idea is more original: it is that, whereas normal research attempts to *accommodate* negative evidence to the ruling conceptual framework, extraordinary research uses such anomalies to *undermine* the framework.

The accommodation of negative evidence to the ruling paradigm can be effected, in all honesty, by augmenting the traditional theory with *ad hoc* hypotheses designed to save it, or by proposing new theories conceived in the "spirit" of the prevailing conceptual framework. There is nothing wrong with any of these tactics unless serious anomalies keep cropping up, i.e. unless the prevailing conceptual framework enters in a crisis. In such case it is advisable to try radical alterations. Of course, any such reform projects will be resisted by researchers who have become attached to the old framework. They may become so conservative as to censor the publication of criticism, of new ideas, and even of unfavorable data. But eventually resistance to novelty weakens and the new framework prevails: change is of the essence of science and technology.

3.2. *Revolution*

Human knowledge can advance *gradually*, by *breakthroughs*, or by *revolutions*. Gradual advance consists in accretion or attrition: in gaining some items of information or in discarding others upon being recognized as inadequate. Gradual advance occurs always within some conceptual framework or other. Once in a while a breakthrough occurs within a conceptual framework, namely when an important problem or problem system is solved, so that new problems can be posed within the same framework. And revolutions consist in the emergence of new conceptual frameworks, which replace either old ones or just plain ignorance.

This being so, it is a mistake to opt for either gradualism (favored by empiricism) or catastrophism (favored by both rationalism and irrationalism). The history of knowledge, like that of every other human endeavor and, indeed, any other sector of reality, exhibits not only gradual changes and breakthroughs but also revolutions. The synthesis of gradualism and catastrophism is *evolutionism*. According to this view there is (*a*) *permanence* of some over-arching philosophical principles that propel all objective inquiry (e.g. the theses that reality is lawful and can be known), (*b*) *ceaseless addition and deletion* of data, techniques, hypotheses, and

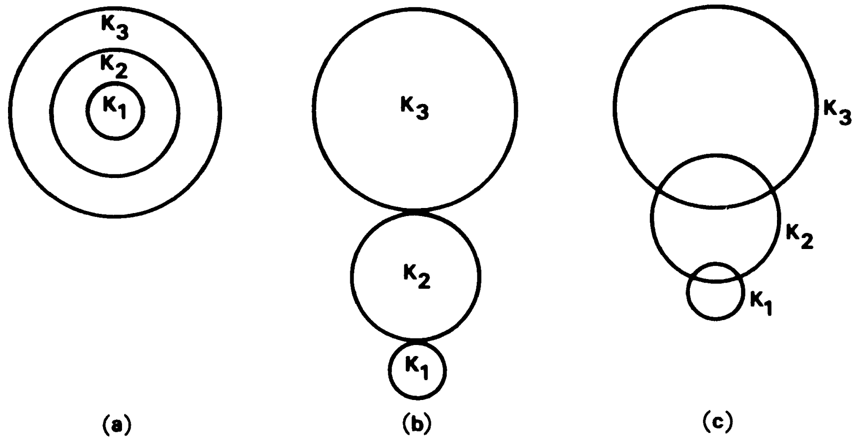


Fig. 13.4. Three models of the evolution of knowledge. (*a*) *Growth*: successive bodies of knowledge include the previous ones. (*b*) *Revolution*: successive bodies of knowledge are mutually incommensurable. (*c*) *Evolution*: successive bodies of knowledge overlap partially.

theories; and (*c*) *occasional revolutions*, using some background knowledge and upsetting other components of it, that result in new conceptual frameworks. See Figure 13.4.

The evolutionary view of the march of knowledge keeps the sound theses of both gradualism and catastrophism while rejecting the false or misleading ones. In particular, evolutionism rejects the fashionable thesis that knowledge advances primarily by replacement, not by addition. This is not always so: mathematical analysis, abstract algebra, genetics, control theory, psychobiology, and economic history—to cite but six scientific revolutions—replaced nothing but ignorance. In these and many other cases there was no rival conceptual framework to be criticized and replaced.

A second, related thesis, is that every epistemic revolution is a response to some crisis. (A research field is said to be in a state of *crisis* if it is stagnant, or is dominated by a single narrow school, or is fragmented into several warrying schools, or is split into many narrow and weakly related specialties, or some of its own accomplishments are threatening its dominant conceptual framework.) True, every epistemic field seems to have passed through a period of crisis, and some fields, such as sociology, seem to be in a permanent state of crisis. However, in some fields breakthroughs and even revolutions may occur without any deep crises. For example the discovery of inconsistencies in mathematical analysis, and later on in set theory, did not throw the mathematical profession into disarray and did not force it to give up any basic principles. The problems were solved with more of the same, i.e. more rigor and further theories.

A third mistaken thesis of catastrophism is that every epistemic revolution razes past accomplishments: that it produces the "collapse" of earlier theories and methods, which are "overthrown" by the victorious rivals. This analogy with politics and warfare is wrong in many cases. Thus Einstein's two relativities, far from demolishing classical physics, constituted its apex: Einstein continued and culminated the work begun by Faraday and Maxwell, as well as by Poisson and Riemann. Furthermore, the news of the demise of classical mechanics and other classical theories is exaggerated, as Mark Twain would say: they are still being worked out and modernized by physicists and mathematicians, and used by engineers. After all, in many cases they help solve problems to an excellent approximation. And in any event even the most drastic revolutions are partial: they alter only some components of the total system of knowledge at any given time.

A fourth mistaken thesis of catastrophism, and a most dangerous one, is that rival frameworks are "incommensurable"—by which "incomparable" seems to be meant, though this is not certain, for romantics are characteristically sloppy. In particular, rival scientific theories would not have a common core of meaning: their senses and reference classes would be entirely disjoint. We have criticized this view in Vol. 1, Ch. 2, Sections 4.2 and 5.1, and Vol. 2, Ch. 7, Section 3.3. Suffice it to note here that, if two theories are deemed to be rival, it is because they share referents and even allow one to pose some common problems. Thus the theory that some mutations are neutral competes with standard genetics, according to which every mutation is either advantageous or, more often, disadvantageous. On the other hand no linguistic theory could possible rival a geological one, for they deal with different domains. In any event it is common practice to compare rival theories and methods, and to choose between them with the help of more or less explicit criteria (Ch. 12). To assert that the choice must be irrational is to distort the picture of actual research and to make an unnecessary concession to irrationalism.

Finally, a fifth and equally dangerous thesis of catastrophism is that each conceptual framework is a sort of mental prison out of which we cannot escape in any rational way: if we do escape it is by an act of faith. Surely this thesis is psychologically and methodologically wrong: scientists and technologists do not operate like mystics or like blind followers of a political ideology, but are often capable of examining their pet theories and methods with a critical eye. They can admit formal or empirical defects and can do something about them, at least tolerate attempts to solve the difficulty. As Popper (1970, p. 56) wrote, "if we try, we can break out of our framework at any time".

In sum, the march of knowledge is smooth in some respects and discontinuous in others. All change, even the most drastic, is partial, not total. (Only crackpots reject the totality of the existing system of knowledge.) And epistemic changes are uneven: at each period some branches of knwledge advance faster than others, thus providing inspiration or even leadership to the less developed ones. Moreover the advancing edge does not fill all the gaps in knowledge: countless pockets of unsolved problems remain behind, some of which are taken up later whereas others are forgotten.

Some of the crucial advances in knowledge have been both philosophical and scientific. In particular, some have consisted in *epistemological revolutions*, or radical changes in the way of gaining knowledge. One of them was introduced by the Ionian philosophers of nature, in particular the atomists: it consisted in replacing myth (in particular religious myth) with naturalistic speculation consistent with (though not suggested by) observation. Another was Archimedes', namely the combination of mathematics with observation and measurement (and possibly also with some experiment) to produce the first mathematical models of things, namely statics and hydrostatics. This process was quickly halted but was resumed sixteen centuries later by Galileo. He replaced the reading of the Scriptures with the "reading the book of Nature" with the help of mathematics, measurement, and experiment. A fourth epistemological revolution was brought about by Newton, who created the first general, exact, and nearly true factual theory in history, and showed how to use mathematics to build theories and formulate problems. A fifth epistemological revolution accompanied the Industrial Revolution and is still going on: it was the birth of science-based technology.

A sixth epistemological revolution started nearly two centuries ago: it consists in studying human nature and human society the same way nature is studied, namely by observation, measurement, experiment, and mathematical modeling. A seventh epistemological revolution was the invention of the theory of evolution by natural selection: from Darwin on not only organisms but also ecosystems, societies, institutions and even molecules are "seen in an evolutionary perspective", i.e. as members of evolutionary trees and as the results of variation and selection. An eighth epistemological revolution is the computer revolution, which makes it possible, among other things, to solve computation problems that are far beyond the reach of the individual brain assisted only by paper and pencil. Finally, an ongoing epistemological revolution is that introduced by physiological psychology, according to which cognition is not an operation of an immaterial mind but a brain process. All these epistemological revolutions shattered long-established traditions, such as those of relying on myth, or on observation unaccompanied by testable theory, or on routine techniques handed down by the craftsman to his apprentice. However, in every case some regulating principles and some bodies of knowledge were preserved. Even the boldest of explorers needs some equipment, although he may have to alter or discard some of it in the course of his travels.

Sometimes three other events are cited as epistemological revolutions: the practice of systematic observation at the beginning of the Modern period, the requirement of operational definitions, and the introduction of considerations on the observer in the quantum theory. Actually neither of these was an epistemological revolution. Man has always observed, and many people have made extensive, meticulous and systematic observations before—though with little result because of lack of good guiding hypotheses. The novelty introduced by the 17th century in this respect was the use of observation (and measurement and experiment) to *check* hypotheses. As for operationism, or the requirement to "define" every concept in terms of instrumental operations, Bridgman (1927, p. 2) regarded it as a guarantee *against* any further revolutions. He thought that we would stop drifting if only we were to anchor every concept to the supposedly solid rock of laboratory operations. Finally, although Bohr, Heisenberg and others did think that they were producing a revolution by invoking the subject every time they interpreted a quantum-theoretical formula in terms of observations, actually they retreated from the ideal of objectivity, so the Copenhagen interpretation they built was *counter*-revolutionary. Fortunately it can be shown (Bunge, 1967c, 1973a) that such interpretation is groundless, for it matches neither the mathematical formalism of the theory nor the actual practice of physicists.

Finally, note that many a revolution in knowledge has been the product neither of new empirical evidence nor of rational criticism. For example, demonology was not discredited by new, adverse empirical evidence, for every bit of experience could be absorbed or "rationalized" by demonology itself. Nor was it discredited by the criticism practised by a few bold and enlightened humanists and theologians, for they had nothing positive to offer instead of a body of belief that could encompass such wide experience.

As Trevor-Roper (1967) has shown, demonology died together with the whole medieval world view, which fell when the new positive philosophy of nature proved more powerful than the old one: it was one of the casualties of the triumph of the new mechanical world view built by Descartes, Galileo, and a few others. Moral: Don't neglect world views, particularly since a theory or practice that is upheld by the prevalent world view is nearly unassailable, whereas one that lacks such support is unlikely to succeed.

4. Limits and Prospects

4.1. *Limits to Inquiry*

Are there any limits to what we may know? Barring the skeptical answer—that we can know nothing—three replies to that question have been given. One is optimistic: there are no bounds on inquiry. Another is pessimistic: there are severe limitations on what we can know, and we may already have reached the end of our tether. The third is realistic: there are limitations, yet boundless progress is possible, though not inevitable.

The optimistic thesis is that of the Enlightenment. (See, e.g. the reception speech of the marquis de Condorcet at the French Academy in 1782.) This thesis is little more than an article of faith suggested by the scientific, technological and humanistic progress since about 1600 in Western Europe. The pessimistic thesis is based on certain beliefs on the human mind and society. One of them is that the human mind is a finite capacity information processor composed of a large but finite number of finite elements, so that sooner or later it will exhaust all their possible combinations. Another is nativism, according to which we never learn anything, all our ideas being innate. A third belief is phenomenalism, or the thesis that we can know only appearances and the relations among them, never reality, let alone any essential properties of it. A fourth belief is that every society, and perhaps even the whole of mankind, is bound to decline, for having exhausted its drive, or for being unable to surmount its inner conflicts, or for having degraded its natural resources.

The optimistic thesis ignores the real constraints on inquiry, such as the upper bound on the velocity of propagation of all signals, whereas the pessimistic thesis exaggerates them. The former does not account for any setbacks, such as those of the Counter Reformation, Nazism, Stalinism, and the current restrictions on research budgets in the USA and elsewhere.

As for the pessimistic thesis, recall the fate of all the gloomy prophecies on the future of inquiry. On the eve of the relativistic and atomic revolutions Lord Kelvin prophesied that the next advances in physics would only add a few decimal figures to the value of the physical constants. (For more recent pronouncements on the near completion of knowledge see Schlegel (1967) and Stent (1978).) At the end of the first World War Oswald Spengler wrote the obituary of the so-called Western civilization, and two decades later Cristopher Cauldwell that of "bourgeois" culture. Both prophecies were followed by unprecedented advances in all fields of research. Better refrain from making prophecies and contribute instead to the pursuit of knowledge.

Given the failure of both the optimistic and the pessimistic theses, let us turn to the realistic one. (For details see Bunge (1978a).) There can be no doubt that the pursuit of knowledge is subject to constraints of various kinds: physical, biological, and social. The former consist in the impossibility, be it in principle or in practice, of obtaining certain informations, e.g. about the documents contained in the archives burned during the Thirty Years War. The biological constraints on our cognitive activities boil down to this: we cannot outfox our own brains—and human brains, though marvelously competent, presumably are not the last evolutionary word. And the social limitations on inquiry stem from the economic, political and cultural matrices of every scientific community. The social pressures on a community of inquirers may become so intense that research may stagnate, decay, or even disappear altogether at least for a while. Such social constraints have nothing to do with the physical or biological ones, hence they can be relaxed. Let us take a closer look at the three types of constraint.

The *physical limits* on inquiry consist essentially in that not every desirable item of information is accessible. There are at least two classes of events of this kind: those about which all information has been lost, and events about which any information will be too late. A clear case of lost information is this: if the "big bang" cosmological theory is true, then the history of the universe before the beginning of the expansion has left hardly any traces, so we cannot possibly learn anything about that segment of eternity. A similar case, on a far more modest scale, is the loss of geophysical—in particular geological—information as a result of physical processes such as the melting of rocks. The destruction of prehistorical evidence and historical records is similar. However, we may take heart on recalling that we keep discovering or inventing new ways of finding and interpreting traces of past events. So, the historical record, though hopelessly incomplete, can be reconstructed in part with some imagination.

In other cases there are events but there is no information to be had about them. For example, we cannot know what is happening right now in some remote corner of our galaxy, let alone in other galaxies. If and when the light signals accompanying some such events reach the present abode of mankind, they will give only a partial picture and one that may find no beholders. (In other words, the events about which you can get information are those lying within your own light cone.) However, what a single observer cannot attain, a few generations of observers, bound by a common scientific tradition, may attain: some of the signals that are now leaving more remote places will reach some of our descendants, so they may be able to know what is going on out there.

As for the future, it may be conjectured with the help of laws and a knowledge of present circumstances. Surely some of our predictions are probabilistic, but this is no limitation if the processes themselves are stochastic, as is the case with atomic, genetic, and social processes. For example, although we cannot predict when a particular radioactive atom will decay, we can predict when the radioactivity of a large aggregate of atoms of a certain kind will be halved.

In sum, scientific inquiry is subject to certain physical constraints. These constraints limit the amount of information that can be won, but they need not slow down the pace of scientific progress. We can know more and more about certain things while ignoring everything about others. We need not worry about all we shall never know provided we keep on finding out some of the infinitely many things that can be known.

Let us now turn to the *biological constraints* on inquiry. Ours is the most plastic of all known brains, and so the one capable of learning the most. As society becomes more complex and culture richer, our brains are subjected to an increasing amount and variety of stimuli, which elicit an ever increasing number and variety of synaptic connections. The development of a child's brain is probably far swifter nowadays than it was one century ago. And our readiness to expect novelty and to seek it is certainly greater than that of our ancestors, most of whom resisted novelty. Surely there is more to be learned today, but we do not have to learn everything our ancestors knew. We can learn to pilot airplanes without first having to learn to drive oxcarts. The same holds for science and technology learning: most of the theories and data a student is supposed to learn are comparatively new. Progress makes up for the limited human life span.

Much more serious a limitation on the human cognitive ability would seem to be our limited channel and storage capacity—to say it in computerese. But these are not really disadvantages: if we were capable of admitting all stimuli, we would be unable to concentrate; and if we were capable of recalling every single experience we would hardly be able to conceive of new ideas. Blessed be our sensory inhibitions and memory limitations, for they allow us better to create.

Anyway, the brain-computer analogy is superficial. Human brains are biosystems not artifacts: they develop and, collectively, they evolve. Moreover, unlike other organs, the human associative cortex is highly plastic: it does not deliver the same outputs every time it is presented with the same inputs, and it is in permanent activity. Furthermore it is possible that the various mental functions are performed not by fixed neuronal systems but by itinerant ones formed afresh every time a new task arises (Bindra, 1976). If this is so, even though the number of neurons is finite, that of neuronal systems capable of performing mental functions is practically infinite.

The real limitation of the human brain lies elsewhere and it is not to be found out by comparison with computers. It derives from the inability of our associative cortex to function properly unless it is connected to the brain stem, the hypothalamus, the endocrine system, the peripheral sensors, and, indeed, the whole rest of the body. It is these extracortical systems that keep the associative cortex awake and active—and which at the same time cause the nonrational streaks of human thought. We cannot think unless we are alert and motivated, but if we are alert we cannot avoid some distraction; and if we are motivated we cannot keep totally cool. In sum, pure reason is biologically impossible.

However, we often do manage to think rationally and to make rational decisions, and this because we are not isolated. Sensory deprivation leads to hallucination, and social deprivation to mystical visions. I cannot think rationally all the time, nor can my partner, let alone my rival; but they correct me when I lapse, and all three together manage to set up—unwittingly to be sure—a self-correcting system within which we can be sane and productive, outside of which insane or barren. Human knowledge, particularly of the scientific or technological kind, is produced by individuals in society, and ought therefore to be always public property.

In short, the biological constraints on cognition, though real, are less formidable than they appear at first blush. First, because the human brain is marvelously plastic and capable of spinning out new ideas without end.

Second, because the scientist, technologist or humanist need not, nay must not, rely exclusively on himself. He counts on all his peers, this being why he writes *we* know where the artist writes *I* know. His personal limitations can be overcome by learning from the dead and cooperating with the living. He works, in short, in an inquiring community held together by an information network. (Recall Ch. 3.) Which brings us to the third and last type of constraint on inquiry.

Every inquiring community is a subsystem of some culture or other, which in turn is a subsystem of some society, in turn a component of the international system. The culture of a society is only one of its three artificial subsystems, the other two being its economy and its polity. Each of these subsystems interacts strongly with the other two. (cf. Vol. 4, Ch. 5.) In particular culture—and so science, technology, and the humanities—is subject to intense economic and political pressures. So, far from being autonomous, science, technology and the humanities thrive or wither along with society.

A peasant economy cannot support a space research program; a totalitarian state does not tolerate free inquiry into political problems; and a religion-oriented culture does not encourage studies on the origin of life, biological evolution, the physiology of thinking, or the socioeconomic roots of religion. It does not follow that an industrialized society, with a democratic polity and a lay culture, will necessarily support scientific and humanistic research. It will, provided the prevailing ideology happens to be favorable to inquiry—otherwise not. Nor does it follow that an underdeveloped economy, allied to an authoritarian polity and a backward culture, will necessarily ban all inquiry. It may support some research, sometimes even at great sacrifice, as long as its ideology is friendly towards learning, or for believing mistakenly that basic research delivers technology. In sum, if we wish to understand the social control mechanisms of inquiry we must reckon with ideology, for ideology shapes public opinion, which in turn determines cultural policies, which regulate the only two contributions a society can make to its inquiring communities, namely human and material resources. (Tradition can be borrowed up to a point.)

Science and technology are so much taken for granted nowadays that we tend to forget how new they are and how exceptional were the circumstances of their emergence. Thus it cannot be a coincidence that science and mathematics, as well as philosophy, were invented in Greece during the 5th century B.C. Other civilizations had been more advanced in other respects and they shared a number of basic social and economic traits with the Greek city states, among them slavery. The one trait they did not share was political democracy, which was genuine as far as the Greek citizens were concerned. And political democracy involved civil rights, among them the rights to inquire, question, dissent and argue about everything except religion. Apparently nothing more than this freedom and some leisure was needed for mathematics, science and philosophy to flourish, as inquiry comes spontaneously to man, and social conditions can only stimulate or inhibit it. Technology is another matter: it also requires an industry capable of using it, and this was nonexistent in Antiquity. So, technology proper, i.e. technical knowledge based on science, could not emerge until the mid-seventeenth century. Since then science, technology and industry have been supporting one another, constituting a unique system unknown in previous times. We shall come back to this in Ch. 14, Sections 2.2 and 3.1.

To sum up, although there are physical and biological limits to what men can know, they need not impede scientific, technological or humanistic progress. The collection of knowable facts is a nondenumerable subset of the total set of facts, and there is no limit on the variety of plastic neuronal systems capable of handling those facts. The really important constraints upon the evolution of science, technology, and the humanities, are social—economic, political, and ideological. Hence the importance, for all inquiring communities, of a sociology and politology of knowledge capable of revealing the external stimuli and inhibitions to inquiry. We shall return to this point in Ch. 15.

4.2. *Prospects of Inquiry*

What are the prospects of inquiry in science, technology, and the humanities inspired in science? That is, what is the future of our science-oriented culture? Let us not ask *what* discoveries and inventions will be made, for to do so would be to indulge in fiction—except of course in the relatively uninteresting cases where the outcome of current research is predictable. Let us ask instead the far more radical, important, and topical question *whether* research will be conducted at all in the near future.

Most people seem to be optimistic about the prospects of inquiry, and they offer the following reasons for such attitude. One is that modern civilization needs science, technology, and even the humanities. Indeed, there is no technological advance without research, and much of technological research is indebted to science. It is a question not just of continuing to borrow from past achievements, for many of them become obsolete and also because technology and the economy are constantly running up against new problems calling for new theories, methods, and data. So, if modern civilization—i.e. industrial society in any of its versions—is to go on, research must go ahead. This is the *argument from the practical value* of science. It is persuasive provided one accepts the antecedent, i.e. the thesis that industrial society is worth preserving—not so otherwise.

A second reason given is this. Modern society is becoming increasingly a service society: it needs physicians and dentists, veterinarians and agronomists, electricians and electronics experts, accountants and programmers, and so on and so forth, in increasing numbers relative to workmen and farmers. And those technicians cannot be properly trained, or contribute to the improvement of their techniques, unless they master the fundamentals of science. This is the *argument from services*, which is really complementary to the previous argument. Again, it holds water provided one cares for the service society.

A third argument is that science and technology have become no less than the left and right cerebral hemispheres of modern culture. They are intrinsically valuable in addition to having a practical value. Our schools teach them, and the public demands scientific and technological literature as well as gadgets. This is the *argument from the cultural value* of technology. It is correct but it has little practical value, for it will not persuade anyone who is mainly interested in fast profit or power. Besides, science and technology, unlike their industrial outputs, are still elitist. Only a very small fraction of the total population is ever given the opportunity of experiencing the thrill of discovery or invention. Most of us, even most philosophers of science and technology, have never done any research in either science or technology, and so have as much experience of them as the deaf have of music or the blind of painting. Worse, the antiscientific and pseudoscientific publications have a far wider readership than the scientific and parascientific ones.

Finally there is the *argument from politics*, according to which scientists, technologists, and their numerous auxiliaries—technicians, librarians, etc.—have become a substantial sector of modern society, so their needs and aspirations can no longer be safely ignored by the politicians. This argument carries little weight because most scientists and technologists are politically naive and passive: they have no powerful lobby in any congress.

All four arguments come down to this: Modern civilization, West and East, involves science and technology; hence, if it is to continue, science and technology have got to have a future. Unassailable. But who assures us that modern civilization will continue? There is no guarantee: no law of continuity, let alone progress. On the contrary, there are many disturbing signs of decline. Let us review them quickly.

A handful of recent events has badly shaken our faith in progress, particularly in the continued march forward of knowledge. One is the accelerated pace of the arms race, which employs roughly half of the best engineers of the world, wastes nearly two thousand million dollars per day, and is likely to end up in the ultimate democracy of ashes. The second is the rapid depletion of non-renewable natural resources amidst the nearly total indifference of most governments. The third is the near-sighted science and humanities policy adopted by a number of governments, that have shifted the bulk of public support from basic to applied science and technology. The fourth is the spread of religious fundamentalism and authoritarian ideologies, their attack on basic scientific theories such as the theory of evolution, and their attempt to restrict the right to know. The fifth is the revolt against reason, and in particular against science and technology, that is spreading among well-intentioned but ignorant young people who, mistakenly, see in inquiry an enemy of the people.

For all of these reasons we should not take science and technology for granted. Instead, we should ponder seriously the initial question, which might not have been asked in 1780, 1880, or 1980: Do science and technology, and indeed modern civilization, have a future at all? It is not that science and technology are in the grips of a conceptual or methodological crisis. On the contrary, the prospects for further sensational (big and unexpected) breakthroughs in every field of research are brighter than ever. But science and technology are the work of human beings who need a favorable environment. If such an environment is not provided, the coming generations, if any, may lose interest in science and technology. If this happens our offspring are in for a new Dark Age. We may already have entered a period of scientific and technological decline. The decline, if there is one, may be temporary or terminal. It is up to us which it is to be. There are no iron or even tin laws of scientific and technological development. Science and technology will become what scientists, technologists and their fellow human beings choose.

5. Concluding remarks

Knowledge cannot be stored permanently, either in books or in a spiritual world, because it ages. Sooner or later every bit of knowledge becomes stale or, if still valuable, it is bound to be incorporated into new bodies of knowledge, where it acquires a new value. Research takes care of such epistemic metabolism.

Knowledge may pass away, inquiry need not—that is, provided there is the will to find out. Science, technology and the modern humanities are characterized not so much by the amount of knowledge they have amassed, as by their research pace. A swift pace, more than any other characteristic, is what separates them from other forms of knowledge.

Knowledge evolves both gradually and by fits. It evolves as research of all kinds—mathematical, experimental, historical, etc.—yields new results. Some of the results of research fill a predesigned mold—conceptual framework—whereas others break it. In either case good reasons are given for any departure from tradition. And no such departure is a total break with the past: there is discontinuity in some respects but continuity in others.

In principle the progress of knowledge is boundless, but in practice there are restrictions. For example, in principle every single fossil in North America could be dug out; but such digging on a continental scale is practically impossible. In general, there is a gap between what can be known in principle and what can be known in practice.

It is likely that what remains to be known is infinitely greater than what we already know. Yet we do possess already infinite knowledge of sorts. Thus we know how to construct infinite sets, such as continuous functions, and we know law statements that cover infinitely many states, such as the formulas for the propagation of a wave and for the energy levels of an atom.

The enthusiasts as well as the enemies of research and development tend to believe that science and technology cannot be stopped: that, once launched, they are bound to move forward. Taken literally this belief is absurd, for it is inquirers who discover and invent, compute and design, criticize and evaluate—and researchers may cease to exist either for lack of motivation or as a result of a nuclear holocaust. After all, inquiry has declined dramatically several times before and seems to be declining right now: there is nothing necessary about the progress of knowledge. What is true is that creative scientists and technologists, as well as humanists, are mainly inner-directed: they cannot help posing problems, making observations, framing hypotheses, or drawing blueprints—that is, provided they are tolerated by society. And this is precisely the point: society, or rather its rulers, may not cherish research, at least of certain kinds, for regarding it as dispensable luxury or as subversive.

In sum, mutability is one of the trademarks of scientific and technological knowledge. But it is not the only one, and in any case there must be differences between science and technology. Let us investigate them.

CHAPTER 14

KINDS OF KNOWLEDGE

What kinds of knowledge are there, what characterizes every one of them, and how are they related? These are some of the problems to be investigated in the present chapter. They are not very old: they were born with secular thinking, which in the West usually dates back to the Ionian philosophers of nature, and they were not investigated vigorously until the Renaissance. Thus the distinction between mathematics and the other sciences is attributed to Leibniz, and that between philosophy and the sciences to Kant. And the distinction among the various sciences was not drawn until the beginning of the 19th century, when the first modern universities were organized.

It is common to distinguish the following ten knowledge genera: ordinary knowledge, technical prescientific knowledge (technics), basic (formal and factual) science, applied science, technology, the humanities, the arts, ideologies, religions, and pseudoscience. However, the criteria for such distinctions are seldom explicit and clear. Nor is there consensus about such criteria. Thus according to some, the fields of knowledge differ by their subject matter; to others, by their method; to still others, by the kind of service they render to society; and to some, by the way their workers are trained and communicate among themselves. Finally, some scholars hold that such distinctions are conventional—human knowledge being in one piece—or pointless, human knowledge being non-existing.

We shall propose explicit definitions of the genera of specialized knowledge. However, we shall also note their interrelations for, in one sense, human knowledge is in one piece. The interrelations are of two kinds: systematic and heuristic. Two fields of knowledge can be said to be *systematically* related to one another if at least one of them borrows some knowledge from the other. Thus biology and chemistry are systematically related to each other, and so are economics and mathematics. And two fields of knowledge will be said to be *heuristically* related to one another if work in one of them is helped or hindered by the other. Thus religion discourages scientific inquiry, particularly into its own dogmas, but it may occasionally help some investigators conceive fruitful speculations concerning unobservable entities or properties. (However, religion is primarily a matter of faith not knowledge, so it is not really a field of knowledge.)

Since every field of knowledge is related both systematically and heuristically to some other field, one can rightfully speak of the unity of human knowledge amidst its amazing diversity. In fact we shall speak of the *system* of human knowledge—both as a system of knowing subjects and as a conceptual system. Moreover, since the production of knowledge is intimately related to the production, use and exchange of other artifacts, we may also speak of the *total system* of production and exchange of goods and services of all kinds, from theories, blueprints and works of art to cultivars, machines and educational services.

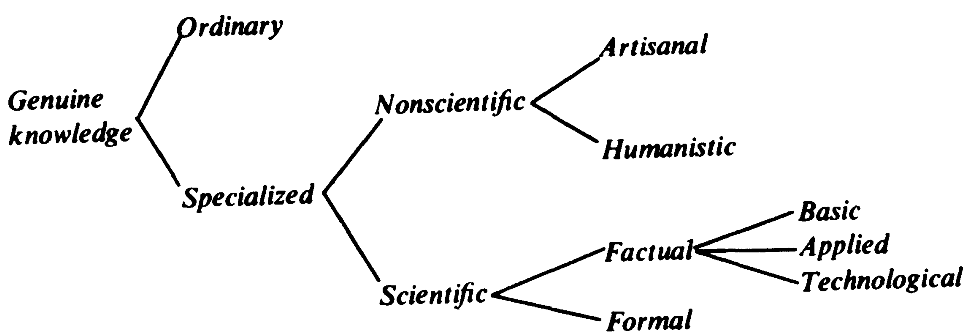
1. Fields of knowledge

1.1. *Knowledge Genera*

There are many ways of classing items, in particular bits of knowledge: as many as criteria (or equivalence relations) are adopted. (See Ch. 9, Section 1.1.) Indeed one may divide knowledge into genuine and bogus, theoretical and practical, scientific and nonscientific, factual and nonfactual, and so on. Which classing is the most convenient depends upon our goal.

From an epistemological viewpoint the first division, between genuine and bogus knowledge, is the most important of all. We define *genuine knowledge* as knowledge that is at least partially true, and *bogus knowledge* as thoroughly false knowledge—false either because it refers to nonexistents or because it represents existents in an utterly false manner. However, the frontiers between genuine and bogus knowledge are not fixed. In particular, some genuine knowledge may come to be recognized as bogus. But the converse process is exceptional: we have here the analog of Gresham's law for currency. More on bogus or illusory knowledge in Section 4.

From an epistemological point of view the following classification of kinds of genuine knowledge is of interest:



This schema must be accompanied by a number of qualifications. First, the bits of knowledge that compose every genus or sector, such as ordinary knowledge, vary with place and time. Thus, the basics of arithmetic, physics and technology have become common knowledge throughout the industrialized world, but are still specialized in the underdeveloped nations. Second, not all ordinary knowledge is widespread although it is in principle accessible to everyone. In fact some of it is expressed by works of art such as novels, dramas, movies, paintings, and sculptures. (Shakespeare and Molière, Balzac and Tolstoy, Chaplin and Costa Gavras have taught us more about ourselves than the whole of behaviorist psychology put together. On the other hand music, ballet and non-representational painting are hardly types of knowledge.) Third, specialized knowledge is not all special or narrow: some of it, such as mathematics, epistemology and ontology, is extremely general. Fourth, what is scientific today may have started by being nonscientific; psychology and social history are cases in point. Fifth, what starts as basic may end up by becoming applied or vice versa. Thus solid state physics, once a branch of basic physics, is now part of applied physics. Sixth, the usual classification of disciplines need not fit the above schema: indeed some of them—e.g. forestry, medicine and psychology—have basic, applied, and technological aspects. We shall deal with such interesting mongrels in Section 3.2.

What about value theory, aesthetics, ethics, and ideology: do they qualify as types of knowledge, and if so which place do they occupy in our schema? *Prima facie* neither does, because they are all concerned with valuations, and these are not cognitive acts although they may be performed in the light of knowledge. However, not every statement in value theory, aesthetics, ethics or ideology is a value judgment: there are also strictly descriptive or explanatory statements in those fields. In particular, a value theory is supposed to encapsulate our knowledge about values, and an ethical theory is supposed to account for the nature and role of behavior codes. So, those fields do have a cognitive content.

Moreover, not every value judgment is unjustifiable or irrational. Thus we may have objective grounds for preferring one kind of food to another (for a given end and in given circumstances), or one method of calculation or measurement to another, or one social program to another (or to none). In particular ideology, though committed to values, may be firmly based on knowledge. Thus suppose that most people in a given community live below the poverty line (first factual premise). Suppose also that we discover that this situation is potentially explosive (second factual premise). It

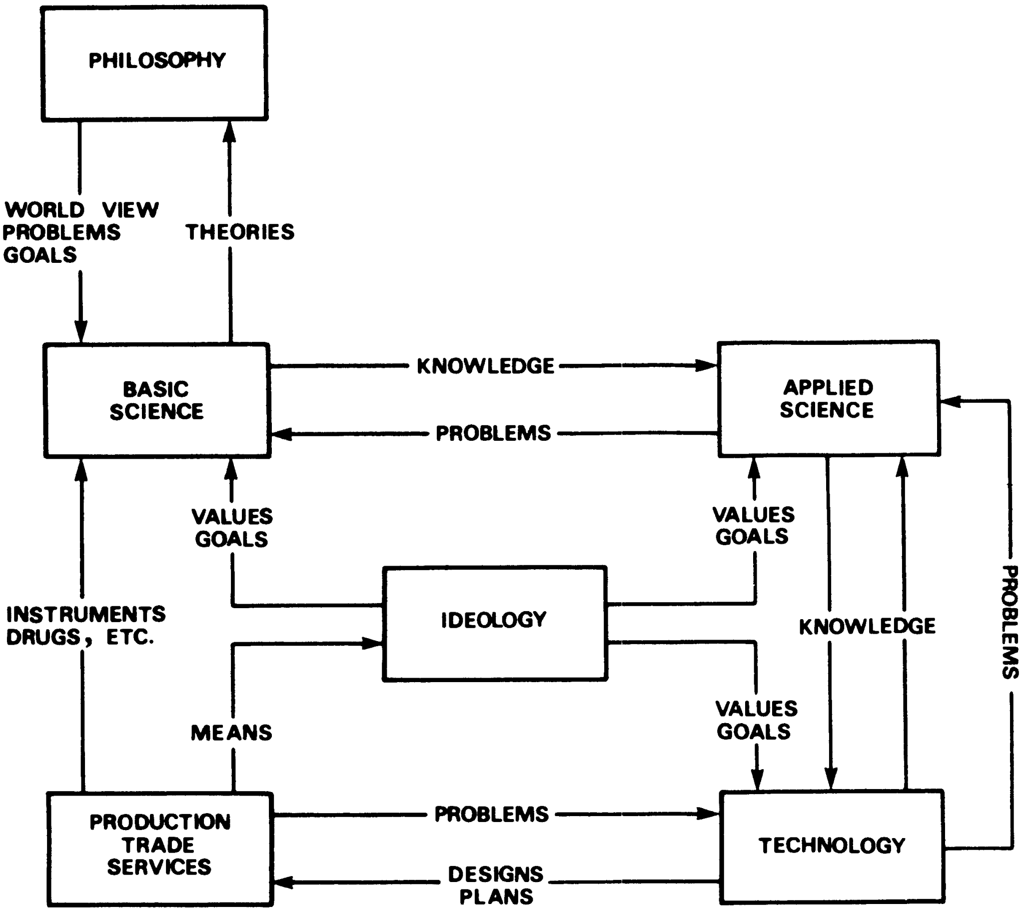


Fig. 14.1. The system of production and exchange of scientific and technological knowledge, artifacts and services in an industrialized society.

follows that it is desirable both practically and morally to set up a social program to remedy the situation—e.g. to change the distribution of wealth, or introduce new jobs, or offer technical training, or what have you. Such mixture of knowledge and valuation does not differ in principle from the decision to replace one method of measurement with a second one given that the latter is more precise, and that greater accuracy is desirable. In sum, although the value disciplines and the ideologies are not properly included in knowledge they overlap with it. Therefore they must be shown in any diagram representing the system of knowledge: Figure 14.1.

1.2. *Research Field*

In Ch. 2, Section 4.1 we elucidated the notion of an epistemic field, and distinguished two kinds of epistemic fields: the belief system and the field of inquiry or research field. We shall now take a closer look at the latter. We start by defining a *family of research fields* as a set every member $\mathscr{R}$ of which is representable by a 10-tuple

$$\mathscr{R} = \langle C, S, D, G, F, B, P, K, A, M \rangle,$$

where, at any given moment,

(i) C, the *research community* of $\mathscr{R}$, is a system composed of persons who have received a specialized training, hold strong information links amongst them, and initiate or continue a tradition of inquiry;

(ii) S is the *society* (complete with its culture, economy, and polity) that hosts C and encourages or at least tolerates the activities of the components of C;

(iii) D, the *domain* or *universe of discourse* of $\mathscr{R}$, is the collection of objects of study of $\mathscr{R}$;

(iv) G, the *general outlook* or *philosophical background* of $\mathscr{R}$, is composed of ontological theses (concerning the nature of the D's), epistemological principles (about the nature of inquiry into the D's), and ethical rules (about the proper behavior of the inquirers in C);

(v) F, the *formal background* of $\mathscr{R}$, is the collection of logical and mathematical theories that are or can be used by members of C, in studying the D's;

(vi) B, the *specific background* of $\mathscr{R}$, is the collection of items of knowledge obtained in other fields of inquiry and utilizable by the C's in studying D's;

(vii) P, the *problematics* of $\mathscr{R}$, is the collection of problems (actual or potential) that can be investigated by the C's;

(viii) K, the *fund of knowledge* of $\mathscr{R}$, is the collection of items of knowledge utilized by C and obtained by it at previous times;

(ix) A is the set of *aims* or *goals* of the members of C with regard to their study of D's;

(x) M, the *methodics* (usually misnamed 'methodology') of $\mathscr{R}$, is the collection of methods utilizable by members of C in their study of D's.

(xi) There is at least one other (*contiguous*) research field $\mathscr{R}'$ in the same family of fields of inquiry, such that (*a*) $\mathscr{R}$ *and* $\mathscr{R}'$ share some items in their general outlooks, formal backgrounds, specific backgrounds, funds of knowledge, aims, and methodics; (*b*) either the domain of one of the two fields, $\mathscr{R}$ and $\mathscr{R}'$, is included in that of the other, or each member of the domain of one of the fields is a component of a system in the domain of the other.

(xii) The membership of every one of the last eight components of $\mathscr{R}$ *changes*, however slowly, *as a result of inquiry* in the same field or in related fields.

Note the following points. First, only the first two components of the ten-tuple, i.e. C and S, are individuals and, more precisely, concrete systems; the remaining components are collections (variable in time). The point of listing C and S explicitly is to remind ourselves that knowledge is not self-existing but an activity performed by real people in a concrete social environment. So much so that a drastic change in either C (such as the appearance of a couple of geniuses or of a new mode of communication) or S (such as a sudden liberalization or a dictatorship) may alter radically the entire field of inquiry. Of course C is a subsystem of S and therefore a description of C should be included in any thorough description of S. However, C should be listed separately because it may emerge or become extinct even if the society hosting it goes on roughly as before at least for a while.

Second, listing the *domain* D or collection of objects of study of a research field is necessary to characterize it—*pace* Kuhn (1962)—because its problematics, hence its methodics, depends largely upon the nature of its D's. Thus $\mathscr{R}$ is a *factual* field only if some of its D's are concrete things. However, it is not possible to identify the fields of inquiry exclusively by their subject matter, because a number of disciplines may share the same referents: think of human genetics and physiology, anthropology and history. So, mention of D, though necessary, is insufficient for an unambiguous characterization of $\mathscr{R}$: it must be supplemented with a characterization of the problematics, the methodics, and the aims.

Third, the *general outlook* G is essential for, as Myrdal (1969, p. 51) said, before there can be a view there must be a viewpoint. Thus the religious viewpoint is composed of an ontology, an epistemology and an ethics radically different from the scientific viewpoint. (More in Section 4.2.)

Fourth, the *formal background* F is the collection of logical and mathematical theories that members of C may use, whether explicitly or tacitly. True, most scientists and technologists care little for formal logic and for the theorems proving the rules of calculation they employ. Still, once in a while they are forced to resort to them, and they are always supposed to admit corrections to their invalid reasonings and calculations.

Fifth, explicit mention of the *specific background* B of a research field is methodologically and practically important, for it places the burden of some proofs on a different research community and it indicates how well integrated the given research field is with the other members of the same family. Thus a chemist normally relies on results obtained by physicists. On the other hand pseudoscientists make no use of knowledge obtained in other epistemic fields.

Sixth, the *problematics P* of a research field is one of its characteristics, for two different research fields may share everything but their problematics. For example, whereas chemical statics studies the properties of molecules, chemical kinetics and dynamics study the formation and breakdown of molecules (i.e. chemical reactions).

Seventh, the *fund of knowledge K* of a research field is not so much the sum total of results obtained in it to date, as the current stock, i.e. the collection of data, hypotheses, theories, and methods that are still available for further research. K is thus the state of $\mathscr{R}$ at the given time.

Eighth, the *aims or goals A* are equally characteristic of a research field, for one and the same thing may be studied with different aims and hence methods—e.g. to know it better, or else for gaining control over it.

Ninth, the *methodics M* must be listed because a problem may be investigated with radically different methods—e.g. theoretically or empirically. A research field is *theoretical* if its methodics includes only conceptual procedures, and *factual* if it also includes empirical procedures.

Tenth, the existence of *contiguous* research fields (condition xi above) is necessary for a research field, whereas belief systems and pseudosciences are characteristically isolated from other epistemic fields. For example, neither theology nor psychoanalysis have intimate relations with any other epistemic fields.

Eleventh, note that condition (xii) stipulates not only that a research field should change, but also that it should do so *as a result of inquiry* not of mere controversy or in compliance with governmental or ecclesiastical demands.

Let us examine three characteristically modern fields of inquiry: basic science, applied science, and technology.

2. Science and Technology

2.1. *Basic Science*

Most philosophers of science believe that science is characterized by a single peculiar trait. In fact the following views on science can be found in the literature:

(*a*) The *consensus* view holds that, whereas theology, pseudoscience and the humanities are rife with controversy, science is uncontroversial or at least aims at "a consensus of rational opinion over the widest possible field" (Ziman, 1979, p. 3). This characterization is inadequate, for (*a*) every field of active basic research teems with controversy, and (*b*) consensus is at most an uncertain indicator of truth, not a goal of research. What is true is that science has means for settling controversies in the long run. It is also true that applied science is far less controversial than basic science, if only because it consists in an application of the firmest results of basic science. Still, the aim of applied research is possibly useful knowledge rather than consensus, which is at best a byproduct.

(*b*) The *empirical content* doctrine maintains that, unlike other types of inquiry, science accepts only empirical data and inductive syntheses thereof, never speculation. Though still popular in the science textbooks, this view was refuted long ago by the very emergence of theoretical physics, theoretical chemistry, theoretical biology, and other fields teeming with concepts representing non-observable entities or properties. Several philosophers, notably Whewell (1847) and Popper (1959), have discredited that doctrine.

(*c*) The *success* view claims that for science only success counts—or, as James put it, that truth is "what it is better for us to believe". This pragmatist view may fit technology, not basic science, which is after truth, depth, and systemicity.

(*d*) The *formalist* doctrine holds that a body of knowledge is scientific only when it has been thoroughly mathematized. This characterization is too wide on the one hand, for it accepts as scientific much stuff that is not—e.g. beautiful mathematical theories of the nonexisting free market in equilibrium. And on the other hand it is too narrow, for it disqualifies experimental science as well as young science, which is often premathematical. The truth is that science cannot advance beyond a certain point without using mathematics.

(*e*) *Refutationism* maintains that the mark of science is refutability, i.e. its dealing exclusively with hypotheses that are conceivably refutable (Popper, 1959). This characterization ignores fact finding research, be it in the laboratory or in the field. And it admits as scientific all refuted beliefs, such as astrology and creationism, whereas it rejects the most general scientific theories, for not being refutable without further ado (i.e. without the addition of subsidiary assumptions).

(*f*) *Methodism* holds that the sole requisite for science is adopting the scientific method. But if every application of the scientific method were indeed a piece of scientific research, then testing the mental ability of atoms, trying to catch ghosts with special nets, or investigating people who claim to be able to read printed matter stuffed in their ears, would pass for scientific research provided certain precautions were observed.

(*g*) *Sociologism* claims that science is what scientists do. This stipulation is inadequate for occasionally scientists indulge in nonscientific activities, such as mindless data collection, untestable speculation, or even straight pseudoscience.

Since none of the above popular characterizations of science work, we must formulate our own. We shall do it by specifying the general definition of a family of research fields proposed in Section 1.2. A *family of scientific research fields* is a set every member $\mathscr{R}$ of which is representable by a 10-tuple

$$\mathscr{R} = \langle C, S, D, G, F, B, P, K, A, M \rangle,$$

where, at any given moment,

(i) the *research community* C has the same general characteristics as those of any other research field;

(ii) the *host society* S of C has the same general characteristics as those of any other research field;

(iii) the *domain* D of $\mathscr{R}$ is composed exclusively of (certified or putatively) *real entities* (rather than, say, freely floating ideas) past, present or future;

(iv) the *general outlook* or *philosophical background* G of $\mathscr{R}$ consists of: (*a*) an *ontology of changing things* (rather than, say, one of ghostly or unchanging entities); (*b*) a *realistic epistemology* (instead of, say, an idealistic or a conventionalist one), and (*c*) the ethos of the *free search for truth, depth, and system* (rather than, say, the ethos of faith or that of the bound quest for utility, profit, power or consensus);

(v) the *formal background* F of $\mathscr{R}$ is a collection of *up to date* logical and mathematical theories (rather than being empty or formed by obsolete formal theories);

(vi) the *specific background* B of $\mathscr{R}$ is a collection of *up to date and reasonably well confirmed* (yet corrigible) data, hypotheses and theories, and of reasonably effective research methods, obtained in other research fields relevant to $\mathscr{R}$;

(vii) the *problematics* P of $\mathscr{R}$ consists exclusively of *cognitive problems* concerning the nature (in particular the laws) of the members of D, as well as problems concerning other components of $\mathscr{R}$;

(viii) the *fund of knowledge* K of $\mathscr{R}$ is a collection *of up to date and testable* (though not final) theories, hypotheses, and data compatible with those in B, and obtained by members of C at previous times;

(ix) the *aims* A of the members of C include *discovering or using the laws* of the D's, systematizing (into theories) hypotheses about D's, and refining methods in M;

(x) the *methodics* M of $\mathscr{R}$ consist exclusively of *scrutable* (checkable, analyzable, criticizable) and *justifiable* (explainable) procedures, in the first place the scientific method;

(xi) there is at least one other *contiguous* scientific research field with the general characteristics noted with reference to research fields in general;

(xii) the membership of every one of the last eight components of $\mathscr{R}$ *changes*, however slowly at times, *as a result of scientific research* in the same field as well as in related fields of scientific inquiry.

Any research field that fails to satisfy even approximately all of the above twelve conditions will be said to be *nonscientific*. A research field that satisfies them approximately may be called a *semiscience* or *protoscience*. And if, in addition, it is evolving towards the full compliance of them all, it may be called an *emerging* or *developing* science. On the other hand any field of knowledge that is nonscientific but is advertised and sold as scientific will be said to be *pseudoscientific* (or a *fake* or *bogus* science). The difference between science and protoscience is a matter of degree, that between science and pseudoscience is one of kind. The difference between protoscience and pseudoscience parallels that between error and deception. Physics has been the paragon of science since Galileo, psychology and sociology are developing sciences, literary criticism is a nonscientific research field, and parapsychology is a bogus science. (See Section 4.1 for a substantiation of this charge.)

Note the following points. First, the research community must be a *system* proper, not an isolated individual or an aggregate of isolated researchers. However, in the absence of a local research community the scientific investigator will belong to a national, regional, or international research community by keeping abreast of the literature and contributing to it. Only crackpots are marginal.

Second, not every society is capable or willing to support a community devoted to scientific research proper. Thus theocratic societies discourage scientific research, and underdeveloped ones lack the material resources to encourage it (although they often find resources to wage war). So, what these societies support or tolerate are small communities of protoscientists, such as naturalists rather than full-fledged biologists. (More on the social conditions of science in Barber (1962) and Bunge (1980b).)

Third, the requirement that D be formed by *real entities* does not exclude speculation on still undiscovered entities. The point is that such speculation should be testable and should eventually yield results one way or another.

Fourth, the importance of the *philosophical background* G of science is often underrated, particularly by those who define science in terms of its method. They are apt to cite the cases of Newton, who was a religious believer, Mach, who held a sensualist ontology and epistemology, and Poincaré, who defended a conventionalist epistemology. One may rejoin that, while doing science, not philosophy, Newton, Mach and Poincaré were being unfaithful to their own philosophies. For example, there is no mention of God in Newton's equations of motion; Mach presumably took it for granted that his instruments did not vanish when he left his laboratory; and Poincaré admitted that Maxwell's equations are law statements not conventions. There is no science without some ontology and some epistemology. To begin with, all the fundamental concepts of science, such as those of thing and property, state and change of state, possibility and actuality, space and time, life and mind, artifact and society, are ontological. Secondly, when exploring some uncharted territory the scientist is tacitly guided by a number of ontological and epistemological principles. For one thing he presupposes that the most general known laws hold in the new territory, and that the most general methodological principles will help him to explore it. (But of course he is ready to correct such assumptions if proved wrong.) If he believes in objective possibility he will investigate things-in-their-environment instead of trying to account for their behavior exclusively in terms of environmental agencies. If he believes in randomness he will try stochastic models, otherwise he will limit himself to "deterministic" ones. If he is an inductivist he will collect as many facts as possible before hazarding any hypotheses. If he is a deductivist he will prefer exploring the logical consequences of hypotheses proposed by others—and so on and so forth. (For more on the metaphysics inherent in science see Whewell (1847), Agassi (1964), Holton (1973), Bunge (1977a).)

The *ethical* component of the general outlook is equally important to tell science from other epistemic fields. The morality of science is largely built into its methodics, this being why we often fail to detect it. According to Merton (1973) this code of behavior includes intellectual honesty, integrity, humility, disinterestedness, organized skepticism, universalism, impersonality, and communism of intellectual property. (Actually scientists are ambitious and often arrogant rather than humble. But they are supposed to be modest in an epistemological sense, i.e. ready to admit ignorance and error.) This code may not be detected as long as scientists go about their business as usual, but it comes to the fore as soon as one of them violates it—e.g. if he doctors data, plagiarizes, distorts results in the service of some ideology, or utilizes them for mercenary or destructive purposes. So much for the philosophy inherent in scientific research—a philosophy without which science cannot be told apart from other fields of knowledge. Let us go on with our remarks on the list of peculiarities of science.

Fifth, the *formal background F* of a science is an (uncertain) indicator of its degree of development. (No wonder that formalists tend to equate scientificity with formalization.) Thus physicists find occasion to use practically all of mathematics. Hence there is no such thing as a selected and fixed collection of theories making up "the mathematics for physicists". The same holds for all the other sciences. If a given branch of mathematics has not yet been used in a certain science, this may indicate only that the workers in it do not know about the existence of that branch or of its possible uses in their science. Hence it is mistaken to teach science students only those mathematical theories that happened to be of use in the past.

Sixth, the *specific background B* of a science is another indicator of its degree of development, as well as an indicator of the ontic level it deals with. Every science presupposes some other sciences. Thus biology presupposes chemistry, which in turn borrows some physics, which uses mathematics, which uses logic. No science borrows all of the knowledge in the sciences it presupposes, but a science with few debts is either very fundamental or very backward. This notion of intellectual debt (or presupposition) suggests the following stratification of the basic or pure sciences:

Formal sciences: mainly logic and mathematics
Physiosciences: physics, astronomy, earth sciences
Chemoscience: chemistry
Biosciences: biophysics, biochemistry (including molecular biology), genetics, physiology, ecology, biogeography, palaeobiology, neuroscience, etc.
Sociosciences: anthropology, sociology, economics, politology, history, etc.

In addition to these pure basic sciences we have a number of mixed or mongrel sciences, such as psychology and linguistics, which are both biological and sociological. (More on mixtures in Section 3.2.) On the other hand physical chemistry, which studies the physical aspects of chemical processes, is a branch of chemistry. Likewise human geography and economic sociology should be counted as parts of sociology.

Seventh, the *problematics P* of a science can be as vast as its workers wish it to be—whence the importance of a liberal education for scientists. In fact we may distinguish eight kinds of problems in each science:

Domain (referent) questions, such as "What is special relativity about: physical entities or measurement operations?", and "What are the referents of social science: individuals or social systems?"

Philosophical problems, such as "Do forms, in particular shapes, have a driving force, and so can they explain morphogenesis (as René Thom believes), or are they rather an outcome of process?", "Even though theory choice is actually done in an intuitive way, would it be possible to set up definite decision rules?", and "Is vivisection morally justified?".

Formal background problems, such as "What do we lose by giving up the excluded middle principle?", and "Does the given equation have any continuous solutions in such and such interval?".

Specific background problems, such as "Is the thermodynamics used by chemists on a sound basis?", and "Does kin selection (assumed by sociobiology) really operate?".

Problematics problems, such as "Is this problem well-conceived?", and "Is that problem worth being investigated?"

Fund of knowledge problems, such as "Are all redshifts of extragalactic objects to be interpreted as indicators of the expansion of the universe?", and "Are all mutations consequential, or are some of them neutral?".

Aim problems, such as "Should psychology explain the mental in neurophysiological terms?", and "Should social science seek societal laws?"

Methodological problems, of the forms "Is this method reliable?", and "How could we go about measuring that property?".

Eighth, the *fund of knowledge K* of a science may be lean or voluminous, depending on the state of the art, but it cannot be empty, for one cannot start from scratch, as the very formulation of a research problem requires some knowledge. Even a new science, when born, possesses some fund of knowledge, either borrowed from ordinary or artisanal knowledge, or inherited from a parent science.

Ninth, the *aims* of a science include the search for or use of laws, since the supreme goal of science is to understand reality, and such understanding can only be obtained with the use of laws. *No laws, no science proper*—at most some protoscience. For example, a scholarly study on the concept of cow in the Rigveda, or on the nature of angels according to the authors of the Old Testament, does not qualify as scientific because it does not seek to find any laws and it does not even make use of any known laws. In general, textual investigation does not qualify as scientific even if it uses scientific means such as chemical analysis or soft X-rays, for its aim is not that of science. The same holds for art history, intellectual history, and other research fields.

Tenth, the *methodics* of any science includes not only its peculiar techniques but also the scientific method (Ch. 7, Section 2.2). A collection of techniques, e.g. for producing high pressures or high vacua, or for measuring the effects of reinforcement on the learning of philosophy, does not constitute a science: methods are means not ends, and they cannot be applied or evaluated apart from a problematics and an aim. Merely exploiting a given technique for obtaining or processing data without any ulterior purposes is not doing science but just keeping busy and possibly salaried.

So much for our characterization of basic science. It may be objected that any set of criteria of scientificity is bound to change in time, so that what is now regarded as basic science may tomorrow be regarded as something else, e.g. applied science or even pseudoscience. (See, e.g. Hyman, 1982.) Granted. But the mutability of criteria does not prove that they do not exist or that they are futile. Likewise the standards of mathematical rigor are changeable in time, sometimes quickly, but this proves only that rigor can be improved, not that it does not exist. We need a definite set of criteria of scientificity, however time-bound, if we wish to produce, teach, and promote genuine basic science rather than something else, be it technology or ideology, and if we do not wish to be taken in by charlatans posing as scientists.

2.2. *Applied Science and Technology*

The expression "applied science" is ambiguous. It may signify the application of one science to another (e.g. of physics to biology) or the use of scientific knowledge to investigate problems the solutions to which may acquire practical importance—e.g. in industry, education, or politics. The application of the probability calculus to statistics, of physics and chemistry to the earth sciences and meteorology, and of biology to medical problems, exemplify the first concept of applied science. Needless to say, this kind of applied science does not follow automatically from the corresponding basic sciences. For example, the earth sciences are not deducible from the sole principles of physics and chemistry. Far from it, they call for specific theoretical models (of, e.g. the earth's interior), specific methods (e.g. for seismological measurements), and specific data (e.g. about the configuration of the terrestrial magnetic field). Moreover the earth sciences involve specific concepts not found in the underlying basic sciences, such as the very concepts of planet, atmosphere, and ocean; of plate, continental drift, and even rock; of earthquake, wind, storm, climate, and so on. In short, the basic sciences are necessary but not sufficient for the corresponding applications.

The common acceptation of 'applied science' is the second, and the one we shall deal with, namely the investigation of cognitive problems with *possible* practical relevance. Here is a *pêle-mêle* list of fields of applied scientific research: materials science, in particular metallurgy; the analysis of natural products with a view to isolating some useful chemicals; the experimental synthesis of promising polymers (e.g. synthetic fibers); the investigation of the biology of plants with a possible industrial interest, such as the *guayule* (that produces rubber); the whole of pharmacology and food science; the entire field of medical research, from internal medicine to neurology; psychiatry and clinical psychology; the entire science of education; and the applications of biology, psychology and social science to investigating social problems such as unemployment and marginality, drug addiction and criminality, cultural deprivation and political apathy, with a view to designing social programs aiming at their eradication.

Applied science, in this second sense, does not follow automatically from basic science either. Doing applied science, like doing basic science, is conducting research aiming at acquiring new knowledge. The differences between applied research and basic research are differences in intellectual debt to basic science, in scope, and in aim. The first difference consists in that all applied science in the second sense is also applied science in the first, though not conversely. That is, applied research employs knowledge acquired in basic research. This does not entail that applied research is necessarily a matter of routine: if it did not yield new knowledge it would not be research proper, and to deliver new knowledge it must investigate problems of its own. However, the task of the applied scientist is to enrich and exploit a fund of knowledge that has been produced by basic research.

For example, the space scientists who developed new materials and studied human physiology and psychology under unfamiliar conditions (zero gravity, isolation, etc.) tackled new problems that could not possibly be solved with the sole help of existing knowledge—and they came up with new knowledge. The applied scientist is supposed to make discoveries but is not expected to discover any deep properties or general laws. He does not intend to.

Secondly, the domain or scope of applied science is narrower than that of basic science. For example, instead of studying learning in general, the applied psychologist may study the learning of a given language by a given human group in particular social circumstances—e.g. the way Mexican Americans learn English in a Los Angeles slum. And instead of studying social cohesion in general, the applied sociologists may study the social cohesion of, say, a marginal community in a Latin American shanty town, with an eye on possible ways of improving its lot.

Thirdly, all applied research has some practical target, even if it is a long term one. For example, the forester is not only interested in forests in general but also, and particularly, in forests with a possible industrial utility. And the pharmacologist, unlike the biochemist, focuses his research on chemicals that may be beneficial or noxious to certain species, particularly ours. In every case we expect from the applied scientist to end up every one of his reports by asserting, not just that he has discovered X, but that he has discovered X, which seems to be useful to produce a useful Y or prevent a noxious Z. But we will not ask him to design an artifact or a procedure for actually producing Y or preventing Z: this is a task for the technologist. Table 14.1 illustrates what has been said so far and prepares the ground for what comes next.

Applied science lies between science and technology, but there are no borderlines between the three domains: each shades into the other. The outcome of a piece of basic research may suggest an applied research project, which in turn may point to some technological project. Once in a blue moon a single investigator spans all three. More often than not each task demands people with peculiar backgrounds, interests, and goals. Whereas the original scientist, whether basic or applied, is essentially a discoverer, the original technologist is essentially an inventor of artificial things or processes. Indeed the invention of artifacts or of social organizations is the very hub of technological innovation. No matter how modest, an invention is something new, that did not exist before, or that existed but was beyond human control. Thus fire existed before man, but

TABLE 14.1
Some applied, technological and economic partners of some basic investigations

Basic science	*Applied sciences*	*Technologies*	*Industry, commerce, services*
Mathematics	All	All	Consulting
Astronomy	Optics of light, radio, and X-ray telescopes; photometry, bolometry, etc.	Design of astronomical instruments; invention and development of photographic plates; observatory architecture; etc.	Optical industry; photographic industry; maintenance and repair of astronomic instruments; etc.
Atomic physics	Physics of semiconductors, electronics.	Design of radios, TV sets, computers, calculators, etc.	Manufacture and maintenance of electronic apparata.
Chemistry	Hydrocarbon chemistry	Petroleum engineering (prospection, drilling, refining, etc.).	Building and maintenance of petroleum machinery, installation and maintenance of oil wells and refining plants.
Biology	Biology of edible plants	Phytotechnology of edible plants: creation of new varieties, study of new cultivation methods, etc.	Agriculture, food industry, food marketing.
Sociology	Development sociology	Development plans.	Implementation of development plans.

only man invented ways of producing, maintaining and extinguishing it at will; likewise nuclear energy has always existed, but man tapped it first in 1945. Whereas the invention of fire-making and keeping devices required no science at all, that of the nuclear reactor and the nuclear bomb were technological feats indebted to nuclear physics and the chemistry of fissionable materials, which originated in the disinterested investigations of Becquerel, the Curies, Rutherford, Fermi, and others.

Most of the inventions proposed until the beginning of the Modern period owe hardly anything to science: recall the domestication of most plants, animals, fungi, and bacteria; the plough and metallurgy, architecture and coastal navigation. Things started to change in the 17th century and, particularly, since about 1800. The pendulum clock and the Watt governor are based on mechanics; the electric generator and the electric motor, on electrodynamics; the synthetic products used in industry and medicine, on chemistry; the electronic gadgets, on atomic physics; the supercultivars on genetics, and so on. In short, since about 1800 and on the whole, technological breakthroughs have followed scientific discoveries. The most common pattern is nowadays: *scientific paper—applied science report—technological blueprint*. And, as is well known, the time lag between these stages is decreasing rapidly.

Saying that an invention "is based on" a bit of scientific knowledge does not signify that science suffices to produce technology but that it is employed to some extent in designing the artifact or the plan of action. For example, Joseph Henry designed the first electric motor on the basis of his knowledge of electrodynamics (to which he himself had contributed); and Marconi invented the radio by exploiting Maxwell's theory as well as Hertz's experiments. The modern inventor need not know much science but he cannot ignore it altogether, for what is called "the principle" of a modern invention is some property or law usually discovered by some scientific research. Thus the "principle" of jet propulsion is Newton's 3rd law of motion, and the antihistaminic "principle" is the antigen–antibody reaction discovered by the immunologists. What characterizes the inventor is not so much the breadth and depth of his knowledge as his knack for exploiting what he knows, his wondrous imagination, and his great practical sense.

Invention is only the first stage of the technological process. Next comes the so-called development stage—which is where most inventions sink. The blueprint must be translated into a prototype, or a handful of seeds of a new cultivar, or a few milligrams of a new drug, or a plan for a new social organization. Once these artifacts have been produced it is necessary to put them to the test, to see whether they are effective. In the case of a new drug the tests may take some years and millions of dollars.

The third stage is the design of the production, in the case of artifacts, and of the implementation, in the case of social programs. This may demand the building of an entire pilot plant, which poses new problems, which demand new inventions. (In technology, as in basic science and in life in general, one thing leads to another.) Even if built, the pilot plant may not work satisfactorily for some reason or other; and even if it is technically satisfactory it may prove to be too expensive, or to produce socially undesirable effects, such as unemployment or high pollution. No wonder that most of the R&D budget goes into the development stages.

The rule of thumb is this: for every $10 of the total R&D budget, $1 is devoted to basic research, $2 to applied research, and $7 to technological R&D. One reason for this is that only between one-tenth and one-hundredth of the findings of basic research are applicable, a similar fraction of the results of applied research reaches technology, and again only a like fraction of the technological patents ever reaches the market. Thus the fraction of basic research that has an impact on production or the services lies between one thousandth and one millionth. The short-sighted policy maker takes this low practical productivity as an indication that basic research is not worth being supported. The long-sighted policy maker understands this to be an indication that, if we wish to increase productivity, we must enhance creativity and expedite the now often clogged channels between basic science, applied science, and technology.

If the third and last stage of the technological process has been accomplished successfully, production may begin and, eventually, marketing may be undertaken. These two stages may require new inventions and new tests concerning the production as well as its organization and that of the distribution of the product. Such new inputs are likely to be the more novel and frequent, the more original the invention and the more massive the production line and the marketing network. However, these problems usually lie beyond the horizon of the first inventor, unless he happens to be, like Bell, Edison or Land, a businessman himself. Again, this is largely due to differences in personality traits: the inventor is primarily motivated by curiosity and love of tinkering, not by hopes of fortune.

Table 14.2 describes schematically the process that ends up in the market. In the case of an artifact, such as an electronic calculator, or a procedure such as a medical treatment, all stages are gone over. On the

TABLE 14.2
From lab to market.

Activity	*Carried out by*	*Mainly at*	*Output*
Basic research	Basic scientists	Universities	Data, formulas, graphs,
Applied research	Applied scientists	Universities, industrial labs., state labs., etc.	experimental designs, blueprints of scientific instruments, calculations, methods, etc.
Technological invention	Technologists	Industrial labs.	Designs and models of artifacts or procedures.
Development	Technologists and managers	Industrial labs., workshops, and experimental stations	Prototypes, pilot plants, production plans, social programs.
Production	Workers, foremen, technicians, managers, clerks	Industrial plants, farms, mines, forests, seas	
Marketing	Marketing experts, managers, salesmen, ad men, etc.	Offices, stores, warehouses, etc.	Goods or services.

other hand in the case of a more modest product or service, such as a canned food or the organization schema of a cooperative, the scientific stages are usually skipped: not that scientific knowledge is totally absent, but it is borrowed rather than freshly produced. In other words, whereas some inventions call for new investigations, others can be produced with the help of extant knowledge.

Just as administrators and politicians tend to mistake science for technology, so scientists tend to believe that they are the only innovators: that what others do is low grade work, nearly always routine, that anyone can do. This arrogant belief is false: there is innovation in every one of the stages described in Table 14.2. There is innovation not only when an industry or a service is established for the first time, but also in the course of its maintenance, particularly when new circumstances appear. To be sure, one could try to keep an artificial system, such as a factory or a school, unaltered over the years, but this would be foolish in a society in which everything else changes rather quickly. New needs and competition stimulate inventiveness, and innovation may confer advantages both for competition and cooperation. (Besides, not all innovation is elicited by need: much of it is the product of pure curiosity.)

So far we have been concerned with the basic science-applied science-technology-economy flux, typical of the contemporary period. But there is also a constant reflux in the opposite direction. The laboratory worker uses instruments, materials, drugs and even experimental animals produced in mass and uniformly by industry. Applied science and technology supply basic science, new materials, new instruments, and especially new problems. In short, every one of the components in Table 14.2 acts on all the others, not counting the remaining branches of culture and politics. All of these items compose a system that is characteristic of the modern times, namely the *system of production and circulation of knowledge, artifacts, and services*. See Figure 14.1.

Finally, note that we have included in technology far more than the traditional branches of engineering. In fact we conceive of technology as *the design of things or processes of possible practical value to some individuals or groups with the help of knowledge gained in basic or applied research*. The things and processes in question need not be physical or chemical: they can also be biological or social. And the condition that the design make use of scientific knowledge distinguishes contemporary technology from *technics*, or the traditional arts and crafts.

Our definition of "technology" makes room for all the action-oriented fields of knowledge as long as they employ some scientific knowledge. Hence in our view technology subsumes the following fields (Bunge, 1977d):

> *Physiotechnology*: civil, mechanical, electrical, nuclear, and space engineering.
> *Chemotechnology*: industrial chemistry, chemical engineering.
> *Biotechnology*: pharmacology, medicine, dentistry, agronomy, veterinary medicine, bioengineering, genetic engineering, etc.
> *Psychotechnology*: psychiatry; clinical, industrial, commercial and war psychology; education.
> *Sociotechnology*: management science (or operations research), law, city planning, human engineering, military science.
> *General technology*: linear systems and control theory, information sciences, computer science, artificial intelligence.

In conclusion, basic science, applied science and technology have commonalities as well as differences. All three share essentially the same world view, mathematics, and the scientific method. (For the relatively minor differences among them see Vol. 6.) They differ mainly in their *aims*: that of basic science is to understand the world in terms of patterns; that of applied research is to use this understanding to make further inquiries that may prove practically useful; and that of technology is to control and change reality through the design of artificial systems and plans of action based on scientific knowledge.

3. The Knowledge System

3.1. *Inter-relations*

Our very definition of a research field (Section. 1.2) includes the condition of a close connection with contiguous fields of inquiry. (On the other hand the pseudo-sciences are typically isolated from the remaining fields of knowledge.) In particular, the various basic and applied sciences and the technologies are closely related to one another because they borrow from each other's funds of knowledge and share a world view, mathematics, and a method of inquiry.

Moreover, although every level of reality—physical, chemical, biological, social, and technical—can and must be studied by itself, the supraphysical levels cannot be adequately understood except with the help of the sciences that study the underlying levels. (See Vol. 4, Ch. 1 for the concept of a level.) The reason is that the lower level sciences study the components of the systems investigated by the higher level ones. Thus physics studies the components of molecules, chemistry those of cells, and physics, chemistry and biology study the components of social systems, namely persons and artifacts. Thus knowledge, and the study of knowledge—i.e. epistemology—must match the level structure of reality. (Hence epistemology presupposes ontology.)

In other words, the composition of a sociosystem is included in the union of the reference classes of the life sciences, chemistry, and physics. (Which is very different from saying that social systems constitute a special class of physical systems—a physicalist thesis rejected in Vol. 4.) Hence the proper study of social systems presupposes some (by no means all) of the lower level sciences. Likewise the composition of a biosystem is included in the union of the reference classes of chemistry and physics. (Again, this is not to say that biosystems are a special class of physical systems—another physicalist thesis rejected in Vol. 4.) Hence a proper study of biosystems presupposes some (not all) chemistry and physics, the sciences that study, *inter alia*, the cell components.

The attentive reader must have noticed that we have stipulated that the study of systems at a given level presupposes *some* lower level sciences, not all of them. For example, the social scientist need not know electromagnetism, for social systems (his objects of study) are neither electrically charged nor magnetized. In other words, the higher level systems do not possess all the properties of their components—but, in compensation, they possess (emergent) properties of their own. Recall Figure 13.3. This is why a higher level science has a specific background properly included in the fund of knowledge of the immediately prior research field (i.e. $B_{n+1} \subset K_n$). The resulting picture of the system of scientific and technological level is a staircase: Figure 14.2. This picture has the disadvantage of being static. Only a flow diagram can represent the various information flows that interconnect all the specialized research fields, turning them into components of a single S&T (science and technology) system.

Our image of the S&T system fits in with our view of the level structure of reality, i.e. our epistemology harmonizes with our ontology. In particular, the two are emergentist, i.e. they recognize the emergent properties that characterize totalities, and the emergent concepts needed to account for them. To put it negatively, ours is not a reductionist image of knowledge because it does not presuppose a physicalist ontology but, rather, an emergentist one. (See Vol. 4 and Bunge (1981a).)

Ordinarily physicalism and emergentism are held in a rather dogmatic fashion. The only way either of them can be supported is by proving that it is possible, or impossible, to reduce every science to physics. This is a herculean task that nobody seems to be willing to undertake. A far simpler strategy is to try and *disprove* the reducibility thesis in the case of a few selected theories. Thus if we succeed in proving that quantum chemistry, though based on quantum mechanics, is not reducible to the latter, or that genetics, though based on chemistry, is not a chapter of the latter, we shall have disproved physicalism and its epistemological concomitant, namely reductionism. This task, sketched in Ch. 13, Section 2.2, will be undertaken in Vol. 6. It will turn out that full inter-theoretic reduction is the exception rather than the rule. This ruins the simplistic thesis of the unity of science via reduction to physics. Still, we do have a unified S & T system characterized by an enormous diversity of components united by a common *Weltanschauung*, a common mathematics and a common method, as well as by strong information flows.

Because the S&T system is a tightly knit one, none of its components can alter in a significant manner without affecting some other components. Only individual items of information, in particular data, can change gradually and locally. Drastic changes in theory or in method can hardly be circumscribed: sooner or later they result in some overall changes. Therefore the history of every component of the S&T system is influenced by that of some other components. This systemicity of the sciences and technologies is in sharp contrast with the nonsystemic character of ordinary knowledge, which can be altered by little steps, i.e. by the accretion or attrition of individual bits of information. Indeed ordinary knowledge is a heterogeneous aggregate of superstitions and crumbs fallen from the high tables of S & T. Therefore it belongs in the dock not in the court of appeal. However, ordinary knowledge will always be attached to specialized knowledge, for it is the starting point of some research, and because its information vehicle, namely ordinary language, is necessarily used in all fields of knowledge. Therefore we must link ordinary knowledge with the S & T system.

Finally we come to the notion of the total system of human knowledge, composed of ordinary knowledge, technics (specialized artisanal knowledge), basic and applied science, technology, the humanities, and parts of the arts and the ideologies. That these fields do combine into a system is

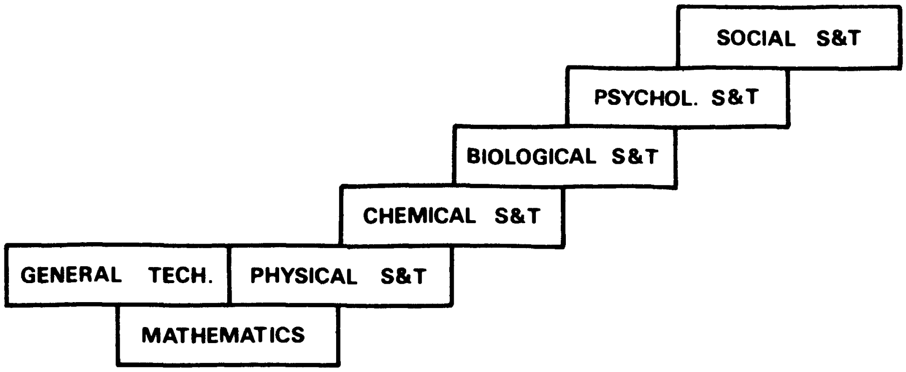


Fig. 14.2. The system of the sciences and technologies. Each step is based on only a part of the preceding one and adds something of its own.

easy to prove. First, ordinary knowledge, despite its limitations, is the *fons et origo* of much specialized knowledge as well as a repository of (often obsolete) stray items of scientific and technological knowledge. Second, science (including mathematics) is the basis of technology as well as a tool and model for the humanities. Third, the technics and technologies serve all and are influenced by all. Fourth, the humanities study, among other things, all kinds of knowledge, and they often draw from the sciences. Fifth, some art genres, in particular the novel and the theatre, give us knowledge of the human condition. Sixth, ideology leads or misleads some research and much technological design. Those seven large fields are then functionally united even though they are logically and methodologically at odds in places. We are therefore justified in speaking of the total *system of human knowledge*: Figure 14.3.

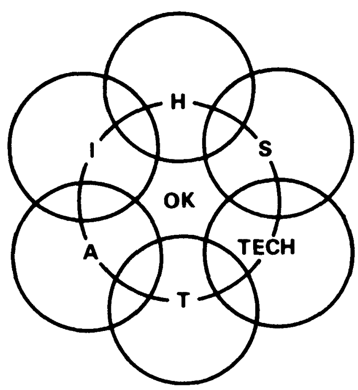


Fig. 14.3. The system of human knowledge: technics (T), technologies (Tech), the sciences (S), the humanities (H), the cognitive part of sociopolitical ideologies (I), and the cognitive part of the arts (A), grow from ordinary knowledge (OK) and enrich one another.

Moreover, as remarked in Section 2.2, knowing is closely related to other human activities, in particular production and the services. Therefore we must introduce the wider notion of the total *system of production and circulation of goods and services of all kinds*, whether cultural, such as theories and books, or noncultural, such as shoes and dental treatments. However, the fact that all these heterogeneous items interact to form a system does not prove that they can be conceptualized in the same manner. In particular, the creation of a new theory cannot be analyzed in the same way as the manufacture of a car.

3.2. *Mergers and Mixtures*

The interrelations among the various components of the human knowledge system enrich and control each of them, and also produce the fusion of some of them. Social psychology is a good example of such fusion: it is not merely the sum of individual psychology and sociology, but a combination of the two. Indeed, it is characterized by constructs and problems of its own that do not occur in either of the parent fields. Two such problems are the changes in personality caused by entry in an unfamiliar social group, and the changes in social organization due to the action of a few individuals. Another interesting synthesis is bioeconomics, which is both a biological and a social science, for it aims at devising methods for the optimal exploitation of renewable resources on the basis of a knowledge of the laws of the latter.

The fusion or merger of research fields is not to be mistaken for their mere addition to form larger fields where each constituent retains its individuality. Such additions, exemplified by space science, geography, and forestry, may be called *mixtures* or *multidisciplines*. (The sum or mixture of two fields of knowledge may be defined as a third field whose first two coordinates equal the physical sums of the corresponding partial coordinates, whereas its eight remaining coordinates are the unions of the corresponding partial coordinates. I.e., $\mathscr{E}_1 + \mathscr{E}_2 = \langle C_1 \dotplus C_2,\ S_1 \dotplus S_2,\ D_1 \cup D_2, G_1 \cup G_2, F_1 \cup F_2, B_1 \cup B_2, P_1 \cup P_2, K_1 \cup K_2, A_1 \cup A_2, M_1 \cup M_2 \rangle$, where $\dotplus$ denotes the operation of juxtaposition or physical addition defined in Vol. 3, Ch. 1. If both fields are branches of science, we may set $G_1 \cup G_2 = G_1$, $F_1 \cup F_2 = F_1$, $A_1 \cup A_2 = A_1$, and $M_1 \cup M_2 = M_1$.)

Unlike fusions or mergers, mixtures have no conceptual unity: what unity they do have derives from the referents common to their components fields. Thus space science is a conglomerate of fields united only by their dealing with space ships and their uses. Some of the components of space science are applied sciences whereas others are technologies. Geography is another examples of a multidiscipline, for it is composed of two main fields, physical geography—which is really an earth science—and human geography—which belongs to social science. Because of their great variety, space science, geography, and other multidisciplines—notably medicine—are able to attract workers with a great variety of backgrounds, interests and abilities.

Forestry is another rich multidiscipline encompassing basic sciences, applied sciences, and technologies. A glance at *Forest Science* or the *Journal of Forestry* will attest that the field attracts laboratory and field scientists, mathematical modelers and statisticians, geologists and geographers, bioeconomists and social scientists, and even meteorologists and pychologists. The *basic science* component of forestry is obvious: it is a branch of ecology, for it studies the forest as a community of organisms, and yields knowledge not to be found in the branches of biology concerned with individual organisms. (*Examples*: study of competition among trees, of cooperation among the latter and the undergrowth, and of the coevolution of trees and animals.) The *applied science* component of forestry is no less evident: foresters make use of basic biology, from genetics and cellular biology to plant physiology and ecology, in studying problems of their own that may lead to results of practical interest, such as new ways of intensifying or inhibiting pollination, accelerating or retarding growth, controlling pests of all kinds, etc. All such investigations use basic biology but, by posing new problems, they ensue in new findings, some of which are fed back into basic biology.

Finally, the *technological component* of forestry may be regarded as constituted by forest engineering and forest management. While both deal with the maintenance and exploitation of forests, forest engineering handles the mechanical aspects, such as clear cutting and log transportation, whereas forest management deals essentially with forest maintenance, forestation, harvesting, and non-commercial uses. Forest management is about forests as well as the people who use them, from rangers and loggers to timber businessmen and tourists: it concerns the forest-man system. In sum, forestry may be regarded as a three-tier system embedded in the S&T system: Figure 14.4.

Why should it matter whether forestry—or for that matter the life sciences, the psychological sciences, or the social sciences—is pure or a mixture, a science or a technology? It matters for several reasons. Firstly

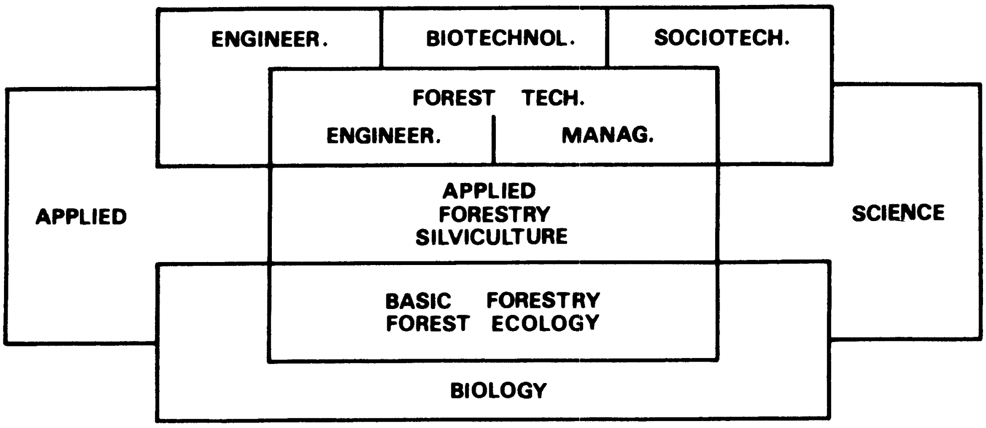


Fig. 14.4. The basic, applied and technological components of forestry.

because, if a field is recognized as a mixture, then it must also be recognized that it is to be cultivated by people with a variety of backgrounds, skills, and interests. (Take note, curriculum designers and science administrators.) Secondly, because a multidiscipline, to stay healthy, must keep open all the communication channels among its components. Thirdly because, if forest management or any other branch of sociotechnology is seen as one component of a rich system with scientific components, it will be realized that any policy and any plan concerning forests—whether it deals with conservation or exploitation—must be based on definite and well tested models of forests rather than on the sole aim of their users. (Thus if the logistic model is used as a rough first approximation, it is realized that the harvesting rate must not exceed the difference between the birth rate and the death rate. Obviously, the criminal harvesting of rain forests has ignored any such model.) In sum, there is no rational action without models and plans based on scientific knowledge. (For a clear pioneering statement of this truism with regard to forest management see Wappes (1926).)

An investigation is said to be *multidisciplinary* if it straddles several research fields, and *interdisciplinary* (or *cross-disciplinary*) if, being multidisciplinary, it does not reach for a collection of separate results but for an integrated view, such as a model interrelating various aspects of the object of study. For example, any reasonable plan for the redesign of a city is the work of an interdisciplinary team composed by geographers, sociologists, economists, city planners, public health experts, teachers, artists, and other specialists exchanging information and views and attacking multidimensional problems.

Most research in science is unidisciplinary or multidisciplinary, seldom interdisciplinary. This is justified in the case of relatively simple objects such as an isolated atom at 0 K. But unidisciplinarity is bound to fail in the case of many-level systems such as chemical reactors, cells, brains, firms, or societies, for they involve several levels of organization. In such cases the prevailing unidisciplinary approach is bound to yield bits of knowledge that "make sense" or "fall into place" only in a wider perspective.

For example, one cannot hope to find out how the brain works with the sole help of neuroanatomy, or neurophysiology, or neuroendocrinology: one needs also evolutionary and developmental studies, as well as individual and social psychology. In practical terms, this entails that the brain should be studied by interdisciplinary teams led by all-rounders. For example, clinical psychologists and neurologists have not learned much about their patients because the anatomy of the latter is usually studied by different workers, namely neuroanatomists and neuropathologists. (Apparently the first cases of Wernicke–Korsakoff syndrome, or severe loss of memory, to have been studied both clinically and anatomically by the same investigators were reported only recently: Mair *et al.*, 1979.) The current fragmentation of fields such as neuroscience, psychology, linguistics, and social science is a major obstacle to their development.

The need for interdisciplinarity is better understood by technologists. The best experts among them are not narrow specialists but multispecialists—not to be mistaken for superficial generalists. That is understandable: the technologist handles things that cannot be artificially isolated from their environment or detached from human action, so he is forced to take many of their aspects into account. He is forced to handle, say, systems of chemical reactions under a variety of conditions, rather than a single reaction under an extreme ideal condition; plants and animals in the field rather than in pots or cages; and people interacting among themselves and solving real life problems, rather than working out academic problems in a psychological laboratory.

The narrow specialist is an endangered species, but nobody should regret its extinction. Things are far too complex to be dealt with by a single discipline. Therefore we need more and more all-rounders of the 17th and 18th centuries savant type. The classical saying about jacks of all trades should be inverted to read *Jack of a single trade—not even master of his own.* After all, the sum total of human knowledge is no mere aggregate: it is a system (Section 2.2). And the systemic character of knowledge matches the systemicity of the world (Vol. 4).

4. Illusory knowledge

4.1. *Pseudoscience*

As defined in Section 2.1, any body of belief and practice that is not scientific but is presented as such deserves being called a *pseudoscience*. There are plenty of pseudosciences, almost as many as genuine sciences. The most popular specimens are parapsychology, psychoanalysis, scientific creationism, Lysenkoist botany, gigo computeering, and monetarism. Pseudoscience is dangerous because (*a*) it passes wild speculation or uncontrolled data for results of scientific research, (*b*) it misrepresents the scientific approach (the "spirit" of science), (*c*) it contaminates some fields of science, particularly the soft and young ones, (*d*) it is accessible to millions of people (whereas genuine science is hard and therefore elitist), and (*e*) it enjoys the support of powerful pressure groups—sometimes entire churches and political parties—and the receptivity of mass media. For all these reasons it behooves philosophers to supply an accurate diagnosis of pseudoscience.

Unfortunately philosophers have so far failed to do so: in some cases they have admitted entire pseudosciences and in others they have rejected whole sciences. This failure is so dismal that one well-known philosopher has concluded that there is no real difference between science and pseudoscience, so that a democratic society should allot equal time to each in all schools (Feyerabend, 1975). The reason for such failure to tell pseudoscience from science is that most philosophers have believed that a single trait—such as empirical content, refutability, or consensus—suffices to characterize science, so that its absence would be sufficient indication of pseudoscience. As we saw in Section 2.1, this is a serious mistake: a field of knowledge must satisfy jointly about ten different conditions to qualify as a science. This is our clue to our definition of a pseudoscience other than the negative definition recalled at the beginning of this subsection.

We stipulate that a field of knowledge

$$\mathscr{E} = \langle C, S, D, G, F, B, P, K, A, M \rangle$$

is a *pseudoscience*, or a *pseudotechnology*, if and only if

(i) C is a community of *believers* who call themselves scientists or technologists although they do not conduct any scientific or technological research;

(ii) The host society S supports C for practical reasons (e.g. because $\mathscr{E}$ is good business) or tolerates C while relegating it beyond the border of its official culture;

(iii) the *domain* D of $\mathscr{E}$ contains unreal or at least not certifiably real entities, such as astral influences, disembodied thoughts, superegos, collective consciousness, national will, UFOs, and the like;

(iv) the *general outlook* G of $\mathscr{E}$ includes either: (*a*) an ontology countenancing immaterial entities or processes, such as disembodied spirits, or (*b*) an epistemology making room for arguments from authority, or for paranormal modes of cognition accessible only to the initiates or to those trained to interpret certain canonical texts, or (*c*) an ethos that, far from facilitating the free search for truth, recommends the staunch defense of dogma, including deception if need be;

(v) the *formal background* F of $\mathscr{E}$ is rather modest. Logic is not always respected, and mathematical modeling is the exception rather than the rule. The few mathematical models that have been proposed (e.g. for psi phenomena) are untestable and therefore phoney;

(vi) the *specific background* B of $\mathscr{E}$ is very small or nil: a pseudoscience learns little or nothing from other fields of knowledge. Likewise it contributes little or nothing to the development of other fields;

(vii) the *problematics* P of $\mathscr{E}$ includes more practical problems concerning human existence (in particular how to feel better and influence other people) than cognitive problems. (Most of the so called pseudosciences are best described as pseudotechnologies.)

(viii) *The fund of knowledge* K of $\mathscr{E}$ is practically stagnant and contains numerous untestable or even false hypotheses in conflict with well confirmed scientific hypotheses. And it contains no universal and well confirmed hypotheses belonging to hypothetico-deductive systems, i.e. law statements;

(ix) the *aims* A of the members of C are often practical rather than cognitive, in consonance with its problematics P. And they do not include the typical goals of scientific research, namely the finding of laws and their use to understand and predict facts;

(x) the *methodics* M of $\mathscr{E}$ contains procedures that are neither checkable by alternative (in particular scientific) procedures nor are they justifiable by well confirmed theories. In particular, criticism is not welcomed by pseudoscientists or pseudotechnologists;

(xi) there is no field of knowledge, except possibly another pseudoscience or pseudotechnology, that overlaps with $\mathscr{E}$ and is thus in a position to enrich and control $\mathscr{E}$. I.e., every pseudoscience and every pseudotechnology is practically *isolated*: there is no such thing as the system of pseudosciences and pseudotechnologies paralleling that of S&T;

(xii) the membership of every one of the last eight components of $\mathscr{E}$ *changes but little* in the course of time and, when it happens to change, it does so in limited respects and as a result of controversy or external pressure rather than research.

We have just described the ideal pseudoscience or pseudotechnology. The description fits psychoanalysis, homeopathy, chiropractice, and other fields. These are all tradition-bound and dogmatic rather than forward-looking and exploratory. (Has anyone ever seen a psychoanalytic laboratory or a mathematical model of the id-ego-superego trinity?) Only parapsychology, which deals with alleged psychic phenomena, is research-oriented. However, 68% of its practitioners have complete, and 22% strong belief in such phenomena (McConnell and Clark, 1980). Besides, parapsychology fails to meet all other conditions for a field of knowledge to be scientific. Let us check them, leaving the details to specialists such as Diaconis (1978), Hansel (1980), and Alcock (1981).

(i) *Domain*. Parapsychology is about immaterial entities, such as disembodied spirits, the existence of which has never been established. So, it is a discipline without a subject matter. On the other hand parapsychology (just as psychoanalysis and mentalistic psychology) ignores the very organ of the mind, namely the brain.

(ii) *General outlook*. The philosopher of science Broad (1949) examined carefully the compatibility of parapsychology with the scientific world view, which he called a "set of basic limiting principles", and concluded that parapsychology does not comply with them—whence they, not parapsychology, had to be given up. For example, precognition violates the principle of antecedence ("causality"), according to which the effect does not happen before the cause. Psychokinesis violates the principle of conservation of energy as well as the postulate that mind cannot act directly on matter. (If it did no experimenter could trust his own readings of his instruments.) Telepathy and precognition are incompatible with the epistemological principle according to which the gaining of factual knowledge requires sense perception at some point.

(iii) *Formal background*. The typical parapsychologist does not excel at handling formal tools, in particular statistics. Thus he consistently selects the evidence ("optional stopping" of a sequence of trials); he does not distinguish a coincidence (accidental or spurious correlation) from a causal relation or a genuine correlation; and he is not fond of mathematical models or even or informal hypothetico-deductive systems: his few hunches are stray.

(iv) *Specific background.* Parapsychology makes no use of any knowledge gained in other fields, such as physics and physiological psychology. Moreover, its hypotheses are inconsistent with some basic assumptions of factual science. In particular, the very idea of a disembodied mental entity is incompatible with physiological psychology; and the claim that signals can be transmitted across space without fading with distance is inconsistent with physics. Worse, parapsychologists brush these inconsistencies aside, claiming that they deal with nonphysical phenomena, so that physicists and other natural scientists are not competent to study them.

(v) *Problematics.* Parapsychology is extremely poor in problems: all its problems boil down to that of establishing that there are paranormal phenomena, i.e. facts that cannot be explained by science. Nor is this problem formulated in clear terms, and this because of the appalling theoretical indigence of parapsychology.

(vi) *Fund of knowledge.* Despite being several thousand years old, and having attracted a large number of researchers over the past hundred years, we owe no single firm finding to parapsychology: no hard data on telepathy, clairvoyance, precognition, or psychokinesis, and no hypotheses to explain these alleged phenomena. All parapsychologists tell us is that these alleged data are anomalous, i.e. unexplained by the science of the day. They suggest no mechanisms and propose no theories. Compare this behavior with that of a scientist, say an astronomer. If an astronomer were to find that a certain celestial object does not seem to "obey" the laws of celestial mechanics or astrophysics, he would feel it his duty to offer or invite some positive conjectures—e.g. that it is not an ordinary body but a quasar or a black hole, a plasma or a laser beam, or some other physical thing. He may conjecture that this thing of a new kind "obeys" laws not yet discovered—but not that it violates well established physical principles such as that of conservation of energy. The parapsychologist does no such thing: he accepts apparently anomalous phenomena as evidence for paranormal abilities, and takes no steps to explain them in terms of laws. Has anyone heard of the First Law of Clairvoyance, or the Second Law of Telepathy, or the Third Law of Psychokinesis? And has anyone ever produced a perpetual motion engine driven by the mind, or a mathematical theory of spooks capable of making definite testable predictions?

(vii) *Aims.* Judging by the accomplishments of parapsychologists, their aim is not that of finding laws and systematizing them into theories in order to understand and forecast. Rather, their aim is either to buttress ancient spiritualist myths or to serve as a surrogate for lost religions.

(viii) *Methodics.* The methods employed by parapsychologists have been scrutinized by scientists, statisticians and stage magicians for more than one century and found almost invariably faulty. The most common defect is lack of strict controls. But deception, either unconscious as in the case of the ordinary experimental subject who wishes the experiment to succeed, or deliberate as in the famous case of the spoon benders, has always plagued parapsychology. (For plenty of amusing examples peruse the journal *The Skeptical Inquirer.*)

(ix) *Systemicity.* Far from being a component of the system of human knowledge, parapsychology is a stray: it does not overlap with any other research field. Therefore its practitioners ask that it be judged on its own merits: on the strength of the empirical evidence they claim to have produced. But this is impossible, quite aside from the fact that such "evidence" is quite suspicious for having been gathered with faulty methods—not to speak of old folk stories and other anecdotal "evidence" still going strong among parapsychologists. Indeed, any fact can be "read" or "interpreted" in a number of ways—i.e. explained by alternative hypotheses. This is one reason that only hypotheses harmonizing with several other hypotheses are worth being investigated. This is not the case with the parapsychological hunches: they do not form a (hypothetico-deductive) system and they do not agree with science. (Recall point (iv).) Moreover, parapsychologists themselves are proud of investigating phenomena, or rather pseudophenomena, that they regard as paranormal for lying beyond the reach of "official" (i.e. ordinary) science.

(x) *Changeability.* Parapsychology cannot be said to be moving fast, the way every genuine science does these days. In fact it is a collection of archaic beliefs that go back to primitive animism. Parapsychologists keep retesting the same hunches over and over again without ever obtaining any conclusive results.

In conclusion, parapsychology is a pseudoscience paragon.

Finally, pseudoscience should not be mistaken for either mere scientific error or unorthodoxy. Science does not have the monopoly of truth: the telephone directories of New York City contain more true statements than all of the social sciences put together—but they happen not to be results of scientific research. While science pursues truth it is not characterized by final truth but rather by—among several other traits—corrigibility. Not so pseudoscience, which is a body of beliefs upheld in the face of either lack of evidence or negative evidence. True, pseudoscientists, like anyone else, accidentally hit on true hypotheses, but they do not investigate them scientifically. Thus Joseph Gall was right in holding that mental functions are discharged by special subsystems of the brain. But there was no experimental evidence for the particular cerebral map he proposed, and therefore no justification for the tenacity with which phrenologists clung to it instead of subjecting it to experimental tests. The same holds for a couple of psychoanalytic hypotheses that may yet turn out to be confirmed by scientific psychology.

Nor should pseudoscience be mistaken for scientific unorthodoxy. In science dissent and controversy are normal and healthy, whereas in pseudoscience they are rare and treated as punishable heresy. Scientific unorthodoxy is just unconventional science; technological unorthodoxy is parallel. Field theory was unorthodox when first proposed because it disagreed with the then dominant action at a distance theories; but it had all the marks of science and it was rife with testable hypotheses and stunning new experiments. Likewise telecommunications, aviation and computer science. All scientific and technological revolutions are, to borrow Isaac Asimov's apt expressions, *endoheresies*—deviations within the S&T system—to be sharply distinguished from *exoheresies* or deviations at variance with S&T. Whether proposed by ordinary members of a research community or by outsiders, a new item of knowledge that fits our definition of science or technology, even though it happens to conflict with some (never all) of the items in the specific background or in the conventional fund of knowledge, qualifies as an endoheresy. Endoheresy, though often resisted, should be welcomed in science and technology, exoheresy should be fought. To paraphrase St. Paul, there is no salvation outside the S&T system.

4.2. *Ideology*

An ideology is a belief system (Ch. 2, Section 3.1) composed of sweeping factual statements and value judgments. We distinguish two genera of ideologies: worldly and unworldly (religious), and two species of the former—world views and sociopolitical ideologies. While everybody holds some *Weltanschauung* (world view) or other, not everybody has religious beliefs or a sociopolitical ideology. Let us examine the various kinds of ideology and the two main epistemological problems they pose, namely their claim to knowledge and their relations to science.

A *world view*, i.e. a body of extremely general beliefs about nature and man, is usually a mixture of deep thoughts and platitudes, well confirmed generalizations and superstitions. In its popular version it often comes in the form of maxims, stories and parables. It is possible to cleanse and systematize any such mixed bag: the result is a philosophy. Whether in the form of a more or less tacit, untidy and inconsistent popular belief system, or in that of an explicit, well-organized and consistent philosophy, a world view is bound to have some cognitive content if only because it concerns the world and our behavior. On the other hand the value judgments and moral norms included in a world view are noncognitive, even though they can be analyzed and even justified in the light of knowledge.

We have argued in Section 2, and also in the Introduction to Vol. 3, that science and technology include certain ontological, epistemological and ethical principles. Hence science and technology may be said to have their own ideology. However, this ideology is not dogmatic: it is being tested and updated by the practice of scientific and technological R&D. Hence it is not ideological in the pejorative sense of the term. (And the rather fashionable claim that science is the ideology of capitalism betrays an utter ignorance of science as well as of the fact that genuine science is roughly the same in all industrialized nations, whether capitalist or socialist.)

A *sociopolitical ideology* is a special kind of world view, namely one concerning only society. And, unlike a general world view, which may be politically neutral, a sociopolitical ideology usually favors the interests of some social group or other, such as a social class or a political party. Beliefs composing a sociopolitical ideology may be grouped into four kinds (Bunge, 1980b): (*a*) *ontological theses* concerning the nature of human individuals, groups, and societies—e.g. answers to such questions as whether persons are spiritual beings, how they get together to form social systems, what makes them conform or rebel, and what are the essential traits of society and its history; (*b*) theses concerning the *economic, political and cultural problems* faced by societies of a given type at a given time; (*c*) *value judgments* about persons and their activities, races, classes, institutions, etc.: in sum, what is good for the individual, the group, or the community; (*d*) *action (or inaction) programs* for the solution (or conservation) of social problems and the attainment (or thwarting) of individual or societal goals.

The first two sets of theses constitute knowledge, sometimes false, but still knowledge. In particular, group (*a*) belongs to some philosophy, whereas group (*b*) is or may become part of social science. On the other hand the value judgments and plans of actions are noncognitive. However, they can, nay ought to, be discussed in the light of scientific knowledge. An example will clarify this point.

Suppose we ask how the Canadian *métis* live and, in particular, what their economic, political and cultural problems are. An a priori answer is likely to be ideological in the pejorative sense of the word. But the question can be investigated scientifically by a team of anthropologists, sociologists and political scientists. Such a team is likely to find that the Canadian *métis* live marginally in poverty and ignorance. Thus, scientific research can confirm or refute ideological assertions concerning social conditions. But if we wonder whether the condition of the Canadian *métis* is fair, we ask for an ideological response—e.g. an inhumane and obscurantist affirmative answer or a human and enlightened negative answer. And no sooner have we admitted the latter than we must ask the further question: Can anything be done about the marginality of the Canadian *métis*, and if so, what? This is an ideological question that poses a political problem.

Now, political problems can be handled ideologically or scientifically. The former procedure consists in adopting some strategy suggested by precedent, intuition, unexamined beliefs, or vested interests. The scientific method consists in designing a social program aimed at solving the problem, in the light of a careful scientific study of the situation. Such study would include not only information about the present condition of the Canadian *métis* but also about the way they would like to live, and the available resources. However, even the most carefully designed social program would fail miserably if it were concocted by well-wishing bureaucrats in Ottawa, out of touch with the *métis*. Successful social programs involve an active participation of the interested party, not only in its implementation but also in its very design. (This again is a result of social research and practice: see Ackoff, 1974.) In sum, some ideological questions can be given scientific answers.

Our example suggests that there are two kinds of sociopolitical ideology: nonscientific and scientific. The former is characteristically dogmatic, incapable of learning from defeat, hence utterly alien to scientific and technological research—ergo anachronistic and utopian. A *scientific ideology*, on the other hand, would be realistic by being inspired by science and by inspiring applied science research as well as sociotechnological design. However, such a possible ideology is still to be built.

A sociopolitical ideology could then join with science to study and solve social problems. But in fact many sociopolitical ideologies have been openly hostile to science or to technology, whereas others have produced grave distortions in the latter. The extreme cases of Nazism *vs.* modern physics, and of Stalinism *vs.* modern biology, are too well known to need comment. A less well known example of the influence of ideology on science is that of the various schools of economic thought: "the mercantilists were the champion of the overseas trader; the physiocrats supported the landlord's interests; Adam Smith and Ricardo put their faith in the capitalist who makes profits in order to reinvest them and expand production. Marx turned their arguments round to defend the workers. Now, Marshall [the greatest of the neoclassical economists] came forward as the champion of the rentier" (Robinson and Eatwell, 1974, p. 39). And, we may add, Milton Friedman pushes monetarism in defense of the near-sighted super-rich and against everyone else.

Nor is ideological influence limited to the social sciences, as the following examples picked at random will show. Spokesmen for the chemical industry defend the hypothesis that cancer is entirely a matter of life style: that it is never caused by the carcinogens released by certain industries and the internal combustion engines. The antipsychiatrists hold that there are no mental disorders but only social ills. Vulgar Marxists claim that every idea has emerged as a product of definite socioeconomic circumstances, and that theories are validated only by social practice. And the partisans of the counter-culture hold that "ethnoscience" (the cognitive part of folklore) is no worse than science. Their favorite argument is the pharmacological knowledge of primitive communities. But a recent study of the Food and Agriculture Organization has disclosed that, out of 20,000 plant species used in folk medicine, only about 200 are effective.

Finally we come to *religion*, i.e. the system of beliefs and practices concerning supernatural agencies and our relations to them. Such beliefs are dogmas, i.e. principles above criticism, and the associated practices are rites of various kinds, mainly rites of worship and purification. Common to all religions is the belief that there are unworldly entities, such as gods and devils—whether purely spiritual or carnal—immortal and endowed with superhuman powers, as well as imperceptible to all but a few chosen individuals, so that their existence must be admitted on faith. The systematic study of such entities is called 'theology', once the very center of intellectual culture. From an epistemological viewpoint the interesting questions with regard to religion and theology are whether they constitute knowledge and whether they are compatible with science.

If knowledge is defined as justifiable true belief, as is commonly done,

TABLE 14.3
Main traits of science and religion, technology and magic.

Component	*Science*	*Religion*	*Technology*	*Magic*
Community	Research community	Church	Tech. community	—
Society	Any modern society	Any society	Any modern society	Any traditional society
Domain	The entire world	Nature and supernature	The manipulable world	Nature and supernature
General outlook	Naturalist ontology, realist epistemology, ethics of free search for truth	Supernaturalist ontology, any epistemology, ethics of acceptance and defence of dogma	Naturalist ontology, opportunistic epistemology, utilitarian ethics	Supernaturalist ontology, instrumentalist epistemology, no ethics.
Formal background	Logic and mathematics	Optional	Logic and mathematics	—
Specific background	As needed	—	As needed	—
Problematics	All cognitive problems concerning the real world	All problems concerning nature and the supernatural	Problems of design and operation	Problems about the control of nature or propiciation of supernature
Fund of knowledge	Growing body of test-factual knowledge, in particular laws	Slowly changing body of dogmas and opinions	Growing body of practical knowledge	Stagnant body of beliefs
Aims	Understanding and predicting with the help of laws	Personal salvation, defense of social status quo	Control with the help of laws	Control
Methodics	Scientific method and scrutable techniques	Revelation, authority, exorcism, incantation, prayer	Scientific method and scrutable techniques	Incantation and exorcism

then theology might qualify as a mode of knowledge—knowledge of the supernatural and of our proper attitude to it. Indeed religious and theological beliefs would be justified (*a*) by revelation to some privileged individuals, such as Moses and Mohammed, and (*b*) by certain canonical texts such as the Bible and the Koran. But we have rejected such definition of "knowledge" (Ch. 2, Section 3.2) because it is possible to know beliefs one does not share, and to believe in items that one does not really know—such as life after death. Moreover we do not admit paranormal modes of cognition (e.g. revelation when in a state of grace) as sources of knowledge, or authority as a valid mode of certifying beliefs. A belief accepted without positive empirical evidence together with the support of the bulk of scientific knowledge is merely a dogma. And dogmatism is not just alien to science, technology and the humanities, but is inimical to them. Therefore religion and theology are methodologically at odds with science, technology and the humanities.

And yet the founders of modern science in the 17th century, almost to a man, from Galilei and Kepler to Boyle and Newton, were sincere albeit somewhat heterodox Christians. Does this not prove that science and religion are compatible? Not at all: it is a mere argument *ad hominem* on the same footing as a commercial exhibiting an athlete endorsing a brand of beer or tobacco. All it shows is that consistency of one's total system of belief is hard to come by, particularly in transition periods and in the midst of a society where the church wields a formidable cultural and political power that can be challenged only at the risk of one's life. The question of the compatibility of religion and science is a matter for methodology, not for history or biography. We wish to know whether the two are compatible *de jure* regardless of the compromises that individuals may work out.

This methodological problem can be solved only by comparing the respective frameworks of religion and science. This is not the place to do so in detail. Table 14.3 must suffice here. It shows that religion is incompatible with science and technology. It is not only that religion is dogmatic rather than critical, and that it accepts revelation but has no use for experiment: if this were all, one might think that the two, though very different, are not mutually contradictory. But they *are* mutually contradictory, for there is no room for supernatural entities in the naturalistic world view that underlies science and technology (Section 2). And this is not a small matter, for it dictates the way some questions are approached and answered. For example, the monotheistic religions tell us that miracles are possible, whereas science and technology recognize only laws and man-made rules;

they hold that the universe was created by God, whereas science admits no creation *ex nihilo*; they maintain that the biospecies have not evolved or, if they have, they have done so under divine guidance rather than by genic changes and natural selection; they maintain that the soul or mind is an immaterial and possibly immortal entity, not a brain function; they claim that "the true religion" originates in God not in man; and, what is worse for the advancement of knowledge, they regard all questions of origins as mysteries not to be tampered with, so that in effect they block research into the most interesting questions.

With the decline of the temporal power of the church in the most advanced countries such theological restrictions on inquiry gradually lost their force to the point that religion, rather than being the supreme judge in intellectual matters, became a subject of scientific research and sometimes even a political defendant. Thus the history, sociology and psychology of religion have found that the supernatural is but a figment of the human imagination, so much so that nearly every society, at least since the Neolithic revolution, has created its own religion or has modified the religious beliefs borrowed from other societies. Furthermore, it has been shown that in many cases the Olympus of a religion is a delayed representation of the social structure (Frankfurt *et al.*, 1946). So, science does have something interesting to say about religion, namely that its myths are in the same epistemological category as those of Aesop's and Disney's fables. The main difference between religious myths on the one hand, and poetic or cinematographic fictions on the other, is social: the former used to discharge some social function (cohesion) or dysfunction (maintenance of privilege).

Humbled by the sensational triumphs of the science of Galilei and Darwin, the authorities of the three monotheistic religions stopped attacking science and tried to reach an accommodation with it. Barring some fundamentalist fanatics, contemporary theologians claim that science and religion are mutually complementary rather than exclusive, and that the condemnations of Galilei and Darwin were merely human errors. According to some, they are compatible because they deal with different questions or "different levels of reality"; to others—in particular Pope John Paul II—because both derive from God. (See, e.g. Barbour (1966, 1968), Bube (1968), Schilling (1973), Schlesinger (1977), MacKay (1978), and the journal *Zygon*.) At least three strategies have been tried to reconcile religion with science: watering down religious dogmas, trying to restrict the reach of science, and distorting the latter.

The first strategy, favored by liberal theologians, consists in abandoning the literal reading of the sacred texts and regarding them as allegories or as what they are, namely fantasies concocted long ago by ignorant men. In particular the book of Genesis, so obviously at variance with scientific knowledge, is brushed aside as a mere fable. But then there is no revealed dogma left, everyone is at liberty to believe what he wishes, and the church is deprived of a firm doctrine as well as of divine authority, as a consequence of which it loses unity and power. Therefore this move is resisted by the fundamentalists, who realize that religion is inimical to free thinking, and who have no patience with the intellectuals who are unable to confess to themselves that they are deists, or perhaps even agnostics, rather than theists.

The second reconciliation strategy consists in holding that "The scientific exploration of the universe, as our late Holy Father [Pius XII] so often emphasized, is good, but it attains its full significance only when it reverently respects God's overlordship. It must be carried out with humility; there must be a real ascesis of knowledge..." (McMullin, 1968, p. 42). In other words, scientists are not to ask any basic questions that may embarrass theologians, and they may not publish any findings contrary to religious dogma. They must be not just modest—i.e. aware of their own ignorance—but humble, i.e. they must undertake only small research projects, leaving the important problems, such as those concerning the evolution of the universe, the origin of life, the nature of mind, the origin of religions, and the social functions of the churches, to the theologians. But of course scientists are naturally curious, so that they do not acknowledge easily any external constraints on their research. This is why they entrench, in the very ethics of scientific research, the right to inquire and question without regard for authority: they are, literally, free thinkers.

The third strategy—accommodating science to religion—is just as unlikely to win the acceptance of scientists, or at least of scientific leaders, for it involves profound distortions of contemporary science. Thus it may involve misreading quantum physics as asserting that it views matter as "manifesting mental, personal and spiritual activities" (Peacocke, 1971, p. 184)—an interpretation of quantum physics that not even the most enthusiastic partisan of the Copenhagen interpretation would subscribe to. It may also involve identifying the beginning of the present phase of the evolution of the universe, i.e. the hypothetical "big bang", with the divine *fiat* (Jaki, 1974; Jastrow, 1978)—a miracle violating all the conservation laws of physics, to say nothing of the naturalistic ontology underlying astronomy. And the accommodation certainly involves claiming that, if biospecies have evolved, then they have done so under divine guidance rather than by themselves—a travesty of evolutionary biology. In turn this entails ignoring that most of the biospecies that there ever were have become extinct—a holocaust difficult to reconcile with the dogma that every creature has been created to some higher purpose by an omniscient and loving Creator.

We conclude that none of the three reconciliation strategies work: that science and religion encroach on one another rather than occupying different territories, and therefore are bound to clash at critical points. Therefore whoever wishes to form a comprehensive and consistent world view must opt for either of them. And whereas those who opt for religion are bound to fight science sooner or later—as St. Paul and St. Augustine did—those who opt for science must be on their guard to prevent religionists from curtailing or distorting science.

To sum up, ideology is at the very core of any culture, so it would be foolish to disregard it and, in particular, to ignore the more or less subtle ways in which it can guide or misguide science and technology. The non-religious ideologies have a cognitive content in addition to a valuational one, and that content should be rendered explicit and compatible with science and the public interest—or else dropped. On the other hand there is no such thing as religious knowledge: there is only religious belief. But there is of course some knowledge of religion and, in general, of ideology—namely the psychology, sociology, and history of ideologies. So, whereas religion ignores science, science knows religion.

In sum, there is illusory knowledge—in particular pseudoscience and nonscientific ideology—alongside genuine knowledge. When backed by a mass movement or by a government, an ideology can be much more powerful than any pseudoscience or any genuine science. In fact, an ideology can galvanize or paralyze an entire society; and, if obscurantist, it can kill entire branches of science and the humanities. Hence the need for scientists and humanists never to lose sight of ideology, to study it and to keep it in check.

The prescientific ideologies, in particular the great religions, used to provide unified world views where every thing and every event had its place and purpose. By destroying such world views science and secular philosophy have created a vacuum. This vacuum is often filled with an assortment of short-lived cults and pseudosciences. Yet at the same time modern science and philosophy have supplied the building blocks for a new, consistent, largely true and inspiring world view—a world of lawfully changing concrete things that can be known and controlled up to a point.

Modern man is therefore confronted with a trilemma. He may retain a traditional world view and reject science altogether, but at the price of remaining totally alienated from modern culture. Or he may try and fashion a dualistic world view complete with matter alongside deities, laws alongside miracles, souls alongside thinking brains, and blind faith alongside open-eyed inquiry. Or, finally, he may restore the lost unity by adopting the scientific world view, which makes room for the wondrous but not mysterious variety, complexity and variability of the world and our knowledge of it.

5. Concluding remarks

There are many fields of knowledge but they can be grouped into ten genera: ordinary knowledge, prescientific technics, pseudoscience, basic science, applied science, technology, the humanities, the sociopolitical ideologies, the arts, and the religions. Each such knowledge genus has its own material and conceptual framework. And each conceptual framework allows one to formulate and investigate certain problems, some of which cannot even be stated in alternative frameworks. (Try to pose a mathematical or physical problem in terms of ordinary knowledge or of religion.)

The various conceptual frameworks have sometimes been regarded as mutually compatible or even complementary. An early version of this view is the medieval doctrine of double truth, philosophical and theological, which proved to be nothing but a trick to gain some freedom of inquiry. A later version was that of James Frazer, of *Golden Bough* fame, according to whom magic, religion and science are continuous for being so many attempts to explain and control the world. A more recent view is the so-called "principle of tolerance" (Carnap, 1950), according to which each framework stipulates its own criteria for the reality of the entities it postulates, so the choice among alternative frameworks is a matter of expediency and fruitfulness rather than truth. An updated version of this view of multiple truths, or rather of nontruth, is epistemological anarchism, according to which science is no better than pseudoscience or religion (Feyerabend, 1975).

To be sure, some conceptual frameworks are mutually compatible with one another. For example, the conceptual frameworks of the plumber and the engineer, of the realist novelist and the sociologist, and of the scientific philosopher and the basic scientist are mutually complementary and even partially overlapping. But others are not. For instance, magic is incompatible with technology, faith healing with medicine, existentialism with logic, psychoanalysis with experimental psychology, and science with ideological or religious dogma. Not only do certain fields compete with others, but some of them are superior to their rivals. For example, magic, religion and pseudoscience are inferior to science and technology as modes of knowledge and guides to action because they do not involve research and do not possess error-correction mechanisms such as analysis and experiment. This is why they have not contributed to shaping the modern world view, which is entirely indebted to science and technology. In short, science and technology are by far the best modes of knowledge and guides to action so far devised by man.

When asserting the superiority of science and technology we do not imply that they are perfect or blameless. Both are and will always remain incomplete (but perfectible); science contains a few pockets of dogma and pseudoscience (but we can spot and eradicate them); and technology is often misguided by immoral goals (as a scientific research into some R & D can reveal). The first point (incompleteness) is obvious, since inquiry is of the very essence of science and technology. If perfection were attainable in these fields, they would become extinct, for they consist in discovering and even making gaps, and in filling them. The second point (dogmatism and pseudoscience) is less obvious. "However alien to science, and not widespread there, dogma *de facto* sometimes infiltrates the realm of research [...] Legitimate disagreement or controversy creates dogma when arguments are no longer listened to" (Öpik, 1977). The tenacity with which some physicists cling to the positivist interpretation of quantum mechanics, and sometimes even attempt to supress dissent, is a case in point. As for the pseudoscientific pockets in science—or rather in the activity of some scientists—suffice it to mention applied catastrophe theory (criticized by Zahler and Sussmann (1977)) and the sociological theories criticized by Andreski (1972). Finally, we all know that not every technological project is undertaken in the public interest. However, unlike ordinary knowledge and the various belief systems, science and technology have the resources for detecting and correcting errors. And the citizens of a genuine democracy have the power to prevent the misuses of technology.

The superiority of the scientific and technological modes of knowledge over all others entails that the former are entitled to pass judgment on alternative modes of knowledge. However, the superiority concerns modes of knowledge, not necessarily bits of knowledge. Thus the farmer may occasionally make more accurate weather forecasts than the meteorologist; the mason may correct the mistakes of the engineer; and the poet may see deeper than the psychologist. But the scientist and the technologist can get to learn more than the farmer, the craftsman or the artist; so, in the end their superior mode of inquiry is bound to yield superior knowledge.

Nor does the superiority of science and technology over the alternative modes of knowledge entail that we can dispense with all of them. We need all seven genera of genuine knowledge: ordinary knowledge and prescientific technics, basic and applied science, the humanities, sociopolitical ideology, and art. But in case of conflict in matters of knowledge the inferior modes of knowledge should defer to the superior ones. In particular, a philosophy marginal to contemporary science and technology, such as Wittgenstein's, or inimical to them, such as Heidegger's, is an anachronism: it has only some historical interest. Philosophy should become increasingly scientific. But we had better leave this subject for the next chapter.

CHAPTER 15

UPSHOT

Knowledge is usually studied in either of four ways: philosophically, psychologically, sociologically, or historically. The philosophical study of knowledge, i.e. epistemology, focuses most of the time on the outcome of inquiry with neglect or even ignorance of that which does the knowing, namely the nervous system of an animal embedded in a society with a definite history.

The psychological study of knowledge, or cognitive psychology, focuses on cognitive abilities, their development and perhaps also their evolution and, in recent times, also their analogies (seldom their disanalogies) with computer information processing. Cognitive psychology is hardly interested in the traditional problems of epistemology and it ignores society as well as history. In the case of the information processing variety of cognitivism, it also ignores the brain.

The sociological study of knowledge, i.e. the sociology of knowledge, investigates the external circumstances of the cognitive process, in particular the nature of the information network and its social matrix. It studies the political and economic circumstances that facilitate or block cognition, and the formal organizations typical of inquiry in modern times. But it is hardly interested in the remaining aspects of the problem of knowledge; it particular it disregards the nervous system and the mechanisms of inquiry, from problem finding through hypothesis framing and data collection to the evaluation of either.

Finally the historical study of knowledge—i.e. the various intellectual histories, such as the histories of philosophy, science, and technology—investigates the evolution of knowledge over the last few thousand years. If deep, it treats the history of knowledge as one aspect of social history. If superficial, it is limited to unveiling particular sequences, such as "*A* taught *B*, who taught *C*, and so on", with disregard for individual motivations and talents as well as for social circumstances.

It should be obvious that every one of these approaches delivers a ghostly product. The first produces an image of knowledge without a knowing subject; the second shows us a knowing subject in a social vacuum and perhaps also with an empty skull; the third depicts a society of brainless individuals pushed and pulled by social forces; and the fourth yields a movie of shadowy individuals or, worse, a catalogue of achievements.

Every one of those partial approaches to the study of knowledge tells us something, though something that makes no sense except in relation to the other three stories. These various partial approaches to the study of knowledge are then mutually complementary rather than exclusive. Indeed the cognitive abilities and activities of the individual knowing subject cannot be adequately understood while ignoring his nervous system and its development, the society he was born into, the nature of the inquiring community he belongs to, and the traditions of the latter.

Therefore only a merger of all four approaches to the inquiry into inquiry—namely the philosophy, psychology, sociology and history of knowledge—can produce an adequate representation of knowledge. This does not entail that there is no place for the philosophical, psychological, sociological, or historical specialist in knowledge: it does entail that none of these specialists should ignore the others or hope to do the entire job by himself. In particular, it is all right to philosophize about knowledge, i.e. to do epistemology, but provided one becomes reasonably familiar with some of the findings of the psychology, sociology, and history of knowledge.

In the preceding chapters we have done some philosophy and psychology of knowledge. In the first section of this chapter we shall have a quick look at the social sciences of knowledge. In the next we shall examine the main arguments pro and con the various epistemological schools. And in the subsequent section we shall gather together a number of maxims, resulting from our preceding work, and constituting a *précis* of our own epistemology and methodology.

1. Social sciences of knowledge

1.1. *The Descriptive Social Sciences of Knowledge*

Social science can be descriptive ("positive") or prescriptive (normative); in the latter case it is called a policy science. In this section we shall peep at the five descriptive sciences of knowledge: anthropology, sociology, economics, politology, and history. We shall examine knowledge policies in the next section.

The *anthropology of knowledge* studies the peculiarities of communities of inquirers. It is the newest branch of anthropology, that fascinating protoscience. Nearly all of the anthropological studies on communities of inquirers are marred by an extreme externalism, i.e. a total disregard for what makes inquirers tick: the problems they tackle, their motivations and their values. The externalist student usually ignores what his subjects do and is naive enough to believe that he can find it out by studying their behavior. Typically he is a social scientist who studies what goes on in a chemistry (or biology or engineering) laboratory without knowing any chemistry (or biology or engineering). So, his doings are necessarily limited to recording the manipulations of his subjects as well as their small talk. He has no adequate idea of their problems, background knowledge, or methods. Consequently his report is bound to be similar to the deaf music critic's or the blind plastic arts critic's.

The anthropologist intent on studying scientific research or technological design without some background in either is in a far worse situation than when confronted with the rites performed by a primitive tribe. At least he can learn the native language and be able to understand the functions of the ceremonies with the help of his informants, for even the most complicated ceremony is simpler than the simplest scientific theory. Not surprisingly, the invariable result of such externalist anthropology of knowledge is that research and design, far from being the work of interacting individuals, appears as the mysterious result of a collective action. Worse, on occasion facts are said to be constructed by scientists rather than studied by them (Latour and Woolgar, 1979; Knorr, 1982). An Amazonian Indian asked to describe the activities of the anthropologist in residence is likely to do better.

The *sociology of knowledge* investigates the ways society stimulates or inhibits the development of the material and conceptual frameworks of the various fields of knowledge. In particular, it studies the ways social pressure distorts perception, preserves obsolete ideas, and discourages innovation. It investigates the reception—favorable, indifferent, or hostile—of new ideas and practices, in particular the fate of "premature" ideas. It also studies the reciprocal influence of ideas on society—e.g. the impact of science on religious attitudes, and of technology on industry and trade and, through them, on life at large.

The *sociology of knowledge* too has been dominated by externalism, i.e. the view that only social, economic or political circumstances dictate whatever scientists and technologists do or fail to do, irrespective of their peculiar background, competence, and interests. This view, *sociologism*, is not only superficial: it can also be seriously misleading. One good example of the ridiculous extreme to which it has been taken is the thesis, of Platonic and Heglian roots, that individual scientists do nothing but capture and develop the *Zeitgeist*, i.e. the ideas inherent in a given society Thus according to Hessen (1931), Newton's *Principia* were not so much a product of Newton's own brain as "the product of his period". A more recent perversion of the same kind is the claim that the problems of the nature of the mind and of social change cannot even be approached from our present "paradigms", which would be those of the capitalist society. But the original sin was committed by the Marxist authors who generalized their correct statement that some economic theories are bourgeois, to the thesis that the whole of contemporary culture is divided into two irreconcilable camps: bourgeois culture and proletarian culture. Fortunately this dogma seems to have been forgotten. But the main theses of sociologism, namely that entire societies, not individuals, are the knowing subjects, and that the social structure determines the content and validity of knowledge, are still going strong—though supported only by quotations, never by research.

The *economics of knowledge* studies research communities as components of the knowledge industry, and their outputs as commodities. It investigates the half life of technological innovation and its importance as an input to industry and the services. It studies the way nascent industries and services stimulate discovery and invention, and the way vested interests (in particular large corporations) block innovation.

Another central problem in the current discussions among the economists of knowledge is the place of science and technology in society. According to some, knowledge is only a commodity and a factor of production alongside land, energy, capital, labor, and management. (Curiously enough this thesis is shared by the Chicago school, in particular Milton Friedman, and numerous Marxists, who place knowledge in the so-called infrastructure.) This thesis involves a confusion between science and technology as well as a rigid separation between economy and culture. To be sure, some science is applied, and some applied science is an input to technology, which in turn is an input to production—but so are ideology and politics. What is characteristic of inquiry is not that some of it affects the economy but that it may produce *new knowledge* rather than goods or services. In short *some* knowledge—artisanal and technological knowledge—is economically valuable and may therefore be regarded as a merchandise and be priced. But this holds only for a small fraction of the totality of human knowledge: remember that only about 1% of the inventions get patented, and only about 1% of the patents are ever used in production or in the services.

The *politology of knowledge*, a nearly nonexisting discipline, ought to study the relations between the inquiring communities and political power. In particular it ought to study how applied science, technology and the humanities can supply intellectual ammunition for either the preservation or the alteration of the *status quo*; how political power propels or inhibits, guides or misguides scientific, technological and humanistic research; how investigators and educators are encouraged or frightened by political conditions; what are the social responsibilities of the workers in the knowledge industry—e.g. what they can do to enlighten the public and lobby the parliament concerning the arms race, and so on. So far most of the politology of knowledge has been the amateur work of concerned scientists, technologists and educators writing in the *Bulletin of Atomic Scientists* or debating in the Pugwash conferences. The field, tough of paramount importance, is still in its infancy.

On the other hand the *history of science and technology* is the dean of the social sciences of knowledge and therefore so far the most useful of all to the philosopher of knowledge. Still, it is beset by a number of conceptual difficulties. For one thing, until recently it has been frankly partisan—e.g. trying to prove that all research proceeds inductively, or that modern science was born from Protestantism, or from the Industrial Revolution, or even much earlier, from theological disputations in the 14th century. Another characteristic of the field until about 1960 was its extreme internalism, that ignored the larger social and cultural context and even the importance of formal organizations. Over the past few decades the pendulum has swung in the opposite direction, to the point of often ignoring the internal motor of research, i.e. curiosity. And, whether externalists or internalists, most historians of knowledge are still to make up their minds regarding certain key problems. Let us list a few of them.

One such problem is the very origin of modern science and technology: when, where and why did they emerge? Many historians of knowledge do not seem to realize that the answer to this question depends critically upon the definitions of "science" and "technology", which is a philosophical matter. If mere observation, i.e. fact gathering, is meant by "science", then science emerged with the first mammals and birds; if systematic observation handed down by tradition, then the Neolithic peoples or at the latest the Babylonians and the ancient Chinese should get the credit; if the blending of observation and reason in the search for laws, then the Greeks of the classical period; if experiment is added, then we must wait until the beginning of the 17th century. The case of technology is parallel. In any case it makes no sense to rush to answer the question of the origins of X before agreeing on a characterization of X.

A second, allied problem, is who is to count as a scientist or a technologist: anyone who handles scientific or technological problems, anyone who publishes his solutions to such problems, or what? Take for example Boscovich, highly praised by the 1801 edition of the *Encyclopaedia Britannica* and by later scholars such as L. L. Whyte, K. R. Popper and M. Hesse, who have held that Boscovich (*a*) is the immediate forerunner of the later field and atomic theories, and (*b*) his system is an alternative to classical mechanics. Yet a reading of his main work, *Theory of Natural Philosophy* (1758), will persuade any physicist that both claims are mistaken. Indeed, Boscovich's hypothesis that the ultimate constituents of matter are centers of force does not establish a field theory: he did not state any properties of the field, let alone properties of the interactions between fields and bodies. He had neither empirical evidence for his conjecture nor the mathematical tools (partial differential equations) for formulating it. Nor was Boscovich's theory a viable alternative to the Newton-Euler mechanics, for it did not contain any equations of motion, and it postulated a single force law (which, if plugged into Newton's laws, yields no solution in closed form for arbitrary distances). In short, Boscovich's work was not a theory proper, it made no testable predictions, and it was at variance with the best science of his time: it was a clear case of pseudoscience rightly ignored by physicists and textbooks. Why then has it been so highly regarded in our own time by some historians and philosophers of science?

A third problem that often plagues historians of knowledge is whether every discovery or invention is necessary, i.e. unavoidable given the conditions at the time it was made, so that if X had not made it then someone else would have done it. Of course such a statement is untestable. We only know that some discoveries and inventions are multiple, but this can be explained by (more or less fortuitous) contemporaries sharing a conceptual framework and struggling with the same problem. The invention of special relativity and the discovery of the double helix structure of DNA are cases in point. In both cases a number of talented people were on the trail, and if the actual winners had not existed someone else *might* (not *would*) have won the race. But this does not apply to the invention of the general theory of relativity or to wave mechanics. These are cases of a one-man race against himself—Einstein in the former case, de Broglie in the latter. In these cases the problem was new, it interested nobody else, and the solution was not only totally unexpected but also fruitful. Moreover society provided no direct input in these cases: it just allowed the investigators to follow their own noses. But the results had eventually a profound and lasting impact on science and, in the case of wave mechanics, even on technology and therefore on industry. It would be extremely artificial to invent a causal chain so rigid that its central links, Einstein and de Broglie, could have been replaced by anyone else.

A fourth problem that still eludes historians of knowledge is the strength of the links between knowledge and society. Take that admirable European 17th century, the cradle of modern science, technology, and philosophy. It was anything but a prosperous and peaceful period: it was a time of economic stagnation and even retreat; it was marked by a number of wars, particularly the savage Thirty Years war, which involved France, Spain, Sweden and particularly Germany, a land which remained totally ruined; and it was a period of witch hunting and burning of heretics on the part of both the Catholic and Protestant churches. Therefore the explanation of the rise of science and technology in that century cannot lie in society, which barely tolerated that splendid knowledge explosion. The explanation must be sought in the history of ideas, beliefs, and attitudes in the immediately preceding period: in the open, adventurous, optimistic approach to inquiry and life generally, that arose in the late Renaissance. Externalism, in particular economic determinism, cannot explain all this.

Externalism of the radical kind, namely sociologism, cannot account for scientific, technological, or humanistic change in general. As we saw in Ch. 13, Section 3, Kuhn (1962), Feyerabend (1975) and others have claimed that different thought styles or paradigms are mutually "incommensurable", to such a point that a person steeped in one of them cannot understand any views belonging to a different style or paradigm. If this were true then the history of knowledge would be impossible, for every historian is presumably immersed in his own "thought community" different from that of the creators he studies. In short, paradigmatism is suicidal.

In conclusion, neither internalism nor externalism does a good job of accounting for the evolution of knowledge. (See Agassi (1981) for criticisms of both.) Nor should this be surprising to a philosopher, for the internalism *vs.* externalism debate is an instance of another two more general debates in philosophy. One is the ontological problem of what determines the behavior of anything: its internal structure, its environment or both? This debate is alive in biology (geneticism *vs.* ecologism), psychology (nature *vs.* nurture), and elsewhere. Obviously the two extremes are mistaken. Everything but the universe as a whole is influenced by something else, but no external action is effective unless it bears on suitable aspects of the thing influenced. However, before a thing can be influenced by external factors it must exist—and it may have come into existence through a process of self-assembly of environmental items. Systemism solves this debate by conceding as much importance to the individual components of a system of inquirers as to their interrelations and their relations to society at large. (See Vol. 4. Ch. 5.)

The internalism-externalism debate is also linked to the traditional controversy between individualists and holists in the philosophy of society. In fact epistemological internalism sides with individualism, for it holds that knowledge is wholly a matter of individuals—empiricists and rationalists nod. On the other hand epistemological externalism comes together with holism, or the doctrine that knowledge somehow resides in society—in agreement with objective idealism and radical sociologism. The *tertium quid* is of course again systemism, or the view that, although only individuals can know and ignore, inquire and doubt, they do so in society, which now stimulates their epistemic activities, now inhibits them.

The internalist approach to the problem of knowledge can achieve only partial successes because no brain can prosper in a social vacuum. And the externalist approach too is partial because not even the most favorable social conditions can make up for the lack of original brains. Only a synthesis of internalism and externalism, of individualism and collectivism, can be hoped to yield a realistic account of knowledge avoiding the extremes of the isolated knowing subject and the brainless community. Such synthesis views cognition as a process occurring in individual brains every one of which is embedded in a society and steeped in a tradition, thus benefiting and suffering from interactions with other inquirers as well as from the suggestions and constraints of past achievements and failures. Such a synthesis can be produced only by a confluence of the philosophy, psychology, and social sciences of knowledge. We must strive then for the constitution of a single field of knowledge of knowledge: an epistemology naturalized, sociologized, and unified, matching the unity of human knowledge we noted in Ch. 14, Section 3.1. See Figure 15.1.

1.2. *Inquiry Policies*

As long as culture was weak and inquiry was conducted by a handful of isolated individuals there was no need to manage it. This is not the case of modern society, where knowledge is pursued vigorously and pervades all sectors of society, to the point that one speaks legitimately of the *knowledge industry*. Like every other industry, the knowledge industry has a management. The knowledge management is usually mixed: it is performed by professional organizations—such as societies of scientists, engi-

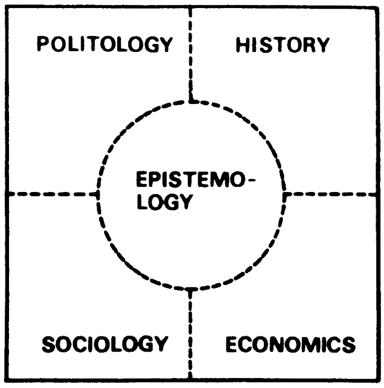


Fig. 15.1. The five aspects of the study of the production, utilization and diffusion of knowledge should be distinguished without being detached. Epistemology is at the center of all the social sciences of knowledge.

neers, or physicians—universities, and whole governmental organizations, not to speak of international bodies such as Unesco.

Like every other management, culture management is supposed to be guided by some policy (or "philosophy" in the vernacular). A policy is a means-ends pair: a set of prescriptions for attaining certain goals with prescribed means, i.e. human and material resources. Like constitutions, policies can be tacit or explicit. A tacit cultural policy is expressed only in practical ways such as the hiring and firing of academic staff in universities and of experts of various kinds in industry and trade; the recruitment (or discouraging) of students; the founding and subsidizing (or throttling) of research projects, museums, libraries, publishing houses, etc. A good unstated but practised cultural policy is better than the best explicit but unpractised policy, which in turn is preferable to an explicit policy aiming at destroying a culture or a sector of it.

Cultural policies, like economic policies, come in two wide genera: noninterventionist ("liberal") and interventionist (planning). The former consist in leaving culture in the hands of private initiative—individual scholars, patrons, foundations, private universities, and the like. In practical terms this means that high grade culture, in particular scientific, technological and humanistic research, is accessible only to wealthy organizations or individuals, for they are the only ones capable of funding such research. This kind of culture freedom—peddled by conservative politicians and economists—is elitist, not democratic. Moreover it curtails freedom of inquiry, for the individual researcher is not supposed to displease his patron. (Remember that Kant's sovereign forced him to take religion seriously.) A democratic society supports its culture on a large scale. And, like any large scale enterprise, it must manage it in a planned way.

Now, planning is double edged: it may stifle or stimulate, according as the planning is authoritarian or democratic. The authoritarian planning of a culture is done from above, i.e. without the active participation of the producers and consumers of cultural goods and services. It is wholly in the hands of bureaucrats or ideologues, who tend to overrate technology at the expense of all the other sectors of culture, and who try to subordinate every cultural activity to the official ideology. Such a planning authority allow creators little or no participation in formulating goals, listing priorities, allocating resources, or even choosing problems and outlining research projects.

Because authoritarian plans are narrow-minded, narrow-sighted, and rigid, they make for mediocrity and inefficiency. This is particularly obvious in the case of scientific, technological, humanistic and artistic creation: the plans for any such activities ought to be outlined by the workers themselves and they ought to allow for sudden changes of technique and even problem. Though obvious, this lesson is still to be learned by many a self-styled democratic government. As Parkinson (1965, p. 116)—of Parkinson's Law fame—notes, "Nowadays, when one country lags scientifically behind another equally prosperous country, the most probable reason is that the government has been telling its scientists what they are to discover. This means, in other words, that too much money has been allocated to specific projects and too little to abstract science. The more resources have been devoted to projects the politician can understand—that is, to the development of discoveries already made and publicized—the fewer resources are available for discoveries which are now inconceivable in so much as they have not yet been made".

A democratic planning of a culture, in particular of its knowledge industry, involves not only freedom of inquiry and teaching but also a balanced stimulation of all the creative sectors of modern culture. Most government officials, particularly if imbued with a narrow ideology, find this difficult to understand: they have no conception of culture, and in particular human knowledge, as a system, and they overrate technology and the services it can render. One must try and teach them that, though technology is indeed central to modern culture, it cannot prosper in isolation from the other branches of culture. One must teach them that advanced creative technology requires basic science; that there is no competitive industry without industrial design, which is an application of art; that there are no effective medical services without medical research, which is applied biology, which rests on basic biology; that social problems cannot be solved without a minimum of sociological research; that there is no science or technology without some philosophy, which in turn interacts with other branches of the humanities; and that there is no cultural creation without an educational system—which must in turn be updated in the light of psychological and sociological research. In short, one must emphasize that the various branches of culture form a system that is intimately linked to the system of production, exchange, and services: that only a systemic vision of culture can help produce an effective cultural policy.

Moreover, given that the clue to success in any social undertaking is active participation (Vol. 4, Ch. 5), a democratic planning of cultural activities will make ample room for such intervention of the producers and consumers of cultural goods and services. Such participation must not be limited to sporadic consultations: the interested parties must belong to the very composition of the culture planning authority. Otherwise the planning will be unrealistic or worse, i.e. stifling.

In short, given the large volume of modern culture, in particular the knowledge industry, culture must be managed on the basis of a plan. A good culture plan (*a*) embraces all the components of the culture system, (*b*) guarantees creative freedom, in particular the freedom of inquiry, (*c*) renders the exercise of such freedom materially possible through generous support of genuine talent, and (*d*) ensures the active participation of the producers and consumers of cultural goods and services in the design of cultural policies and plans as well as in the management of the culture system.

(Whether systemic or fragmentary, democratic or authoritarian, the management of culture, in particular of the knowledge industry, amounts to a control system that regulates the inputs into the culture system. Such control system has a feedback loop that modifies the input rate as a function of the difference between the desired outputs and the actual ones. Assuming that both the inputs or resources and the outputs or cultural goods and services can be reasonably quantitated, the control system can be represented by the following simple-minded mathematical model which neglects, among other things, the time delays, e.g. between problem formulation and problem solution. The model consists of two postulates and their consequences. The first postulate states that the rate $\dot{R}$ of investment of human and material resources is proportional to the difference $D-P$ between the desired value D and the current value P of the cultural production, i.e. $\dot{R}=a(D-P)$. The second postulate is that the rate $\dot{P}$ of production is proportional to the product of P and R, i.e. $\dot{P}=b\,PR$—so that there is neither increase nor decrease in production if there is neither some production to begin with nor some investment. The first factor, a, is a dimensional constant, whereas the second, b, measures the efficiency of the cultural system. The solution to the system of equations is $R^2=(2a/b)\ \ln P^D-(2a/b)P+c$, where c is a constant to be determined by the initial values of R and P. Needless to say, this is only a programmatic model.)

We repeat that cultural planning need not curtail cultural freedom: it will do so only if the plan is authoritarian. Cultural freedom hardly exists when intellectuals and artists depend exclusively on the whim of powerful individual patrons. Nowadays, when culture—in particular knowledge—is mass produced and circulated, often at great expense, cultural planning is indispensable if only to manage its funding. Democratic planning is optimal not only because it can provide the necessary resources to all the components of creative culture, but also because it can protect the individual creators, particularly the more original among them, from the many and assorted foes of free inquiry.

There are two kinds of enemy of free inquiry: external and internal to culture. The former are easily spotted: they are the oppressive bureaucrats, the fanatic ideologists, the pseudoscientists, the military leaders who would like to turn all research into a means of destruction, and the business leaders who would like to restrict inquiry to what promises immediate practical benefits. The internal enemies of free inquiry are less conspicuous but no less obnoxious. They are of three kinds: the researchers who corrupt science to their own personal advantage, the externalist students of knowledge, and the philosophical censors. The former are those who, knowing better, produce low grade, uninteresting and useless results with the sole aim of swelling their *curricula vitarum* and their research grants. Such results, far from improving our understanding or control of reality, produce only an annoying information overload, strain the resources, and make young people bored and disillusioned with the pursuit of knowledge.

The externalist students of knowledge, who are interested only in the social sources and sinks of research, often oppose freedom of inquiry and demand instead that all inquiry have some immediate impact on industry, social services, or government. Obviously only applied science and technology should be assessed by their practical fruits. The main value of basic science, the humanities and the arts is intrinsic not instrumental: they should be judged by their contribution to the enrichment of culture. Therefore, if we wish to save culture we must resist the pragmatist and externalist wind that is sweeping the sciences of science. We must insist that evolutionary biology, cosmology, mathematical logic, and other fields of disinterested research are at least as important as computer science, cancer research and weapons research.

Finally there are the philosophical self-appointed censors of culture. They would like politicians and culture managers to slow down or even suppress all the research that does not conform to their own philosophies. I do not mean just people who disapprove of certain research projects on philosophical grounds, yet take no steps to suppress them. (Classical examples are Comte's disqualification of atomic physics and Bridgman's opposition to field physics.) What I mean is the use of political power, or explicit calls to the powers that be, to interfere in certain research lines or even eliminate them. Two classical examples are Giovanni Gentile's reforms, which all but eliminated the flourishing Italian school of logic, and Zhdanov's attacks on "bourgeois" science and arts. Two more modest and, mercifully, ineffective shows of philosophical censorship of science are Lorenzen's (1967) plea that the state should stop subsidizing abstract mathematics, in particular set theory, and Feyerabend's (1975, p. 216) contention that the social authority of science "has by now become so overpowering *that political interference is necessary to restore a balanced development*" (italics in the original).

2. PHILOSOPHIES OF KNOWLEDGE

2.1. *Rationalism and Empiricism*

By undertaking a philosophical study of knowledge we have tacitly assumed that philosophizing is a mode of knowing: that it may give us some knowledge. (This is far from obvious. Thus irrationalists philosophers do not seek any knowledge, or at least they do not attain any. And Wittgenstein and his followers hold that philosophy is an activity, namely linguistic analysis, to be conceived of as curing the sickness called 'philosophy', so that it yields no knowledge at all—which is true in their case.) And in organizing this *Treatise* we have also assumed that the philosophy of knowledge is among the basic sectors of philosophy: Figure 15.2.

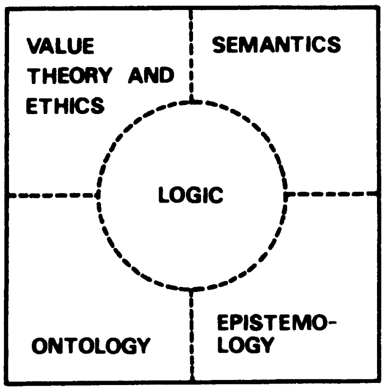


Fig. 15.2. The five sectors of philosophy. Logic is also a part of mathematics and it ought to underlie every branch of philosophy.

As is well known, philosophy, in particular epistemology, far from being homogeneous is divided into a number of schools. Such plurality of schools indicates the hold that tradition has on philosophy, the close links it maintains with ideology, and the rather primitive state of the art. (Controversy is the salt not the meat in matters of knowledge.) In this section we shall review critically some of the living schools of epistemology, note some of their virtues and vices, and place our own theory of knowledge in the map of contemporary epistemology. (For clear discussions of a number of modern theories of knowledge see Blanshard (1939), Hill (1961), and Rorty (1979).)

The two great traditions in epistemology are of course rationalism and empiricism. (Idealism and materialism are ontological schools, each of which can be combined with either rationalism or empiricism.) According to radical rationalism reason, and to radical empiricism experience, is both necessary and sufficient to know what can be known. Rationalism involves the *principle of sufficient reason*, according to which some reason must be given for every statement. Empiricism involves what may be called the *principle of sufficient experience*, on which every statement must be supported by some experience. (Popper and his followers dub *justificationist* anyone who abides by either principle, and they hold that there can be only reasons or experiences against, never for, any ideas. This doctrine, which may be called *negative rationalism*, does not fit the research practice in any field of knowledge: every investigator looks for both positive and negative evidence, conceptual or empirical. Recall Ch. 12.)

Every epistemological school may be regarded as either a special version of rationalism or of empiricism, or as a combination of theses fitting into either of the two great schools. For example, conventionalism is a variety of rationalism, and pragmatism one of empiricism. And kantianism, dialectical materialism, logical empiricism, and our own theory of knowledge are so many syntheses of rationalism and empiricism: Figure 15.3. (Irrationalism does not figure in this diagram because it includes no theory of knowledge.)

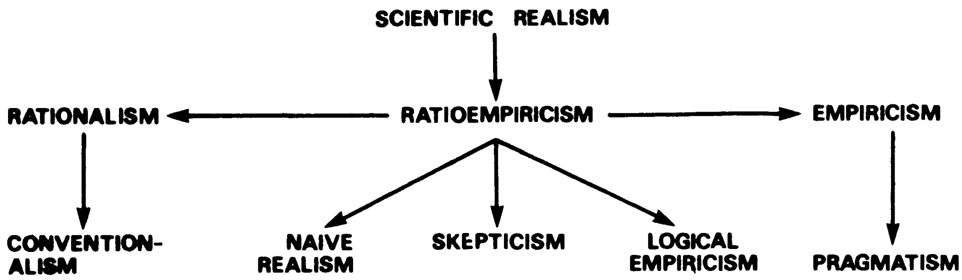


Fig. 15.3. Relations among the main contemporary epistemological schools. The arrow represents the projection operation, whereby a system is reduced to one of its components or one of its versions.

Because of the supremacy it assigns to analysis, theory, proof, and discussion, rationalism is the spontaneous epistemology of most mathematicians and a great many philosophers. Indeed, mathematical and philosophical research are purely conceptual activities—to the extent that these can be dissociated from sensory-motor processes. Rationalism accounts also for the theoretical aspects of scientific, technological and humanistic research. In particular it fits the fact that theory precedes experience—sometimes. On the other hand radical rationalism fails utterly to account for the empirical aspects of inquiry, from perception to experiment: it does not fit the specific activities of the laboratory, field, or workbench inquirer, let alone those of the man of action, for it underrates the value of data and induction; it does not study measurement and experiment—which it regards as, in the best of cases, mere means for weeding out false conjectures; and it exaggerates the importance of deduction and controversy. Finally, the principle of sufficient reason is insufficient: although we ought to give reasons for our hypotheses and even our data, such reasons can be only necessary, not sufficient. More on the inconclusiveness of confirmation and refutation in a while.

Empiricism is the spontaneous philosophy of experimental and field scientists because it extols the virtues of observation, induction, and trial and error. In particular it fits the fact that experience precedes theory—sometimes. Likewise pragmatism—a special kind of empiricism—is the spontaneous epistemology of technicians, technologists, managers and men of action, because of its insistence on the value of practice. On the other hand, because it underrates conceptualization, in particular theorization, empiricism—and particularly pragmatism—is a serious obstacle to theoretical research. And, because of its thesis that every theory is nothing but a summary of data, empiricism also obstructs advanced experimental and technological research, which is guided by theory. Finally, the principle of sufficient experience is insufficient: although we ought to support or undermine our hypotheses with data, these can only be necessary, never sufficient.

We see then that both rationalism and empiricism contain true theses along with false ones. Both reason and experience are necessary yet neither suffices by itself. Kant realized this situation and set out to build a synthesis of rationalism and empiricism. Unfortunately, he combined the bad halves of both. In fact Kant put together the apriorism of rationalism and the phenomenalism of empiricism. (Remember that Kant held that the understanding imposes its laws on nature but that it can know only appearance, not reality. In a way he joined Christian Wolff and David Hume.) Wishing to revolutionize epistemology, Kant effected a genuine counter-revolution.

We have attempted to combine what we take to be the sound halves of the two great epistemological traditions. These are conceptual analysis, theorizing, proof, and discussion, together with observation, measurement, experiment, and praxis—the way Bacon had preached and Galilei practised. The product of this combination may be called *ratioempiricism*. To this we add two other components. One is *critical realism*, which boils down to the thesis that we can build approximately true theories of reality. The other is *scientism*, or the thesis that science is the highest type of knowledge of nature and society, and therefore the best ground for the rational and effective control and enrichment of reality. (More on critical realism and scientism in Section 2.2.) This particular synthesis of rationalism and empiricism, enriched with critical realism and scientism, will be called *scientific realism*. (A few other philosophers call their epistemologies by this name, but they differ from ours in essential respects, particularly with regard to scientism.)

Scientific realism, we claim, accounts for the conceptual components of cognition without being rationalist, and for its empirical components without being empiricist. And it makes room for doubt without falling into skepticism. (We need a strong dose of skepticism to look for errors and be willing to correct them. But we also need to trust the intentions and the veracity of our fellow inquirers if we wish to learn from them. And we must also place some trust, however temporary, in certain guiding principles and methods without which no further research would be possible. Einstein said of Euclidean geometry that it had given the human mind the confidence it needed for its future achievements.)

Scientific realism makes also room for conventions without embracing conventionalism. (Every factual theory involves not only conventions concerning the signification of the symbols it is couched in. It also contains conventions about units and simplifications. Such conventions are necessary to build approximately true representations of real things.) Furthermore, unlike other combinations of rationalism with empiricism—such as kantianism, logical empiricism, and naive realism—our epistemology includes a critical variety of realism. Finally, scientific realism is unabashedly scientistic without being reductionist, in particular physicalist. On the contrary, it acknowledges that every research field has its peculiar concepts and methods, that correspond to the peculiarities (in particular the emergent properties) of its domain or reference class. More in Vol. 6.

Scientific realism is *justificationist* in that it requires every proposition, be it hypothesis or datum, to be ultimately justifiable theoretically or empirically. (A proposition may be said to be *theoretically justified* if it follows from premises in a consistent theory, and *empirically justified* if it is supported by controlled empirical evidence.) However, justification has its limits: it is relative or conditional most of the time. For one thing, the consistency of most theories cannot be proved beyond doubt; and most consistency proofs are relative or conditional, in that they assume the consistency of some other theory. For another, empirical data are just as fallible as hypotheses—but they are equally corrigible and (conditionally) justifiable with the help of some body of (more or less tentative) knowledge. In short, most propositions are not fully justifiable but only conditionally justifiable, and in principle we can always impugn or improve a justification.

Our kind of justificationism is then *fallibilist*, in opposition to the mainstream of epistemology, from Aristotle to Descartes, Husserl, and Dewey, which was always in search of final certainty. Whatever their professed philosophy, scientists and technologists are practising fallibilists, i.e. they believe that theorizing, experimenting, designing and planning are subject to error. But, far from being skeptics, they are *meliorists*: they hope to spot error and reduce it. They do so by rechecking, designing and performing experiments, inviting criticism, etc. So, regardless of how they feel about papal infallibility, scientists and technologists do not believe in their own. (Caution: fallibilism, which surfaced with Peirce, should not be mistaken for falsifiabilism, i.e. Popper's negative rationalism.)

Being fallibilist, our epistemology is not foundationist, i.e. it does not assume that there is a privileged set of unshakeable propositions upon which all the rest can be securely built. (Rationalist foundationism holds that the truths of reason perform that function, whereas empiricist foundationism assigns protocol statements that role, and intuitionism certain allegedly infallible but usually obscure intuitions.) The history of knowledge supplies no evidence for the existence of such a rock bottom. However, this does not entail that there are not even transient foundations. In fact every body of hypotheses (though never of data) can be organized axiomatically, and the set of axioms is rightly called the axiom (or postulate) *basis* (or *foundation*) of that body. What happens is that such basis may not be definitive. Analytical advances may require a reorganization or overhauling, and empirical findings may call for the modification or even rejection of some or all of the axioms. So, *pace* Lakatos (1978, Vol. 2), theories do have foundations, even though these are not final and therefore provide no guarantee of truth. Hence foundations research cannot find certainty but does find order and a greater depth (Hilbert, 1918; Wang, 1966).

Knowledge has no epistemic foundation, whether rational or empirical, but it has a factual or material foundation, namely the real world. Experience is no solid rock but rather shifting sand, and so is theory. In current cognitive practice experience and action mix with theory and analysis, so that neither experience nor reason is supreme or ultimate: each stimulates, supports or corrects the other.

Finally, on scientific realism the traditional subject-object opposition disappears because the knowing (or rather inquiring) subject is seen as being part of the world. Of course one must be able to distinguish facts from ideas about them, but the latter are just further facts, namely those that happen in selected parts of our brains. Scientific realism is thus ontologically monistic in holding that cognition is part of the world—and therefore subject to some of the laws of matter. However, it is pluralistic in that it recognizes a variety of modes of knowledge: ordinary, technical, scientific, technological, and humanistic—though not all of them equally valuable. (Recall Ch. 14.)

(Clearly, the thesis of epistemological monism is ontological, not epistemological. And monism may be idealistic or materialistic, empiricist or neutral: i.e. it may hold that every existent is either ideal, material, experiential, or neither. In line with the ontology expounded in Vols. 3 and 4, we adopt a form of materialist monism. However, this must not be mistaken for semantic materialism, or the thesis that every meaningful statement refers to some chunk of matter. Semantic materialism holds for factual statements but is false of others, particularly the mathematical ones.)

Let us now take a closer look at the two components that distinguish scientific realism from other varieties of ratioempiricism.

2.2. *Realism and Scientism*

By definition factual knowledge is knowledge of facts, i.e. items in the real world, such as states of things and changes in things. The facts that are objects of inquiry can occur inside the inquirer or in his external world, and they can occur spontaneously or as a result of his activity.

Facts are sometimes said to be *hard* in that they do not occur or fail to occur just because we fancy or abhor them. In contrast, our knowledge of facts can be said to be *soft* for being incomplete, at best nearly true, and always corrigible. This distinction between facts in themselves and our images, concepts, or descriptions of them is sometimes blurred in ordinary parlance, as when one speaks of 'getting one's facts straight' when actually meaning "getting an accurate knowledge of the facts that interest one". Occasionally philosophers indulge in this confusion. Thus Feyerabend states that "Facts [probably meaning 'our descriptions of some facts'] contain ideological components" (1975, p. 77), and he rejects the thesis of the autonomy of facts (*ibid.* pp. 38–39).

All scientific and technological inquiry presupposes the autonomy of facts, even of the facts resulting from our own action. (Recall Ch. 4, Section 3.2.) Otherwise, i.e. if we could invent facts, or if we were always unable to distinguish them from fiction, we would not care to check our ideas about them. Error, a mismatch between knowledge and its object, is ever present in cognitive enterprises, and in principle it can always be corrected—if not right now, later on. Knowing that our knowledge of facts may contain errors, and must therefore be checked, presupposes and also confirms the reality or autonomy of the facts in question. (See Bunge (1981), Appendix, for the use of error to criticize subjectivism.). To put it positively: the very search for factual truth presupposes the philosophical theses that there are autonomous facts and that these can be known if only partially. These two theses constitute the nucleus of *realism* or *objectivism*.

Objectivism is the thesis that objectivity is attainable and desirable. Now, objectivity is a property of some perceptions, ideas, or procedures. Actually the word 'objective' designates at least three different concepts, which we shall call 'referential', 'alethic', and 'methodological'. *Referential objectivity* is identical with lack of reference to the knowing subject—as in the statement that there are two people in this room—regardless of its truth value. *Alethic objectivity* is truth for all, as in "I feel pain every time I experience a tooth extraction", which is referentially subjective but alethically objective, for it holds for all the substitution instances of the pronoun. And *methodological objectivity* is reliability together with lack of personal bias in preparing the material of study or performing tests on it, as when using randomization in constructing a statistical sample or conducting studies of performance in evaluating a new artifact or plan.

Natural science, social science and technology are supposed to be referentially, alethically and methodologically objective—i.e. objective *simpliciter*. Objectivity is not easy to attain, particularly when the subject of study is new or one of ideological controversy. Thus it is possible to give a true account of the luminous side of a society while silencing its dark side—a characteristic of propaganda as opposed to social science. A commitment to objectivity is insufficient: we also must strive to account eventually for the *whole* object of our research.

Nor should objectivity be equated with the search for truth. I may truly assert that I am feeling well, and I may claim that all stars are composed of ice. In the first case I state a truth about a subjective state of mind, in the second I am falsely affirming a referentially objective construct. Nor is objectivity identical with the refusal to study subjective experience. Indeed the requirement of objectivity is methodological, not ontological: it entails searching for descriptions the truth value of which does not depend on arbitrary individual fiat, and prescriptions the efficiency of which is likewise independent of fancy. And such research can bear on subjective experience and opinion: witness many psychological and sociological studies of either. (Hayek (1955) confused subjectivism with recognizing the existence and importance of opinions, and consequently declared the social sciences to be subjectivistic.)

Another confusion to be avoided is that between objectivism and impartiality or the refraining from taking sides. If somebody exhibits a certain preference and can somehow justify it in the name of objectivity, then he is being partial without being subjective. For example, anyone engaging in curve fitting prefers order to chaos and hopes that the smooth curve will be truer than the isolated irregular empirical dots. And if a theoretician prefers a deep inaccurate theory to an accurate superficial one, he is not being subjective but partial to deep theories. In either case the scientist is supposed to be able to argue for his preferences instead of taking refuge in dogma. In any case the search for truth involves taking sides for the most promising ideas and methods.

Occasionally scientists forget the requirement of objectivity or even revolt against it. One famous example was von Uexküll's (1928) influential view that "there are as many worlds as there are subjects". He seems to have drawn this conclusion from his own important work on the different ways different animal species perceive their environment. Now, it is true that a scientist and an octopus perceive the world differently, but (*a*) physics, chemistry and biology assume that it is the same world, and (*b*) unlike the octopus, man can become aware of subjective factors and can succeed in overcoming the limitations of perception by constructing objective and even approximately true theories.

Another example of subjectivism is the extreme Copenhagen interpretation of quantum physics, according to which the experimenter "conjures up" all the facts. A related view is Wheeler's (1974) "no-knower-no-world" view, included in his wider "anthropic principle". According to it, the universe was designed and built as man's home—an old religious dogma—so that we are really participants in the foundation and subsistence of the world—presumably a heresy. Of course there is not a shred of evidence for this piece of theological speculation.

The most fashionable vindication of subjectivism rests on a misunderstanding of epistemological realism and a misinterpretation of the recent experimental refutation of Bell's inequalities. Actually these inequalities, which hold for all hidden variables theories, are not so much committed to realism as to two tacit principles of classical physics, which quantum theory does not obey, and which Einstein mistakenly believed to be inherent in epistemological realism. These principles are that (*a*) all physical properties are "sharp" or "well defined" at all times, and (*b*) what happens at a given place can be influenced only by what occurs in its immediate neighborhood (principle of locality or separability). Experiment has refuted the conjunction of these two principles. Realism cannot be refuted experimentally because every well-designed and well-performed experiment involves a clear distinction between object, apparatus, and subject: we must know, at the very least, what is being measured by what and by whom. More in Vol. 7.

In addition to such occasional flares of subjectivism among scientists, the ideal of objectivity has been criticized by two groups of people. The one is formed by the Nazi and Stalinist ideologists who demanded that scientists explode the "myth" of objective research and embrace openly a partisan attitude, i.e. one agreeing with the party line. As is well known, such attacks on objectivism culminated in uncounted personal tragedies and in the banning of entire fields of research. The second group of enemies of the ideal of objectivity is formed by those philosophers, historians and sociologists of science who, following Fleck (1935), Polanyi (1958), Kuhn (1962), and others, claim that all knowledge is personal (meaning subjective), that truth is a social artifact, that the scientist constructs his own object of research, and that the very term "discovery" should be abandoned because it presupposes the autonomous existence of the object of discovery. (See, e.g. Brown, 1977; Barnes, 1982.)

Of course we construct all of our concepts and propositions: these are not found ready made in nature; but we construct models of the world not the world itself. Of course every scientific finding is the end result of a process of inquiry and creation in which imagination is involved; but whatever is discovered—unlike that which is invented—was presumably there to begin with. Even technological inventions are modifications of existents rather than creations out of nothing. Of course original research and invention are subjective processes impossible without passion and hope, fancy and intuition; but in science, technology and the humanities the results of such subjective processes must be checked for truth or usefulness.

If the net results of the "new" epistemology associated with Fleck, Polanyi, Kuhn, Feyerabend, and their followers are that the scientist (or the scientific community) constructs reality instead of modeling it; that there are no objective methods and no objective standards of evaluation; and that society determines what is good or bad science, then this is a *counter-revolution* accompanying the vogue of transcendental meditation, biorhythm, tarot cards, astrology, and parapsychology. It sounds attractive because it emphasizes the subjective aspect of cognition and its social matrix. But it is obscurantist in so far as it rejects objectivity and condones sloppiness. So much for the recent revolt against realism.

Realism is a family of doctrines. Two genera of realism are usually distinguished: ontological and epistemological. *Ontological* (or metaphysical) *realism* asserts the autonomous existence of the world, i.e. its reality independent of the inquirer. (The Platonic variety of ontological realism holds that ideas are the most real of all entities.) *Epistemological realism* maintains that the world can be known. (The scientific variety of realism contains the semantic thesis that scientific theories refer to putatively real entities even though some such entities may turn out not to exist.) Obviously, epistemological realism presupposes ontological realism. Having defended the latter in Vols. 3 and 4, let us turn to epistemological realism.

We distinguish two species of epistemological realism: naive and critical. *Naive* (or *spontaneous*) *realism* holds that the world is just as we see it, i.e. that we know it directly through our senses. On the other hand *critical* (or *constructive*) epistemological *realism* holds that perceptual knowledge is deficient—superficial, incomplete, often wrong; that it must be enriched with conceptual (in particular theoretical) knowledge; and that concepts and their compounds—such as propositions, rules, and theories—are constructions that often go far beyond appearance; and that these constructions represent the world, albeit imperfectly, in a symbolic, not an iconic fashion.

Ordinary knowledge suffices to refute naive realism by noting that one and the same fact may be perceived differently by different subjects. (This argument presupposes that the perceived fact is indeed unique, which of course need not be accepted by the naive realist. But then he runs the risk of switching to subjectivism.) A somewhat more sophisticated argument, used by the later Wittgenstein to attack the naive realism inherent in his *Tractatus*, is that plenty of indispensable concepts, such as those of negation and disjunction, have no real counterparts. (To counter this argument the naive realist may demand that we use a logic deprived of negation and disjunction—which would render discourse extremely poor and clumsy, and argument totally impossible.) A still more sophisticated argument against naive realism is that many important scientific theories refer to unobservable entities and are couched in a complex mathematical language, as a consequence of which they are counterintuitive and cannot be checked by ordinary means. (The naive realist's reply could be that he does not care for such theories.) In conclusion, if we care for science (and technology) we must reject naive realism—not for being realistic but for not being realistic enough, i.e. for not admitting that reality is far more inclusive than appearance.

Realism, then, must be critical or constructive, i.e. it must admit that, in order to know reality, we must invent constructs, in particular theories, that represent reality in devious ways. In fact such theories neither epitomize experience nor represent reality in a point-wise fashion. The first point is clear: theories must be processed and enriched with data before they can represent any empirical facts (e.g. observations). As for the manner in which scientific theories represent reality, we argued in Vol 1, Ch. 3 that it is not the case that every component of a theory has its real counterpart. Rather, the theory *as a whole* represents its referent (or rather some aspects of it) *as a whole*, so the correspondence is global rather than point-wise. For example, the hub of classical electromagnetic theory is the system of equations stating that the d'Alembertian of the four-potential is proportional to the four-density current. In this statement, the d'Alembertian operator is syncategorematic (has no independent meaning); the four-potential is an auxiliary concept (does not represent directly the field); and its analysis into components is (like that of the current) as conventional as the choice of a coordinate system and of a system of units. Only the whole represents (to a first approximation) a system of currents and fields.

Scientific realism is a special version of critical realism, namely the one distinguished by its scientistic component. The word 'scientism' has been assigned several significations (Lalande, 1938). We are interested in two of them. The first is *epistemological scientism*, the thesis that science can give us, and actually often does yield, rather accurate and deep knowledge of reality and, in fact, the best possible factual knowledge. The second is *methodological scientism*, the assertion that the scientific method can and must be tried in all cognitive fields, including the social sciences and humanities.

One may adopt epistemological scientism and not methodological scientism; in fact many natural scientists and many writers on social problems doubt that the latter can be studied in a scientific manner, and some favor "humanistic sociology" and even "humanistic psychology" instead—of which more in Vol. 7. We adopt the two theses of scientism. But we do not accept the reductionist (physicalist) thesis according to which everything biological and social can be reduced to physical and chemical terms: recall Ch. 13, Section 2.2. Nor do we approve of a "slavish imitation of the method and language of Science", which is how von Hayek (1955) defined 'scientism'. Nor, finally, does our variety of scientism require that we accept science in its present state: on the contrary, the attitude of scientism, like that of science, is one of inquiry not belief.

All that scientism states is that scientific research is the best mode of inquiry into any matters of fact—even though it often errs. So, embracing scientism entails accepting alternative modes of inquiry—in particular ordinary knowledge—only provisionally, or because no more accurate or deeper knowledge is needed for a particular purpose. It also entails adopting the philosophical outlook of factual science, which includes not only a realist epistemology but also an ontology of lawfully changing things and the ethos of the free search for truth (Ch. 14, Section 2.1.)

To summarize this section and the preceding one. There are several "theories" (actually views or doctrines) of knowledge, and nearly every one of them contains some truth. In particular, rationalism is adequate for the early phases of rational speculation as well as for formal science, whereas empiricism fits the early phases of factual inquiry. They should therefore be weeded and merged, as well as enriched with principles suggested by the actual practice of inquiry in the most advanced epistemic fields. Two such principles are those of critical (or constructive) realism, and scientism. The resulting synthesis may be called *scientific realism*. It is the epistemology we have tried to build in this book.

We claim that scientific realism is not one more speculative or dogmatic *ism* but the epistemology practised by all scientific investigators in the basic and applied factual sciences, regardless of the philosophy of knowledge they may happen to profess. This we have tried to show in every chapter of this book, by supporting our principles on scientific ways and findings, and criticizing their philosophical rivals on the same grounds. In other words, we presume that our epistemology enjoys a strong inductive support of a very special kind: that of contemporary scientific research. Hence it may fit neither the science of the year 1600 nor that of the year 2200.

For ease of reference we shall now proceed to collect, in the next two sections, some of the epistemological and methodological maxims scattered throughout the previous pages.

3. Maxims of Scientific Realism

3.1. *Descriptive Principles*

The core of any epistemology is composed by a set of descriptive principles every one of which is supposed to epitomize an important aspect of inquiry. Besides having a philosophical value (or disvalue), such principles may contribute to orienting (or disorienting) investigators. Here is a sample of descriptive epistemological maxims gleaned from the previous chapters.

*E*1 The world exists on its own, i.e. whether or not there are any inquirers.

*E*2 We can get to know the world, though only in part, imperfectly, and gradually.

*E*3 Every knowing subject is an animal endowed with a plastic nervous system. (*Corollary*: organisms not so endowed, animal societies, disembodied spirits, machines, etc., cannot know.)

*E*4 Every cognitive act is a process in the nervous system of some animal.

*E*5 Knowing is learning, and the knowledge of an individual is the totality of what it has learned.

*E*6 Learning is the formation of new interneuronal connections (i.e. the constitution of new psychons).

*E*7 Every learning ability develops throughout life and is a result of a long evolutionary process.

*E*8 Humans can know objects of only two kinds: material entities and conceptual objects (constructs).

*E*9 Whereas some constructs derive from percepts, others are invented.

*E*10 An animal can know something about a thing only if the two can be linked by signals that the former can detect.

*E*11 For any given system connectible by signals with an inquirer, the latter can get to know some of the components, some of the things in the environment, and some of the relations in the structure of the system.

*E*12 For any fact physically accessible to an inquirer it is possible to devise means to observe some features of the fact, but there is no means whereby all the traits of any fact can be observed with total accuracy.

*E*13 No inquiry starts from complete ignorance: We must know something before we can formulate a problem and investigate it.

*E*14 Every cognitive operation is subject to error, but every error is corrigible. (*Consequence*: Every correction involves some error, which is in turn corrigible.)

*E*15 There are several ways of knowing: By perceiving, conceiving, and acting; and these various modes combine in many an investigation.

*E*16 All knowledge of factual matters consists in, or involves at some point, some direct observation.

*E*17 Indirect observation yields more and deeper knowledge than direct observation.

*E*18 Some factual knowledge is epitomized in inductive generalizations, and some in hypotheses involving non-observational concepts.

*E*19 Any two humans share some knowledge, but every human knows something that nobody else does. (*Corollary*: Nobody knows everything.)

*E*20 Every animal capable of learning is capable of teaching, e.g. by example.

*E*21 All human inquiry is done in society, and therefore in cooperation and competition with others.

*E*22 All societies set some limits—cultural, political, or economic—on inquiry.

*E*23 All scientific, technological or humanistic inquiry is done nowadays in some research community or other.

*E*24 The main bond that keeps the members of a research community together is the exchange of information, via several languages, with a common goal.

*E*25 Communication among inquirers is the easier, the greater the fund of knowledge, methods and goals they share.

*E*26 In principle every fact and every construct can be investigated: there are problems but no mysteries.

*E*27 Every construct can be clarified. (*Corollary*: Whatever cannot be elucidated is not a conceptual item but merely a noise.)

*E*28 There is no limit to the mathematizability of constructs.

*E*29 Knowledge can be of particulars or of patterns.

*E*30 Knowledge of patterns is compressible into hypotheses and theories.

*E*31 The deeper hypotheses and theories are those involving mechanisms of some sort (not necessarily mechanical ones).

*E*32 In all fields of inquiry we are bound to form causal and probabilistic hypotheses as well as combinations of the two.

*E*33 The hub of every advanced field of factual inquiry is a set of laws (or rather law statements presumably representing objective patterns).

*E*34 Every body of hypotheses can be systematized into a theory, and every theory can be well organized (axiomatized).

*E*35 Every theory proper, when enriched with subsidiary hypotheses and data, can predict, but only mechanismic theories can explain.

*E*36 Every factual theory is a partial representation (global or detailed, true or false to some extent) of supposedly real objects.

*E*37 The testing of a factual theory involves indicator hypotheses relating unobservables to observables.

*E*38 Indicator hypotheses should be checked empirically and justified theoretically.

*E*39 Only theories that can help make predictions are empirically testable.

*E*40 The degree of truth of a theory, and the efficiency of a design, can be found with the help of observation, measurement, experiment, and further theories.

*E*41 Every piece of knowledge and every proposal or design can be improved by research, but not every one is worth being improved on.

*E*42 Advances in knowledge (or in anything else) are sometimes gradual, and at other times they are quick and involve profound alterations.

*E*43 No scientific, technological or humanistic advance comes out of the blue: it always originates in some body of antecedent knowledge.

*E*44 Every novelty in knowledge is comparable with its predecessor, if any, and only such comparison gives us objective grounds for choosing between the two.

*E*45 Every research field is characterized by a general outlook, a background, a problematic, goals and means of its own, as well as by a peculiar research community.

*E*46 Scientific research is the highest of all modes of inquiry.

*E*47 Ordinary knowledge, artisanal knowledge, art, and some socio-political ideologies contain nuggets of genuine knowledge, but they cannot advance any further without the help of scientific, technological or humanistic research.

*E*48 The totality of genuine knowledge constitutes a system, whereas illusory knowledge is marginal.

*E*49 The system of human knowledge is closely connected with the system of production and circulation of goods and services.

*E*50 There are limits, both natural and social, to what man can know, but only the social limits are important—and only these can be overcome.

3.2. *Regulative principles*

Every human inquiry process involves some explicit or tacit epistemological principles. Some of these are regulative, i.e. they guide (or misguide) the planning and execution of inquiry by inspiring problems, methods, hypotheses, or inferences—as well as by suggesting the doubting or rejecting of alternatives, and searching for new principles. Here is a sample of such regulative or methodological principles.

*M*1 Keep studying what others have found, but remember that the quickest and most rewarding way of learning is by conducting independent inquiries.

*M*2 Start your inquiry by choosing a problem that is presumably open, that you feel you can handle, and that may give somebody some satisfaction.

*M*3 If you need a promotion go for the sure thing. If you can afford to be curious, have fun working on tough problems.

*M*4 Do not despise small tasks: Every big problem is surrounded by small problems, and somebody has got to solve them.

*M*5 Formulate your problem clearly: Unearth (or restrict or widen) its context, presuppositions, and data.

*M*6 Do not mistake problems of being for problems of knowledge—e.g. do not try to define causality or free will in terms of predictability.

*M*7 Do not mistake problems of knowledge for problems of being—e.g. do not believe that facts change when seen through alternative conceptual frameworks.

*M*8 Do not let the available techniques dictate all your problems: If necessary try new techniques or even whole new approaches.

*M*9 Plan the investigation into your problem—but be ready to change your plan, and even your problem, as often as necessary.

*M*10 Whenever possible handle your problem scientifically, i.e. armed with scientific knowledge and scientific methods, and aiming at a scientific or technological goal.

*M*11 Do not skirt difficulties—but if you get bogged down switch problems for a while.

*M*12 Do not tolerate obscurity or fuzziness except at the beginning: Try and exactify every key concept or proposition.

*M*13 Do not reify constructs—nor place them in a Platonic realm.

*M*14 Quantitate whatever comes in degrees—but do it only if there is hope of effective measurement, however indirect.

*M*15 Do not commit yourself before checking: First get to know, then believe—and doubt.

*M*16 Revise periodically your most trusted beliefs: You are bound to find fault with some of them.

*M*17 Assign the greatest credence to the best confirmed proposition, the greatest confidence to the most reliable and effective artifact.

*M*18 Regard every principle, method and artifact as fallible in principle—but do not hesitate to use it as long as it does not lead consistently to disaster.

*M*19 Trust your fellow inquirers to pose questions that you did not think of asking, and to find out what you failed to know.

*M*20 Trust your fellow inquirers to make mistakes that you can discover and perhaps correct.

*M*21 Do not try to be totally self-reliant in matters of knowledge: Ask others for information, advice, or help—but feel free not to make use of either.

*M*22 Keep up to date but do not stick to fashion: Every radically new idea, procedure or artifact is unfashionable in the beginning.

*M*23 Listen to your critics but, if you have good grounds to believe that you are on the right track, do not let them bully you.

*M*24 Do not just accumulate data for the sake of increasing the volume of information: Look for patterns.

*M*25 Make liberal use of analogy and induction—but always be on the lookout for their limitations.

*M*26 Simplify—but keep track of your simplifying assumptions and alter them if truth demands it.

*M*27 Make the most of deduction—but check the conclusions to evalute the premises.

*M*28 Do not just accumulate hypotheses: Try to organize them into theories (hypothetico-deductive systems).

*M*29 Prefer theories couched in mathematical terms—but remember that mathematics gives clarity, unity, and deductive power, but not factual truth.

*M*30 A theory a day keeps false data away, and a datum a day keeps false theories away.

*M*31 Watch for errors of all kinds and be ready to correct them if it is worthwhile to do so.

*M*32 Study every entity as a system or a component of such.

*M*33 Recall that every object of study is many-sided and should therefore be approached from several viewpoints.

*M*34 Specialize but never to the point of being unable to understand that an alternative approach is possible.

*M*35 Look for change beneath apparent rest, as well as for the invariants of change.

*M*36 Look for pattern beneath apparent chaos, and for randomness alongside or beneath regularity.

*M*37 Investigate every level in its own right as well as in relation to adjoining levels.

*M*38 Do not skip levels—e.g. do not try to write the Schrödinger equation for the brain.

*M*39 Reduce as far as possible but do not be blind to emergence.

*M*40 Try to integrate all the fields of knowledge that study the same objects.

*M*41 Do not stop trying to explain, but shun theories that presume to explain everything, for they are likely to explain nothing.

*M*42 Do not stop trying to forecast—but do so only with the help of reasonable theories and data.

*M*43 Shun ideology in basic science but watch for it everywhere, and declare frankly your social values in applied science and technology, particularly in social technology.

*M*44 Do not expect to invent any important artifact or social organization without some basic scientific knowledge.

*M*45 Do not expect basic science to deliver technology without further ado.

*M*46 If in a position of scientific or technological power, do not use it except to destroy barriers to discovery and invention.

*M*47 All rules are limited in scope as well as fallible, improvable, or replaceable.

*M*48 Rules of inquiry may help but they do not guarantee success: Original research and design are inventive rather than rule-directed.

*M*49 Don't try to ignore philosophy: Those who ignore philosophy only succeed in reinventing it.

*M*50 In attempting to advance epistemology take into account all the sciences of knowledge.

4. Concluding remarks

Our epistemology, scientific realism, is characterized by the following theses formulated and discussed in the preceding pages:

(i) *Realism*: The world exists on its own, i.e. independently of the inquirer, who can succeed in knowing parts and aspects of the world.

(ii) *Naturalism*: Cognition is a brain process, and there are no supernatural or paranormal (i.e. noncerebral) modes of cognition.

(iii) *Evolutionism*: All cognitive abilities evolve.

(iv) *Epistemological socialism*: Human inquiry is done by individuals learning from one another and embedded in society, the norms of which now stimulate, now inhibit research.

(v) *Historicism*: Every inquiry starts from tradition, which it expands and corrects.

(vi) *Moderate rationalism*: Reason is necessary for knowing.

(vii) *Moderate empiricism*: Experience is necessary to know the world.

(viii) *Constructivism*: Concepts and their compounds, even those that represent real things, are our own creation.

(ix) *Conventions*: Some of the concepts occurring in our models of real things are conventional.

(x) *Representation*: Science produces symbolic representations of the world.

(xi) *Praxis*: Technology helps change the world.

(xii) *Justificationism*: Every proposition and every proposal ought to be justified by reason, experience, or in both ways.

(xiii) *Fallibilism*: Every item of factual knowledge, be it datum or hypothesis, is fallible.

(xiv) *Meliorism*: All factual knowledge is perfectible.

(xv) *Scientism*: Anything knowable and worth knowing can be known scientifically or technologically better than in any other way.

(xvi) *Systemism*: The family of research fields, and the body of knowledge reaped in them, each constitute a system.

This concludes our study of general epistemology and methodology. The next volume of our *Treatise* will be devoted to applying those general principles to the investigation of selected topical problems in the philosophy of science and technology.

APPENDIX 3

PARTIAL TRUTH

Our aim in this appendix (revised from Bunge (1981d)) is to find a suitable function from propositions to numbers, such that the value of such function for a given proposition be interpretable as the truth value of that proposition. It is convenient to take the real unit real interval [0, 1] as the range of the function V defined on the set of propositions capable of acquiring a truth value. Thus we shall assume that the truth value function is of the kind $V:P \rightarrow [0, 1]$.

There are many possible choices for V but of course most of them are inadequate. The simplest choice is the usual one, where the range of V is made to collapse into the true-false set. Such theory is inadequate, for it makes no room for half truths. The next simplest choice is to assume $0 \leq V(p) \leq 1$, $V(\neg p) = 1 - V(p)$, and $V(p \vee q) = \frac{1}{2}[V(p) + V(q)]$. This theory is inadequate because it assigns conjunctions the same truth value as disjunctions. (To prove this express the conjunction in terms of disjunction and negation, and make use of the above postulates.)

A third possibility is the theory based on the axioms

*A*1 For all p in P, $V(\neg p) = 1 - V(p)$.

*A*2 For all p and q in P,
(i) $V(p \vee q) = max\{V(p), V(q)\}$,
(ii) $V(p \& q) = min\{V(p), V(q)\}$.

This theory distinguishes correctly between the two basic modes of composition. Besides, it includes a desirable generalization of the *modus ponens*, namely: If p and $p \Rightarrow q$ have the same truth value v, then $V(q) = v$ as well. The trouble with this theory is that a single totally false conjunct suffices to ruin a conjunction, even an infinite one. (*Proof*: Set $V(p) = 0$ in *A*2(ii).) Intuitively, one wishes to say that such a conjunction is partially true rather than totally false.

There are other, more complicated, theories of truth, in particular the ones I proposed in 1963 and 1974a. However, there are problems with them. I have come to suspect that one source of trouble common to all these theories is the way they handle negation. Let me explain. All the theories of truth I am familiar with assume, explicitly or tacitly, that truth and falsity are symmetrical, i.e. that the truth value of not-p is the complement of the truth value of p. This assumption looks intuitive but, like many intuitions, it may turn out to be false. If this is the case then it suffices to ruin all of the extant theories of truth. Let us investigate how the assumption fares in a simple case.

Johnny turns 10 years old. His friend Peter believes Johnny to be 11, whereas his friend Jane suspects that he is 9. Both are in error but not by much: their relative error is 1 in 10, so the truth value of each of their beliefs about Johnny's age can be taken to be $1.0-0.1=0.9$. Charlie, a third friend of Johnny's, is uncertain about his age and, being a very cautious person, avoids any risky estimates and states "Johnny is *not* 9 years old". He is of course right, and would also be right if Johnny were 8 or 11, 7 or 12, and so on. So, it is a bad mistake to assign his statement the truth value $1.0-0.9=0.1$, which is what the postulate $\ulcorner V(\neg p)=1-V(p)\urcorner$ demands. (In general, when there are more than two possibilities it is less risky to assert a negative proposition. Equivalently, the less informative a proposition, the more chances it has of being true.)

The upshot is that $V(\neg p) \neq 1-V(p)$. But this negative result of ours is a cheap truth. In order to build a theory of truth we must commit ourselves, i.e. make a positive assumption about the truth value of the negate of a proposition. A simple assumption, to be qualified later on, is this: $\neg p$ is completely false only if p is completely true; otherwise, i.e. if p is less than completely true, $\neg p$ is completely true. In other words, we may assume that, for every proposition p that does have a truth value, $V(\neg p)=1$, if, and only if, $V(p)<1$, and 0 otherwise. (We shall add an important qualification later on.) Let us conjoin this assumption with a plausible postulate concerning the composition of propositions, to produce a theory for which we may claim at least partial truth.

We presuppose ordinary logic and stipulate that the truth valuation function $V: P \rightarrow [0,1]$ satisfies the following three axioms.

*A*1 If p is not the negate of another proposition, then

$$V(\neg p)=\begin{cases}0 & \text{iff } V(p)=1\\ 1 & \text{iff } V(p)<1\end{cases}$$

Otherwise, i.e. if p is the negate of a proposition q, which in turn is not the negate of another proposition,

$$V(\neg p)=V(q).$$

$A2$ For all p and q in P,

$V(p \& q) = min\{V(p), V(q)\}$

$A3$ For all q and p in P,

$V(p \vee q) = max\{V(p), V(q)\}$

Let us now extract some consequences and see whether these are plausible.

COROLLARY 1 For all p in P,

$$V(p \& \neg p) = \begin{cases} 0 & \text{iff} \quad V(p) = 0 \quad \text{or} \quad V(p) = 1 \\ V(p) & \text{iff} \quad 0 < V(p) < 1 \end{cases}$$

COROLLARY 2 For all p in P,

$V(\neg(p \& \neg p)) = 1$

COROLLARY 3 For all p in P,

$V(p \vee \neg p) = 1$

COROLLARY 4 For all p and q in P,

$$V(p \Rightarrow q) = \begin{cases} 1 & \text{iff} \quad V(p) < 1 \\ V(q) & \text{iff} \quad V(p) = 1 \end{cases}$$

THEOREM 1 For all p_i in P, where $1 < i < n$,

$$V\left(\bigwedge_{i=1}^{n} p_i\right) = min\{V(p_1), V(p_2), \ldots, V(p_n)\}.$$

THEOREM 2 For all p_i in P, where $1 \leq i \leq n$,

$$V\left(\bigvee_{i=1}^{n} p_i\right) = max\{V(p_1), V(p_2), \ldots, V(p_n)\}.$$

The first two parts of Axiom 1 seem to capture our considerations on negation, and the third restricts the scope of the standard theorem on double negation. (The latter may warm the heart of intuitionists although it was not meant to.) Axiom 2 and its generalization, Theorem 1, formalize the intuition that the truth value of a conjunction equals the smallest of the truth values of the conjuncts. Axiom 3 and its generalization, Theorem 2, formalize the intuition that disjunction is always safest—if least informative. Corollary 1 says that a contradiction, though absurd, may not be worthless, for it contains at least one worthy proposition. (The trouble with contradiction is not that at best it is half true, but that it entails everything and thus "says" everything that can possibly be said.) Corollary 3 is of course the familiar excluded middle, and thus no part of intuitionism. Corollary 4 generalizes the *modus ponens*.

The theory we have just sketched seems reasonable—to the present writer at the time of writing. It has, however, a defect that may prove to be more than a mere *Schönheitsfehler*, namely that it postulates $A3$ instead of deducing it from the other two axioms. This suggests trying to replace $A1$ or $A2$ with some other assumptions capable of entailing the truth value of a disjunction. And, whether or not this goal is achieved, two further developments are desirable. One is to generalize the theory to encompass the truth valuation of formulas containing quantifiers. (Theorems 1 and 2 may prove useful to this effect.) The other is to build a theory of truth based on the intuitionistic predicate calculus.

The notion of partial truth is used in the next Appendix to elucidate that of predictive power.

PREDICTIVE POWER

Several measures of predictive power are conceivable. The simplest of all is this. If T is a factual theory that is adjoined a set S of subsidiary hypotheses, and is fed a set D of empirical data, then the predictive power of T relative to S and D is the number of different predictions entailed by T together with S and D. A defect of this measure is that it does not involve the accuracy of the predictions. This defect is easily repaired by taking the sum of the truth values of the predictions relative to the new body E of empirical data produced in order to test T and S. Since in general these truth values are comprised between 0 and 1—i.e. the predictions are more or less true rather than either totally true or totally false—we may define the *predictive power* of T relative to S, D and E as

$$\pi(T|SDE) = (1/n)\sum_{1}^{n}{}_{i} V(p_i|E), \text{ where } T \cup S \cup D \vdash p_i,\ 1 \leq i \leq n, \text{ and } E \cap D = \phi.$$

Clearly, the predictive power of a theory is relative to the additional assumptions and data: a change in either is likely to modify the value of π.

A shortcoming of the above measure is that it makes no difference between new and old predictions, i.e. it does not include the originality or audacity of the theory. The latter can be defined as the dual or complement of the conformity with the relevant antecedent body of knowledge. More precisely, we define the *degree of originality of* proposition p_i relative to both the antecedent body A of knowledge and the fresh empirical evidence E as

$$\omega(p_i|AE) = V(p_i|E) - V(p_i|A),$$

so that the originality is maximal for a true proposition incompatible with the antecedent knowledge, and minimal for a false proposition compliant with the latter. Besides, we rule that the truth value of a proposition be assessed relatively to both the fresh empirical evidence E and the antecedent knowledge A. More precisely we stipulate that

$$V(p_i|AE) = \tfrac{1}{2}[V(p_i|E) + V(p_i|A)],$$

which is comprised between 0 (empirically false heterodoxy) and 1 (empirically true orthodoxy). Finally, we assign each such truth value the weight $\omega(p_i|AE)$, i.e. we multiply truth values by originalities, and add. The result is called the *predictive power* of T relative to S, D, E, and A:

$$\Pi(T|SDEA) = (2/n) \sum_{i=1}^{n} \omega(p_i|AE) \cdot V(p_i|AE)$$

$$= (1/n) \sum_{i=1}^{n} [V^2(p_i|E) - V^2(p_i|A)]$$

The range of this function is $[-1, 1]$. Table A.1 exhibits a few typical values for the a typical case in which all the predictions have the same truth

TABLE A.1

Typical values of the predictive power of a theory T relative to sets S of subsidiary assumptions, D of data used to compute the predictions, E of data used to check them, and A of items of relevant antecedent knowledge.

Theory	$V(p_i\|E)$	$V(p_i\|A)$	$\omega(p_i\|AE)$	$V(p_i\|AE)$	$\Pi(T\|SDEA)$
True conformist	1	1	0	1	0
Half true conformist	$\frac{1}{2}$	1	$-\frac{1}{2}$	$\frac{3}{4}$	$-\frac{3}{4}$
False conformist	0	1	-1	$\frac{1}{2}$	-1
True nonconformist	1	0	1	$\frac{1}{2}$	1
Half true nonconformist	$\frac{1}{2}$	0	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{4}$
False nonconformist	0	0	0	0	0

values. True nonconformist theories have the greatest predictive power, whereas false conformist ones have the smallest; true conformist and false nonconformist theories are symmetrical as regards predictability; and a half true heterodoxy has a greater predictive power than a half true orthodoxy.

FORMAL STRUCTURE OF EXPERIMENT

Let us investigate the algebraic structure of an experiment. We shall consider the physical transactions involved in an experiment, and assume that the conceptual component involved in the design and interpretation is represented in some of those physical processes.

Consider a system composed of an observing thing a, or *observer*, and an observed thing b, or *object*. The observer system a is a person or a team equipped with suitable instruments. The two components can be connected in either of the following ways: a acts on b, b acts on a, and a and b interact. Moreover the couplings can be energetic, informational, or both. If energetic we speak of actions as *inputs*; if informational we speak of *signals*. (But of course every signal is carried by a process that transfers energy.) We are particularly interested in the case where the action of the object on the observer is a signal, i.e. an input to the observer's brain. In this case we have the possible situations shown in Figure A.1. To analyze these three situations we avail ourselves of the concept of state space (Vol. 3, Ch. 3) as well as of a remark of Krohn *et al.* (1967).

Call $S(a)$ and $S(b)$ respectively the state spaces of the observer a and the object b, Σ a set of signals that a is capable of admitting, and E a set of actions or inputs that the object b can receive from the subject a. Then our three situations are summarized as follows:

Observation (of b by a) $\omega: \Sigma \times S(a) \to S(a)$
Action (of a on b) $\alpha: E \times S(b) \to S(b)$
Experiment (of a on b) $\varepsilon: \langle \alpha, \omega \rangle$

Each observation act is described as follows. When signal $\sigma \in \Sigma$ coming

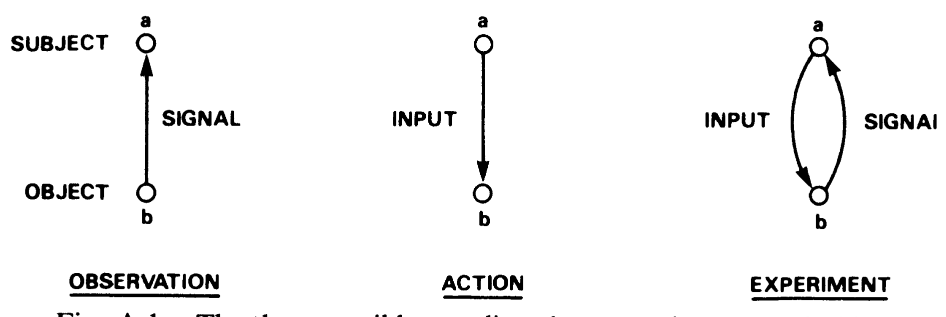


Fig. A.1. The three possible couplings between observer and object.

from object b arrives at observer a in state $s \in S(a)$, a jumps from s to a new state $u = \omega(\sigma, s) \in S(a)$. (We are disregarding time delays, which can easily be incorporated.) Two successive observations combine into one. Thus, if observer a in state $u = \omega(\sigma, s)$ receives signal τ, a jumps to state $v = \omega(\tau, u) = \omega(\tau, \omega(\sigma, s)) \in S(a)$. Hence the set $G(\Sigma) = \{\sigma \in \Sigma | \omega_\sigma\}$ of signal transformations is a semigroup under function composition. Or, if preferred, $\langle S(a), G(\Sigma) \rangle$ is the transformation semigroup acting on $S(a)$. We call it the *observation semigroup*. In other words, the set of all observations performed by a subject on an object with the help of signals of a given kind has the semigroup structure.

What holds for observation holds, *mutatis mutandis*, for action. That is, two successive actions combine to produce a third one. Thus if the object b while in state $f \in S(b)$ receives input $e \in E$ from the observer, it jumps to some other state $g = \alpha(e, f)$. And if b now receives another input h from a, b goes from state g into state $\alpha(h, g) = \alpha(h, \alpha(e, f))$. In terms of the input transformations we have $\alpha_h(g) = \alpha_h(\alpha_e(f)) = \alpha_{he}(f)$. So, actions too form a semigroup, namely $G(E) = \{x \in E | \alpha_x\}$. We call $\langle S(b), G(E) \rangle$ the *action semigroup*.

A while ago we characterized an experiment as a pair of functions, namely an observation function and an action function. We see now that it is more than that, namely an observation semigroup together with an action semigroup. This characterization is correct as far as it goes, but it does not go far enough, for it treats observation and action as mutually independent and on the same footing, which they are not. In fact the inputs to the object do not come from outside the system but are produced by the observer, who is supposed to control them carefully. That is, each input $e \in E$ acting on the object b corresponds to some change $f \in S(a) \times S(a)$ in the state of the observing system or object. (I.e. it is always some event in the observer or its delegate that triggers the experimental action it exerts on the object.) We assume that this correspondence is functional, i.e. that it is represented by a function.

$$k : E \to S(a) \times S(a)$$

to be called the *control function*. The observer event $k(e) \in S(a) \times S(a)$ is called the *controlling event*. Since not all of the changes of state of the observer ensue in controlling events, k is injective (into).

Finally we incorporate a further relation, namely this. Every signal conveys information concerning the state of the object; more precisely, every signal is a message "telling" the observer in a given state what state the object is in as a consequence of the application of an experimental input. We introduce then what we shall call a *messenger function*

$$\mu : \Sigma \times S(a) \to E \times S(b)$$

such that, for a signal $x \in \Sigma$, and an observer state $s \in S(a)$, $\mu(x, s) = \langle e, t \rangle \in E \times S(b)$ is the message that x conveys to a in state s, namely that the input e is paired off to the object state t.

We collect the various items met so far into the following diagram, where the nameless arrows designate projections.

$$\begin{tikzcd}
S(a) \times S(a) \arrow[r] & S(a) & \Sigma \times S(a) \arrow[l, "\omega"'] \arrow[ddll, "\mu"'] \arrow[dd] \\
E \arrow[u, "\kappa"] & & \\
E \times S(b) \arrow[u] \arrow[rr, "\alpha"'] & & S(b)
\end{tikzcd}$$

So far we have regarded observations and experimental actions as forming semigroups somehow related by the control and messenger functions. Actually they are richer structures, namely categories. In fact, consider the triple $\langle S(a), G(\Sigma), i_a \rangle$, where i_a is the identify morphism (or transformation) in the observer state space $S(a)$. The observer states, i.e. those in $S(a)$, constitute the "objects" of the triple, the observations or signal transformations (i.e. the elements of $G(\Sigma)$) the morphisms, and the null observation (i.e. i_a) the identity morphism of the triple, which is then a category. Likewise in the case of actions: here the "objects" are the object states, the morphisms the experimental inputs, and the null action the identity morphism in the object state space $S(b)$, designated by i_b. In sum we have to do with two categories:

the *category of observations* $\langle S(a), G(\Sigma), i_a \rangle$

and

the *category of actions* $\langle S(b), G(E), i_b \rangle$

which together represent an experiment.

APPENDIX 6

DEGREE OF CONFIRMATION OF A THEORY

To appreciate the magnitude of the inductive leap we take when attributing truth or falsity to a theory imagine an omniscient being confronting a theory T with things of some kind K, such as neutrinos or humans. Being omniscient he knows every one of the infinitely many formulas of T as well as every single possible state of all things of kind K, and he can pair the former off to the latter to find out whether the formulas match the facts. Such omniscient being will find use for the following definitions relating the theory T to a suitable state space S_K for things of kind K:

(i) T *represents* K's (or is a *representation* of things of species K) iff there exists a function ρ: $S_K \to 2^T$ assigning to every state s in S_K a set t of statements in T;

(ii) T is *true* (or a *true representation* of K's) iff (a) the map ρ is bijective and (b) every member of $\rho(s)$ is totally true for every s in S_K.

Since we, finite beings, do not have access to the totality of formulas of T or to the totality of states of a concrete thing, we must settle for injective maps and partially true propositions in finite numbers. All we can hope for is a high degree of confirmation. This concept can be defined in terms of the concept of partial truth—not probability.

Let T be a specific factual theory and E the totality of empirical data relevant to T such that every member of E is true—a gross simplification. Call E_T the subset of E composed of data joined tó T in order to derive testable accounts of facts in a certain domain. The complement $\bar{E}_T$ of E_T is the collection of data employed to check T. When enriched with E_T, T yields n testable consequences $t_1, t_2, \ldots, t_n$. Suppose we check empirically every one of these and ask how well or poorly they account for the available evidence $\bar{E}_T$. If we use the black and white notion of truth we can define the coverage or *degree of confirmation* of T relative to a run of n empirical tests whose outcomes form the set $\bar{E}_T$ as the fraction m/n of hits or confirmations.

A far more realistic measure of the degree of confirmation of T relative to $\bar{E}_T$ is

$$C_n(T|\bar{E}_T) = (1/n) \sum_{i=1}^{n} V(t_i|\bar{E}_T),$$

where $V(t_i|\bar{E}_T)$ is the degree of truth of theoretical consequence t_i judged on the strength of empirical evidence $\bar{E}_T$. Assuming that these degrees of truth range between 0 (total falsity) and 1 (total truth), the values of C_n are similarly comprised between 0 (every prediction is false) and 1 (every prediction is true). An intermediary value such as $\frac{1}{2}$ can be produced in a number of manners, e.g. when every t_i is half true, or half of the t_i's are true and the rest false. The *power* or *volume* of T in accounting for the given facts may be defined as the product of its converage by its depth, or number of levels referred to by T.

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