Scientific Uncertainty, and Information

By LEON BRILLOUIN

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INTRODUCTION

Every day we read: This result is proven by science.... Our drug was scientifically tested.... Science teaches that.... The word "science" seems to contain some magic power; if put forward, it should bring immediate consent, with no possible discussion. This situation is exploited for advertisement, but it is basically unsound. Science is not a creed. It was not revealed to man by some superior deity. Science is a product of the human brain, and as such, it is always open to discussion and possible revision. There is no absolute truth in it; rather relativity is its rule. It represents a logical summary of human knowledge, based on human observation and experience, both of which are always of limited range and finite accuracy. As for the logic introduced into the classification of empirical facts, it is typically a product of our brain. We select experimental results that appear to us as logically connected together, and we ignore many facts that do not fit into our "logic." This rather artificial procedure is our own invention and we are so proud of it that we insist its results should be considered as "laws of nature."

We also select a few laws as of superior importance and we call them "principles." There are no sacred principles in science: laws are only summaries of experimental facts, selected and classified by human thinking.

Human beings are incredibly conceited fellows, too prompt to admire their own work, and to indulge in wishful thinking! Scientific theories introduce connections between empirical facts, but a *theory* may be discarded, while *facts* remain, if they have been correctly observed, and the connections will be maintained in a different theory.

Our grandsons will soon deride our simplicity, and make fun of our theories, just as we now deride the "light-ether" of old optical theories: "We now know that there is no ether, it was scientifically proved by Einstein. . . ." Let us candidly admit that we know nothing with certainty, that all our theories are open to discussion and revision and will be modified over and over again. As for the theory of light, it would be wonderful if we actually had *one* that really could be trusted. But we have to be satisfied with a strange structure, a mixture of electromagnetic fields, quantization, and relativity. When we speak of photons and of electromagnetic waves, it is obvious that unity has not yet been achieved in this field, despite many remarkable achievements. Quantum electromagnetism is still meeting strange difficulties that were recently brushed aside, but not eliminated.

In the present book, we intend to discuss the validity of theories developed in experimental sciences: in physics, chemistry, or biology. We leave out of our consideration all the possible chapters on pure mathematics, since they represent completely different problems, quite apart from the structure of the other sciences.

Science is not a mere accumulation of empirical results. It is essentially an attempt at understanding and ordering these results. What scientists are trying to do is to discover some logical frame of thinking that may enable them to find interconnections and relations between experimental observations that may be stated as "scientific laws."

The exact value and significance of these laws must be considered and weighed very carefully. They may easily be underestimated, or, on the contrary, largely overrated. Very divergent opinions have been uttered, ranging from total skepticism to blind faith in the absolute power of science. This is where a delicate and open-minded examination of the situation is actually needed. The scientist and the layman should both agree and discover together how much reliance they may have in science and how far it can reach.

Scientists always work against a background of philosophy, and while many of them may be unaware of it, it actually commands their whole attitude in research. The need for a clarification of this philosophical attitude has been deeply felt by many thinkers who spent much of their time in discussing the basic fundamentals of science. Max Planck devoted many years, in his old age, to these discussions, and his papers and pamphlets are of the highest importance. A great mathematician H. Poincaré, who was also a very remarkable theoretician in physics, wrote a few booklets that remain as great documents of scientific thinking. A. Einstein underlined some other aspects of the questions involved, and found himself in sharp opposition to views developed by N. Bohr. In this country, the great experimenter P. W. Bridgman wrote some fundamental remarks leading to his "operational method." More recently, we saw a revival of age-long discussions of "determinism *versus* probability," with L. de Broglie, and even Schrödinger, on one side, while M. Born, Heisenberg, and most theoretical physicists stood in the opposite camp.

The philosophical background of science is a very serious problem, still worth discussing, and most important for a better understanding of science. We do not expect to solve the question. It goes back to ancient Greek thinkers and it will stay open as long as scientific research itself remains alive. But even if we cannot give a final answer, we must not ignore the problem, and we shall try to explain where the difficulties lie and how they can be properly stated.

Information theory happens to be a powerful means of investigation and, in our opinion, a very safe guide—a sort of Ariadne's thread, to keep us from getting lost in this labyrinth.

The purpose of this book is not to give a definite and final answer but to look at all sides of the problem, to discuss the different possible attitudes of a thinker, and to state, as correctly as we can, the questions involved in the philosophical background of science.

This is not supposed to represent a creed for scientists to follow, but to open a discussion that may develop together with science itself, and always leave a possibility for adaptation to new situations. In a word, it is an essay, not a textbook, and we think it would be unfair to the reader to present it otherwise.

Many metaphysical creeds have been proposed, including the so-called dialectic materialism. All these artificial structures were soon discovered to be jails for free thinking.

It would have been easy enough to organize our discussion in a strictly logical fashion, and to make it look like a solid building. But this would have been entirely artificial: just as much of an artifice as the presentation of a standard mathematical textbook, with its propositions, lemmas, theorems, etc. Mathematics is not being discovered this way. It is invented piecemeal, at random; the mathematician follows his inspiration like an artist. He is actually a poet. Later on, the teacher in him takes over and writes theorems and lemmas, and the fun of discovery is gone forever.

So, we tried to avoid any prefabricated structure, for that would be a hindrance; and we did not superimpose any post-fabricated system of frames and subdivisions, for that would be false pretense. Our aim is simple, and can be explained in one phrase: unbiased free discussion.

This means unorthodox thinking, really free investigation, trying to open ways and alleys for further thinking in poorly known territory.

This means also unfinished business: all these problems, just as science itself, will never be finished. Complete achievement would mean the death of any research.

As a conclusion, we might summarize the whole discussion in a few words: the greatness and the shortcomings of theories.

The present book is divided into two parts. In the first part we discuss general problems of scientific research, especially the roles of observation, information, and imagination in the formulation of scientific laws.

The second part is devoted to classical mechanics, supposed to represent the stronghold of strict determinism. We show that this doctrine contains many uncertainties and we scrutinize the role of the great Poincaré theorem.

It was not found necessary to repeat many mathematical or theoretical proofs that could be found in other books. The reader will not be surprised to find many references to "Science and Information Theory" (1962) and "Tensors in Mechanics and Elasticity" (1961), both books by the present author and published by Academic Press.

MARCH, 1964 LEON BRILLOUIN

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Part 1

Information and Imagination in Science

Chapter I

THERMODYNAMICS, STATISTICS, AND INFORMATION[1]

1. SADI CARNOT—A PIONEER

Once, long, long ago, there was a lone scientist who deeply wondered about the mechanical power of steam engines; he kept dreaming and thinking about this strange phenomenon and finally published a short pamphlet about his findings. His name, Sadi Carnot[2]; the book, "Réflexions sur la puissance motrice du feu" (Paris, 1824). Carnot was 28 years old at that time and he had just discovered the famous principle that still bears his name. But he had

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[1] Revised from articles first published in *Cahiers Pléiade* **13**, 147 (1952) (in French); *Am. J. Phys.* **29**, No. 5, 318–328 (1961).

[2] The Carnots are a famous family of scientists and political thinkers in the history of the three French Republics. The founder, General Lazare Carnot (1753–1823), was the Minister of War for the first French Republic and for Emperor Napoleon I. He won the title of "Organizer of Victory." He also was a brilliant mathematician, and his work is quoted by Sommerfeld (1952). Lazare's oldest son, Sadi Carnot (1796–1832), was the inventor of thermodynamics. His later work on the first principle is briefly quoted by Sommerfeld (1956, p. 22, footnote and p. 26) who writes: ". . . we shall follow the classical path initiated by Sadi Carnot in 1824 and then followed by R. Clausius from 1850 and by W. Thomson from 1851 onward."

Lord Kelvin repeatedly tried to discover the notebook of Sadi Carnot (25 years later!), of which he had heard through some French friends, but he could not find it.

The second son of Lazare was Hippolyte Carnot (1801–1888), Minister of Education in 1848 (second Republic) and a well-known sociologist. Marie-François Sadi Carnot (1837–1894), a son of Hippolyte, became President of the third French Republic in 1887 and was assassinated by an Italian anarchist.

The papers of Sadi Carnot, the scientist, were published by the French Academy of Sciences in 1927 under the title: "Biographie et Manuscrits de Sadi Carnot." The biography, written by the great mathematician E. Picard, is full of valuable information.

been careless enough to invent the principle we call "second" before stating the first one! And professors told him time after time, for more than a century, how wrong he had been! Professors were also wrong not to read the second book of Carnot.[2] To their credit, let us say that the second book was published only in 1927, more than a century later; and this requires a few words of explanation.

After writing his first book, Carnot kept thinking and wondering; he made short notes in a small notebook, intending to rewrite the whole thing more carefully later on. But there was no "later"; Sadi Carnot died in 1832 during an epidemic of cholera that ravaged Paris. He was then 36 and had left in his notes the detailed statement of the first principle, plus a computation of the mechanical equivalent of heat (which was only 15% off), plus a sketch of the kinetic theory and of thermal agitation! It took scientists half a century to rediscover all these fundamental ideas.

And what about the notebook? It was given to Sadi's brother Hippolyte, a sociologist, who kept the book in his library but did not suspect its extraordinary importance. Hippolyte gave this book to the French Academy of Sciences around 1878 and some abstracts of it were published at that time. Complete publication with a photocopy of the notebook occurred only in 1927. It does not seem to have attracted great attention; however, it does contain some striking statements, if you remember the date, 1830:

> Heat may be a vibrational motion of molecules. If this be the case, a quantity of heat is but the mechanical energy used up to put the molecules into vibration. . . . Heat is a motion. . . . *Total energy* [in Carnot's words, *puissance motrice*] *exists in nature in constant total amount.* It is never created nor destroyed; it simply takes another aspect. . . . Production of one unit of mechanical energy requires the destruction of 2.7 units of heat.

The computation was based on gas diffusion experiments, and was very similar to a later computation made by R. J. Mayer in 1842.

Some of Carnot's notes are not so easy to read. He had to create his own vocabulary, which we often do not understand clearly. Unquestionably, he was a pioneer. He found his way through an unknown territory and traced his own footpath, but he did not have time to build a highway for students to follow. For instance, the word "caloric" is often obscure. It used to mean "quantity of heat," but in the Carnot pamphlet, it can be best translated by "entropy," as was first noted by Bronsted and La Mer (see La Mer, 1949).

Carnot was one of the great geniuses in science, but death carried him away leaving his work tragically unfinished.

2. Two Principles of Thermodynamics

First Principle: Conservation of Energy

Despite its various aspects and its perpetual transformations, energy keeps an unchanged total value as long as we consider a closed system isolated from its environment. A charged battery and a weight raised to a certain height represent typical forms of energy, and may be changed into chemical energy, work, or heat. The reverse transformations, from heat into work, for instance, are also possible, with certain limitations related to the second principle.

To the usual forms of energy, Einstein added a new one: mass. Any mass possesses energy; any energy represents a given mass. The old principle of conservation of the mass is thus enlarged and integrated into the energy conservation principle.

Second Principle: Carnot

Caloric energy requires a special treatment with a sort of two-way accounting. Let us consider a machine or a physical system in contact with heat sources. We first establish an energy balance sheet of the operations, with credit and debit columns, depending on whether the machine absorbs or yields heat.

Then we use an exchange coefficient, which varies according to the temperature at which each heat transfer occurs. In dividing the quantity of heat Δq (positive or negative) by this exchange coefficient, we calculate the amount of entropy ΔS supplied, for which we keep a separate account. The exchange coefficient is none other than absolute temperature T. In the centigrade scale, it is the usual temperature, increased by 273.15°. Absolute zero corresponds to $-273.15°$ centigrade. According to this procedure, we start from a fundamental relation between the quantity of heat Δq received by a system, in the course of a given operation, and the increase of entropy ΔS in this same system.

$$\Delta S = \Delta q / T \quad \text{or} \quad \Delta q = T \Delta S \tag{I.1}$$

and we must keep an account of the quantities q and S separately.

Think of a goldsmith dealing in precious metals. He keeps an account of the weights of metal bought or sold. In another column he records prices paid or received. For each transaction mentioned in the first account, there corresponds a mention in the second column. Imagine that Δq represents the weight of metal bought, silver, for instance. If T is a weight of silver worth a dollar, ΔS is the amount of the operation in dollars. The comparison links the idea of entropy to the notion of value. We shall have further opportunity to discuss this analogy (Section 4).

Between heat and entropy, we have an exchange office, using a variable rate of change according to the temperature: a strange sort of planned economy. Still more curious is the tendency of entropy always to increase. Dip a hot metal into a container filled with cold water. A certain quantity of heat will pass from the metal into the water. The entropy lost by the metal is calculated with a very high coefficient T_1; the entropy gained by the water contains a lower coefficient T_2; thus, the entropy lost by the metal is less than the entropy gained by the water. All in all, the metal-water system has gained entropy. Here is another experiment: Let us warm up water by means of an electric resistance connected to a battery. The battery loses electric energy but very little entropy, the water gains heat and entropy. The following general rule can be deduced: For any isolated system, the total energy remains a constant, but the total entropy has a tendency to increase. The system's entropy may, at the least, remain constant (if nothing happens, or if the transformations are reversible); it can increase in the case of irreversible transformations (as in the two given examples), but the entropy of an isolated system can never decrease.

The economy of the entropy is not only planned; it follows a one-way street.

These remarks call for a definition of the total entropy S of a system. Let us suppose that we can build a system from its component parts while using only a series of reversible transformations; the heats $q_1, q_2, \cdots$ used in each operation can be observed, and the corresponding entropies $s_1, s_2, \ldots$ calculated.

The total entropy of the system can then be defined as a sum,

$$S = s_1 + s_2 + s_3 \ldots \tag{I.2}$$

of the entropies corresponding to each of these reversible steps. If some of the transformations are irreversible (explosive chemical reaction, noncompensated heat), we can no longer estimate exactly the entropy of the final system; each step that is irreversible increases the total entropy at a rate that is hard to estimate. We thus get an incomplete result:

$$S \geqslant s_1 + s_2 + s_3 \ldots \tag{I.3}$$

The actual final entropy is necessarily larger, in case of irreversibility, than the sum of the entropies of the successive steps of the buildup.

3. THERMAL ENGINES

Carnot's principle has a very important consequence: It is impossible to transform heat into work as long as only one source of heat is used. Such an operation would mean, for this sole reserve of heat, a loss in calories; therefore, a decrease in the system's entropy. This is contrary to the absolute rule that the total entropy of an isolated system must constantly increase.