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QUANTIFICATION IN THE HISTORIOGRAPHY OF SCIENCES AND TECHNIQUES

For about twenty years now, the discussion on "quantitative history" or, better (as specified in an introduction I have written with O. Itzcovich for didactic purposes), on "quantitative historiography" has been intensifying. The issue at stake is the *Quantitative Work in History* discussed by the authors of a well-known collection edited by V. R. Lowin and J. M. Price (London, 1972), an issue we have been concerned with since the end of the 1950s. At once we led back the discussion to its theoretical knot, to the relation between event or act or occurrence (unique, unrepeatable but for analogy and, according to historicism, not measurable, but valuable almost unforeseeable in its peculiarity) and structure or system or assembly or class (this joins facts considered to be identical, measurable by virtue of their homogeneity, in which quality or value is given by quantity). It is known that these two worlds, that of event and that of structure, were deemed antithetical: in fact the former was peculiar to historiography and to the ever-moving Heraclitean flow, the latter was peculiar to sciences dealing with the permanence of the universe (what we shall call Eleatic).

For more than a decade we have been explaining in several editions of our *Introduzione alla storiografia* that, on the contrary, the two worlds are so much pervaded and interdependent, that it is not possible to conceive (apart from primitive intuition) of an event without any structure not referred to any event (at the limit in that provided with actual or potential existence in some element of itself or reduced to the idea of the being in some real way) is not even imaginable. I also explained that the identity which sciences talk about (from mathematical notions to crystals or to chemically simple bodies or elementary particles, though for these the matter is more complex) is a fiction to refer to, or an abstraction from real elements,[1] which we use according to the principles of formal logic; it has gained high degrees of axiomatization, but the historian can utilize it only to verify or warrant the internal coherence of his subject-matter. He starts from the principle of change, of the different, non-axiomatizable, because the introduction of constants (or of their elements, such as "whole," "always") contradicts change, so that it is not possible to speak of a logic of the concrete, *parallel* to the formal one, but perhaps of a metalogic, in the Gödelian sense, in comparison with the first, at least in some aspects.[2]

Formal logic tries to come out of the Eleatic kind of world, trying to explain changes, evolution, obviously within structures. Hence, for example, originated temporal logics in Italy studied by people like Scabia, whose pages attracted the attention also of physicists interested in these problems. Most of them abandoned for professional reasons the deterministic mechanicism of classical physics. But a few are still back-

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[1] Slowly scientists become conscious of this fact, as they are hindered by some beliefs: for instance, the impossibility of distinguishing the elementary particles of the same kind legitimately assumed by them as identical, even if the "reality of averages" has an evident conventional basis of operative utility (after G. Pompili and G. Dall'Aglio, *Piano degli esperimenti*, cf. L. Briatore and C. Castagnoli, *Storia dei concetti di evoluzione*, in: *Introduzione allo studio della storia*, Milan, 1975, edited by the author. These concepts have been expressed by us since the 1950s and recapitulated in *La scienza come storiografia*, ERI, 1970. We refrained from stating "ultimate" considerations, for example, about a global interpretation, though we attached to the needs (and problems) of both static and dynamic "totality" no less importance than that claimed for it by the great philosophical systems of the 19th century, already experienced by critical philosophy, like that of Rosmini, and now partly recovered by philosophers of science such as L. Geymonat (starting from different presuppositions), about the problems of the "universe" known or to be known, that is, of totality in nature (or, we add, in history) and science. Consequently, rather than profess this realism or idealism, we followed the phenomenology of a kind of knowledge such as the historiographic one, considering it the most familiar experience in our everyday research work. The "totality" which prevalently recurs is that of history, supposed to exist beyond historiographic interpretations, if each which is in its turn absolute and tends to assume and give a unitary statement structurally definable as a system (by its particular totality) at the root of a possible total reconstruction on the basis of the application of that system method (by assemblies, classes, etc.) to the philological analysis. But the historian is aware of the relative objectiveness of his totality reconstructed according to the chosen methods and which permits him a sort of axiomatization in the formulation of his "evolutive subject", ruled by formal logic as well as by the logic of the concrete, with explanatory procedures illustrated by the theory of historiography. On the inconstancy of constants, cf. A. Masani, *Sulla costanza delle costanti fondamentali*, "Giornale di fisica", 1977.

[2] As regards the updated philosophical developments of the historiographic themes dealt with in this context, we refer to L. Geymonat (*Scienza e realismo*, Milan, 1977); he adds dialectics, as the logic of the concrete, to formal logic. Dialectics appears to us only one of the methods of the logic of the concrete, profitable when in the course of history the negation of a former reality, a foregoing statement, i.e. a contradiction, occurs. Other methods and means of the logic of the concrete, like analogy and metaphor (or fiction, *als ob*), appear just as valid, though they certainly do not exhaust all the procedures of this logic.

ward, for instance, in comparison with biologists, in the analysis of indeterminacy and probabilism, perhaps just for some dogmas such as the identity of the particles of the same kind among themselves. Nevertheless the principle of complementariness, no less than the one of indeterminacy, about half a century ago already allowed them to approach formulations of an historical type through which not only cosmological (actually cosmogonic) problems could be dealt with, but even, as C. Castagnoli and L. Briatore proved recently, the problems of the history of matter. Also that of inorganic matter, because the historical interpretation of living matter had been for a long time tackled by biologists and had blended with the theory of the evolution of species. Therefore Fantini's use of the expressions mechanicism and the like as synonyms of causalistic determinism or "physicalism" in a subtle essay included in Volume VII of the *Storia del pensiero filosofico e scientifico* edited by L. Geymonat, does not seem to be fully correct. In reality, cause has correlation 1 to the higher limit of the probabilistic scale, in which the base zero means, on the contrary, case, at least in relation to selected elements.

In the historiographic interpretation, the probabilistic explanation is analogous to those in other sciences, so that in our studies *Scienza come storiografia* we took it as a prototype of scientific explanation and as a metaphor to denote our indifference or our incapacity for probing into correlations, and we used it "as if" there were no connection (connection zero); also determining cause is a metaphor denoting our indifference to go deep into a phenomenon: this way, we shall content ourselves with a constant correlation (at not too critical levels), i.e. "as if it were 1". Both metaphors ultimately simplify the subject, though they constitute more an escape toward explanation than a completely developed explanation: if (efficient, determining or final) cause were entirely developed, it would be extruded from epistemology and from any critical argument. Similarly, the notion of identity and, lastly, what we call determining cause, would revert, on the contrary, to connections which have characters or probabilistic values between O and 1.

Practical necessity not only leads us to these assumptions of "as if" but to very different conceptual fictions as well—from the permanence of constants to that of atomic weights; these fictions have to be considered in the (historical) reality as mere averages of the results of the experiments carried out.

Yet we must not conclude—as some neo-idealists have done—that these and other fictions relegate sciences (which are certainly not exhausted by them) to the world of the practical in order to elevate historiography to the world of the theoretical. Historiography, too, must continually resort to structures, fictions, alleged constants (mostly not expressed by numbers), just like sciences do. But historiography has perhaps a longer and wider critical consciousness of it therefore it is more cautious in generalizations and prefers to speak of preconditions, propitious circumstances and the like rather than of laws or trends. One reason for this is that historiography deals with entities—men—whose behaviour is very diversified, at least for some sectors of mankind, especially when one studies them as "persons" or individuals rather than as *anthropoi.* But the anthropological foundations of the action of human individuals and of the masses cannot be ignored by the historian who finds in them—together with the diffusion of the democratic attitude—more and more important problems to be treated. Hence he takes recourse to methodologies peculiar to anthropological analysis and to mass-phenomena, i.e. to quantifying operations, disowned by the aristocratic historiography of the "élite" thus showing indifference to the masses. In the 1975 issue of "Scientia" (vol. 110) P. Redondi devoted a lucid essay to the *Problemi epistemologici della storia quantitativa.* We too remain sceptical about "panquantification" because any merely statistical historical narration will be able to oppose a criticism of valuation about classes involving numbers, and classes are not definable by numbers but they may be designated with numbers conventionally only (i.e. after a non-quantitative argument). However, it stands to reason that, starting from elements which are anthropologically considerable in human events (climate, soil, installations, health, etc.) or more specifically economic (economic goods, land, capital, labour, production, consumption, services, etc.) or generally mass-elements, at levels which are not distinctly anthropological (electors, readers, researchers, inventors, soldiers, civilians, teachers, students, etc.), we must distinguish more and more consciously between what one can more easily trace back to organic evolution and what one can more directly link with extra-organic (super-organic, hyper-organic) or cultural or so-called voluntary facts. On the other hand, we acknowledge strong or feeble interactions, as Bellone says, between phenomena of anthropological-organic kind and the anthropological-extra-organic (super-organic or cultural) facts, in which learning and its presence at extra-organic (or more precisely, cultural) levels play a decisive role, as happens not only in the history of sciences and technology.

The quantification of anthropological facts proper, that is prevalently biological, is analogous to that of other natural events, at least within the limits of "long" or "very long" periods of time and of "deep" data concerning species rather than individuals. In fact the sciences which study these, from biology to physics, quantify abundantly.

As the naturalistic element subtended to economism blends with events supposed to be "voluntary", quantification becomes more difficult, when it does not conceal the ignorance of the case or the superficiality of the alleged discovery of the cause, as said before, in mass--phenomena concerning the same individual.

From such a point of view, which scientifically is surely not the most valuable one, proceed many approaches to quantitative historiography in its relations with historical and social sciences, with which political economy having the primacy of scientific foundation for more than two centuries now. Through the ideas of need and happiness (on which L. Trénard's studies should always be consulted) quantitative historiography dips its roots into physiology and psychology (and therefore into anthropology) examined closely in quantitative terms by that great economist, engineer and sociologist Vilfredo Pareto. All these quantitative aspects were outlined in the age of early positivism, though with some superficialities caused both by epistemology and by experimental methods that had rapidly been growing obsolete till World War I, while cultural anthropology was making decisive steps being applied to "primitive peoples." Anthropometry unfolded above all through data obtained from medical findings and by the end of the 1920s econometrics was taking shape, the former governed by biostatistical conceptions (which improved in medical or sanitary statistics) and the latter by economic science with the limits already mentioned. These limits were kept also in the historical version of the analysis of economic dynamics by the 1930s, while in Italy in De Maria's historical kinematics during the ensuing decades, while demographic historiography was being enriched with the results of genetics, though retaining rather superficial characteristics, as one can see in the aims limited to the reconstruction of certain "rates."

Only indirectly "external" historiography applied to physics and production technology could get some help from these as regards reconstruction of the socio-economic environment, as an exigency to set discovery, invention, research, into social history and that in a more general (not merely human but extrahuman as well) environment, as "geo-history" taught. Geo-history was propagated by the *Annales d'histoire économique et sociale* started by Maurice Bloch and Lucien Febvre in 1929, in whose path followed Fernand Braudel, E. Le Roy--Ladurie with his historical climatology and historical macro- and microspatial surveys began to appear, for instance, about oceanic transports or problems of regional history, up to the *histoire sérielle.*

But a completely quantitative economic historiography and, outside economics, the quantitative historiography of linguistics, date back to the 1960s; the former, as defined by J. Marczewski, was practised in national accounting almost on a parallel with the U.S. *New Economic History*, while the latter harvested, from a historiographic point of view, the fruits of the Chicago school of quantitative glottology.

The utilization of new sources permitted the passage from traditional demographic historiography to the historiography of spirituality, or at least of spiritual attitudes in certain anthropological facts, such as births, deaths, food, not to mention the religious or the cultural collective by starting with the technological training which for millennia had been found upon a history of agriculture but which recently became, as historiography, more and more interdisciplinary, from the chemistry of the soil to meteorology, from cultivated plants to the physiopathological consequences of their use.

The historiography of terrestrial physics had some rather indirect stimuli, though, through other fields (for example, concerning hydraulic, seismic phenomena, etc.) but the historiography of physics was inclined to remain a biography of physicists, with only occasional resort to quantitative methods: the old positivism had ventured the seasonal frequency of inventions, including the "absurd" ones too (for instance, those based on "perpetual motion") but ended by discriminating rather than promoting quantitative methods, not always wrongly ridiculed by the neoidealistic reaction. On the other hand, terrestrial physics could be utilized by historians, inquisitive not only of meteorological fluctuations, of the "drifts" of continents or of seismic phenomena (until the last century at the centre of a school of *Vulcanists* opposed that of *Neptunians*), not to mention what is related more properly to historical geology.

By comparison, the French quantitative historiography of the last generation designated by Chaunu as *histoire sérielle* achieved relative refinements concerning human cultural attitudes of an affective, sentimental, religious kind (for instance, by studying "baroque piety"), of a generically moral kind (like sexual behaviour with a view to birth-control), of an aesthetic-cultural kind (reaching "mentalities," i.e. certain intellectual and practical behaviours), of a participating-cultural kind (education), or of a material kind, but reaching the qualities of life and the individual, class or community "mentalities," quite different from the old demopsychology.

It is evident that historiography applied to education relies on statistics, on school institutions, teachers, students—also studied for their social origins—libraries and their readers, publications characterized sectionally and topographically. But none of this is yet a quantitative historiography of science and technology, as Costabel's instructions to historians on the use of computers and the mechanical processing of historical data of "archaeography" (S. Schmidt's term) are rather preliminary to a social or external historiography of science and technology. The pioneers of archaeography were the Soviet authors V. Ustinov and J. Khostova who devised a vectorial analysis for the classification and study of documents such that would ensure the retrieval of a great quantity of material unavailable by traditional methodologies and an extension of the subject of historical knowledge over the whole field of social phenomena (as Redondi explains). As regards external historiography, it can become internal when, instead of generalizations peculiar to political, economic, social sciences or others related to social history, it receives the requirements specific to various mathematical, physical or natural sciences.

Sometimes there is a certain coincidence, when, for example, social--economic periodisations fundamental to economic history, are also technologically considerable: the age of the first industrial revolution coincides with that of the technological revolution induced by the use of coal in metallurgy and in energy production, just as the second industrial revolution (often interlaced with the industrialization of the countries, for instance, of Central, Southern and Eastern Europe that remained agricultural) almost coincides with the revolution in new energy sources, or as the third industrial revolution takes place by the diffusion of electronics, neocapitalism, etc. Economic and social transformations induced by technological changes could find some historical confirmation in econometric analysis, as in fact it has been happening by now, though for a few years we have been pointing to the dangers or faults of it. Technological development is not only an increase in artificially "transformed calories" available to each person, as C. M. Cipolla stated, just as the value of an invention is not only in the function of its utility exploited in economic activity. Nor do we deem significant for internal or technological development, the mixing of physical units like the CV, with monetary data: economic historiography absorbs physical data in the function of its conceptions and schemes. Halfway between economic and technological historiography are Purs' studies on the asynchronism in industrial development and on the technological gap of the last century as is recent research into productivity and the maximum utilization of telecommunications systems with comparisons of assessments of monetary and technological costs with the respective outputs.

J. Piaskowski was among the first (at the ICOTHEC Congress in Jablonna in 1973) to supply a method of the technical measurement of industrial progress founded on the rationalization of productive elements, a method utilized by G. B. Scott to single out primitive productive organizations. The "technical historiography of technology" (histoire technique des techniques) which Febvre had wished for since 1925, with an evaluation of the part taken by science in invention and with the placing of technology among other human activities, began then to have its quantitative manifestations. M. Daumas had tried to quantify the "tension" factors which caused a technological event in connection with the concomitant events by the relation $F\ \frac{A}{B} = \frac{K}{T}$ to signify tension factors in the time interval *T* which separates the determining scientific or economic occurrence *B* from the determined technical occurrence *A*, while K is a constant including coefficients or reticles of impulses, filiations, receptivity, etc., differing from time to time. The shorter the times, the more intense the tension. As Redondi resumes, the discovery of atmospheric pressure by Torricelli and Otto van Guericke was an event that originated the Savery fire-pump, which in its turn, with the air-pump, originated the Newcomen engine. Apart from the "social" or inner elements which contribute to the "tension," i.e. apart from the nonscientific elements which enter into the computation, in a basic statement if it is at all possible, we can reflect after all only the Comtean opinion of technology as derivative to science, of innovations as consequences of scientific inventions, while today the line of demarcation is as uncertain as ever and, at least at some levels, it is impossible to establish differences if one only thinks of the elements of falsification coming to so-called science from so-called technology, to scientific progress of these and technical progress of those, and to the sophisticated empiricism of certain so-called technologies which produce results quite analogous to the reasoning that would guide the so-called sciences.

The quantitative "technical historiography of technology and science" or internal historiography is very different from that developing in the United States, by the effort of many people such as Rainoff, Merton, Derek and Garfield who are well known also in Italy due to the intuition of the publishers of translations (who generally pay little heed to studies carried out in our country), or such as Cardno in the history of psychology; it is not a question of quantifying the diffusion of scientific ideas as evidenced by publications, quotations or translations, nor the social origin of scientists, nor their means of research and teaching. All this, as said before, belongs to the history of culture. Also the Soviet "Science of Science" is relatively general, as an overall theory of the development of science which, as S. Mikulinsky puts it, is "the set-up of the theoretic bases of the organization, planning and management of science, that is a system of measurements founded on the objective logic of science development." Nor is it internal historiography, however philologically useful to the historian, when one analyses texts and scientific language in analogous of what is done in historical glottology, "in order to measure the coincidences and frequencies of apparition, in definite contexts, of scientific terms." Nor again is the analysis of *nosological* parameters qualified to show the synchronous relation among diseases (but also among pathological social conditions) as a statement previous to their diachronic knowledge of internal historiography. We write of it elsewhere with reference to the biological and medical sciences and to the historiography of health: let us only mention the historiographic meaning of *pathokenosis* formulated by M. D. Grmek in connection with actual pathological states correlating within a certain historical ambit and explained as epidemic.

The quantitative technical historiography of technology and science shares, as a more recent quantitative or serial historiography, the aspiration to model the historical subject and to consider itself scientific according to the contemporary principle of scienticism—that of probabilistic indeterminacy (or determinacy, which is the same), not in contrast with Marxist mechanicism if we remember the Marxian dialectic tension including nature. Undoubtedly not only the mathematizable is rational—French Marxists object—but, we add, the rational becomes clear and exact if it is mathematized. French Marxists seem to forget the nature-economy relationship basic in Marx, his enthusiasm over evolutionistic theory when he perceived in it a possible unitary interpretation, obviously when enlarged to encompass the superorganic, an extension that began then to be made though in the form of Spencerism and Social Darwinism, which Marx surely (and rightly) could not appreciate. The supposition that quantitative historiography may not state or interpret value of revolutionary events (for instance a scientific revolution) is as absurd as to believe that seismographs cannot measure a very violent earthquake only because a few of them have broken down. Soboul sensibly mentioned Lenin's historical-statistical interpretations. Operative choices made by quantitative historiography must obviously be expressed by the presentation of numerical data, as in any statistical series, and must be consistent with the chosen logical methods of explanation in terms of formal logic and of the logic of the concrete.

Whereas it is obvious that mass-events can find a suitable methodology of historical representation only in quantitative historiography, it would appear less evident that a technical historiography of technology and sciences must rely on the same interpretation. Let us consider the following: 1) historiography is not confined to the comparatively few technicians and scientists whose biographies are known but to very numerous (so much so as to appear innumerable) researchers and authors usually left in the shade, to say nothing of the difficulties in getting at their usual testimonies except for those referring to their work; it seems simpler and less redundant to limit the subject to the most representative ones; 2) the history of sciences and technology proceeds by propositions in which one can perceive subsequent reductions in the redundance of information. [3]

That is why the first serious endeavour to propose a historiographic methodology with regard to this remark about the reduction (not to be mistaken for the quantitative increment of useful information) can be made.

Meanwhile we point out that the reduction in redundance is an economic criterion (getting a utility, such as information, at the minimum cost); as we shall see, it is part of the historical progress not only as a diachronic phenomenon but it also appears as a trend connected with other trends, summarizable by a theory of evolution or of historical dynamics.

The first attempts to quantify the progress in the history of physical

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[3] Geymonat, *op. cit.*, explains how rationality and the "scientific technical heritage" of mankind grow, and synthetizes the main attitudes of contemporary philosophers of science with regard to this (from Popper to Lakatos, after the classical theories of Galilei, Laplace and Klein) through headways towards superior levels of explicative capacity and the achievements of new empirical data. In comparison with the growth of science, the historical events in which this proceeds seem to be "richer," as they include also failures and errors (or a lot of useless or untrue propositions among which the true ones could disappear if their evidence did not emerge together with their utility). When it follows the course of the growth of science, meant as a growth of truth or utility (we do not feel like asserting the latter is an attribute of the former, and prefer to consider them as two aspects or sides of the same reality), this "wealth" is anything but a linear succession, interlaced with revolutions (Kuhn) or more frequently falsifications (Popper) or new terms and relevant concepts (Bellone) or "research programmes" (Lakatos), this "wealth", we said, can be deemed *redundant* by the historian, getting him to separate the theoretical elements by which he schematizes the increase of the rationality peculiar to the sciences.

In fact, like all other sciences, historiography too tends to eliminate the redundant, and therefore rational reconstructions rise from it, not to be confused with the pseudorationality of aprioristic (hence antihistorical) schemes, typical, for instance, of the philosophy of history and a certain philosophy of science.

On the problem of "simplicity" or maximum unifying capacity of a theory see N. Maxwell (*Induction, Simplicity and Scientific Progress*, "Scientia," XIV, 1979), who on the one hand brings us back to the problems of the comparability of theories (cf. B. M. Kedrov, *On Scientific Revolutions and Their Typology*, ibid.), to their essentiality (cf. E. Agazzi, *Epistemologia, metafisica e storia della scienza*, Roma, 1978) or rationality (cf. G. Ganguilhem, *Idéologie et rationalité dans l'histoire des sciences de la vie*, Paris, 1977, and G. Radnitzky and G. Andersson, (eds.), *Progress and Rationality in Science*, Dordrecht, 1978), and on the other hand to the original intuitive element of science (cf. G. Holton, *The Scientific Imagination: Case Studies*, New York, 1978, ch. VI, "Can Science be Measured?"), and to *On Aesthetics in Science* by J. Wechsler (ed.), MIT, Mass, 1978) with the concepts of "elegance," "simplicity," "economy," "beauty," "the sense of rightness," "of inevitablity," "of perfect correspondence" often linked together when not coinciding.

theories were made by the researches of an "indicator," as A. Borsellino writes in *L' informazione e il progresso delle scienze* (in: *Il concetto di progresso nella scienza*, introduction; edited by E. Agazzi, Milano, Feltrinelli, 1976), i.e. the research of a quantity fit by its changes over some period of time to indicate the kind of evolution in the whole information gained by the observer of nature, according to Borsellino and Toraldo di Francia (*Il concetto di progresso in fisica, ibid.*). They both consider models and theories as a non-redundant recodification of the information supplied by nature. Borsellino, a biophysicist, regards nature as a source of information received by the central nervous system and specifies the "empirical data" provided by nature as primitive data in comparison with theoretical argumentation, which starts when one tries to eliminate data deemed redundant in order to symbolize, for example, a phenomenon by a function formula. This would then be a question of reducing the number of the coefficients of the polynomial relative to the function, in comparison with the number of data, in a way consistent with the experimental uncertainty (error) which is presupposed to occur in the data. Theories eliminate the redundance of the data that can be recodified in an efficient way and retain only the independent information about them.

The invariance itself of objects is a redundance and models must not be more ambiguous than the one assumed in the data, by what Bernoulli calls the "non sufficient-reason" principle. It is a matter of constructing the simplest algorithm in the sense of Kolmogorov with reference to the theory of information (which, through the definition of complexity, is connected with Godel's theorem of incompleteness) which should retain the independent information acquired. The reduction of the uncertainty of empirical data (for instance, with regard to the errors of measure) is fundamental in scientific progress.

From a statistical point of view, as Shannon views it, the theory of information indicates a measure of logarithmic type $I \approx \lg \frac{\vartheta_1}{\vartheta_2}$ to evaluate the informative advantage, if one improves the measure changing from ϑ_1 to ϑ_2.

Similarly the wider considerations of Toraldo, also presented at the Chiavari meeting (1974), though proposed only as "promising suggestions," follow the way of reducing the "redundance" of information about nature, but with a marked logical-historiographic consciousness. The statement that "the progress of physics helps to understand better what is progress in physics" means in fact that historiography of physics as well as of any other science or field, must stick to the concepts of physics. Unfortunately here Toraldo assumes as science not the one *in fieri*, but that of the results stated in books or other sources, i.e. not the process of discovery but the discovery as an acquired fact.

In spite of such a reduction (which keeps us from acknowledging science as historiography) the proposal can be used for a reduced quantification, i.e. restricted to results (regardless of the subjective costs), tested in their "validity" separated from historical context, from what is called "external history."

After having formulated the progress of physics in terms of "corroboration" and "historical scale" (remembering the "rule" of the single theories) more than in the "graveyard" terms of Popper, who means the falsification of theories, Toraldo soon leaves this way which—as we shall see—will resume some time later to re-enter into the debate in terms of a theory of information, and therefore of "redundance" taking as an example the case of the visual universe of an observer in a certain space-time.

The observer's eye can perceive in it 10 distinct points, in each of them it can perceive 100 luminous levels and different colours. Supposing the probability of each of the 100 cases is equal, the happening of one of them gives us information equal to: log [in base 2] $100 \approx 7$ bit. By multiplying the number of the afore-said discernible points, we deduced 10 bits of information. Imagine the time which would be necessary to process this great amount of information, since we can proceed with some hundred bits per second only.

In practice, instinctively or deliberately, the observer reduces those 10 points to a small fraction by choices. These are reflected in ulteriorly selected representations of participation, for example by drawing only outlines, as in maps, or, particularly effective, points or groups of points (as in the drawing of figures). Appropriate codes permit a reduction of that redundance.

It is easy to translate into bits the quantity of information obtained from a measurement in relation to its precision (Toraldo gives the example of the measurement of the temperature of a fluid). If the ε of precision tends to zero, information tends to infinity. But as every bit of information costs at least K ln 2 thermodynamic units (K is the Boltzmann constant), the entropy of the system in which the measurement takes place increases as much, and the specification ε is necessarily over.

The discovery of a law, as regards a certain subject of itself exemplified by Toraldo, could lead, with respect to a single physical system, to the equation $I = I_0 - I_1 = \log \dfrac{P_0}{P_1} = \log \dfrac{A}{a}$, in which a

is a strip in the total area A, P_0 is the number of cases possible *a priori*, P_1 the number of cases possible after experimentation. The generalization of the law would involve the multiplication by the number of such systems which really exist in the universe.

It is easier to rely on samples, on the averages of experimentations, and on a *nomothetic* language. But by doing it, we expose ourselves to the ever-present risk of falsification. Scientific explanation sometimes fills the gaps of sampling experimentation by linking together different experiments, thus enlarging in a methodical way the great deal of the data at the basis of a nomothetic proposition (the Stein paradox in statistics is reduced to this).

Toraldo uses a vocabulary with slightly different meanings: he calls *nomological* the knowledge by virtue of laws which concern classes of subjects and situations, and *factual* that concerning single objects and historical situations. But all the laws concern factual situations, our factual or historical universe: even the second principle of thermodynamics, which explains the increment of entropy, depends on certain initial or historical circumstances peculiar to the universe in which we began to think and formulate it. This brings us back to the concept of the historicity of science and of science as historiography, as a conscious inquiry into its situation, which Toraldo had at first discarded. His observations on the super-laws or the laws of invariance or preservation—mere devices of a logical process whose faults are evident—do not seem to modify our formulation on sampling and thus we point to the possibility of falsification. We cannot leave this out of consideration since to the physicist no ideal situation worded in well-known passages concerning the knowledge of all events of the present and of agent forces ever occurs. This knowledge would make us cautious in resorting to the laws and connected logical procedures peculiar to the physicist's work, for instance to spatially and temporally translated invariance, or to the relativistic invariance of the different classes of concepts called symmetries or, just, invariances.

The fundamental nucleus of quantifying methods proposed by Borsellino and Toraldo, in this phase, consists in introducing formulations characteristic to the theory of information, but more developed in connection with the redundance of the messages coming from nature (and, therefore, from sensations), than in relation with the intellectual process of reduction, different from what has been defined sample-taking. Toraldo then announce his intuition of progress in the "domains of theories" to which we shall return later.

The subtle pages of Jean Dieudonné, included in the same collection, do not carry us much further. He writes with regard to the *Idea di* *progresso in matematica* to illustrate "in what the progress of pure mathematics consists" with fine exemplification concerning the work of physicists. This progress applies to the solution of problems, the understanding of mathematical phenomena, the invention of good notations and useful algorithms (which does not seem peculiar to mathematics). Thus stated, those developments do not appear quantifiable to us, totally heterogeneous as they are: the first two items theoretical, the third one economic.

As by now the more promising way to reach the quantification of progress of sciences and technology appears to us the one which originated within the "internal" historiography of technology, with a clearly economic character, but not formulated according to terms of information theory.

The economic fact of the reduction of redundance is certainly correlative to an interpretation of economic type, but is perhaps expressible in a more general and complex way, i.e. in terms of efficiency.

The concept of efficiency as the ratio between input and output, or cost and income, dominates—besides political economy—the sciences of engineering and physics which involve factors of "work" (and therefore, for instance, thermal, optical factors) and, more generally, factors of measure and "resolving power," not to mention of physiology. But every science handles different concepts and hence the kinds of efficiency (for instance, their "peaks") can diverge. Physicists, accustomed to close systems, mass and/or energy preservers, cannot forecast "ideal" efficiencies exceeding 100% [1]; but as we do not believe in perpetual motion and are persuaded by the entropic interpretation of the second principle of thermodynamics, efficiency is always minor, in case no wanted lost or residual heat is not considered. Economists, as well, while not considering certain costs (for instance, of intelligence and of its training, of the unpaid part of work made possible by autophagy) deem the 100%, i.e. the 1, to be normally attainable on the market, and even to surmount, on pain, for instance, of the failure of a firm. Chemists, following Lavoisier, believe in the 1 in transformations; and we could continue, if it were not sufficient to draw the conclusion that, apart from the necessity of common measures (caloric units or other physical or also monetary units with constant and precise values), it is necessary to keep an eye on which type of efficiency we refer to and in which geographic-historical, social context, or, as we prefer to call it *technosystem*, the subject is inserted.

A sum of high individual efficiencies can turn, it is known, into collective failures—i.e. minimum efficiencies—so that it is possible to infer that optimal collective efficiences have excellent moral and ethical contents, if you assume morals or ethics as a science of behaviour. Hence the also quantitative explanation of historical and environmental disasters, collective degradations and the like and, on the contrary, of collective progress, owing to technosystems which did not clash but developed harmoniously in the wider technosystem of production, programming, fruition, etc.

Single sciences offer us paradigms and methods relevant to efficiencies, i.e. attainable utilities, and techniques are, at the limit, excogitations of optimal efficiencies. The economic character of all this does not require further explanations: it is less usual to understand that *true* proposition is also a *useful* proposition, that is the economic aspect of rationality—we wrote on this in an article *Storia della scienza e storia dell'economia* ("Cultura e scuola," n. 58, 1976).

To the more abstract debates on scientific truths we can substitute, at least within certain limits, the more actual ones about the utility of scientific truth, constructing so concretely quantifiable connections between techniques and sciences, or better, between technical realizations and scientific excogitations. We shall examine only some applications of the above mentioned concept of efficiency.

To the notion of provisional truth typical of falsificationism, we can draw near that, also provisional, of "efficiency" (i.e. susceptible of declining or increasing), typical of technometry; whereas the former is based on the exactness of logical formal control, the latter after all refers to the same exactness, but pursued through measurement and calculation, and its provisionality links up to the help of better efficiencies, as the help of falsificationism comes from the opinion that other formulations are possible (in case they are denied by the preceding ones), generally of wider or more powerful or definite truth, and therefore these formulations too refer to the economic aspect of truth or reason.

How can historical technometry be applied to the history of sciences? At first, for what concerns sciences directly equivalent—without any uncertainties and possibilities of objections—to techniques (as applied sciences, of engineering, etc.) or that give results translatable into fulfilments or realizations according to the principle enunciated by them, the methods proposed already in 1974 by the author in Tokyo, exemplifying them in thermodynamics and linked realizations in the field of heat-engines. The increasing of efficiencies of steam-engines, from the Newcomen engine to those of the mid-19th century, with values comprehended between a few thousandths of a unit and a few tenths (later quite exceeded by turbine-engines), is a precise evidence of the value of the increase of scientific knowledge in (theoretic and practical) thermo-dynamics, which finds its best theorization in the pages of Carnot in 1824. But these were the outcome of many observations of several scholars and practitioners and Carnot's proposition was known by larger circles much later. Before him they resorted to the formularies or principles of single scholars such as Watt, of engineers of undoubtable scientific value, but worried about many mostly collateral problems, able to increase over-all or global efficiency (minding mechanical efficiency, various dispersions, the best utilization of fuel, etc.), only moderately worried about efficiency individualized as thermic, just because they were unacquainted, in its clear formulation, with the principle which it was Carnot's merit to state precisely and which we acknowledge as true, in that still accepted nowadays so that we can say Carnot's proposition represents the same value 1 apropos of thermodynamics as science from 1824 to 1978.

We can set this 1 as numerator in the η of the formula which expresses that principle, and the result, which does not change from a quantitative point of view, denotes the full truth of that proposition, its ideal capacity.

But the ideal η of Carnot was really translated, in the best cases, into $\eta = 0{,}25$ (which represents the gap between theory and practice). Moreover, it had had many precedents and approximate formulations in relation with mechanical applications which, for the most part, are of even lower efficiency, and in a good number can be enumerated. Then the η of 1824–1978, we could also indicate η [1824–1978], we can compare other η's: If these coincide with the one of a Newcomen engine (we suppose improved by 1770 and deemed the best engine), we can write $\eta = 0{,}001$. In such a case the effective or realised η [1770] is related to that realized in the mid-19th century as $0{,}001 : 0{,}25$. The progress of thermodynamic application in the course of 80 years originates from such ratio.

We must add at once that the progress of *tribology* was moderate in that period, as to the applications to the steam engine, and therefore progress was mainly due to the observations of Watt who had noticed, among other things, that a lot of heat was lost by warming the walls of the cylinder at every cycle.

In fact, in Newcomen engines at every cycle it was necessary to cool the cylinder with water to condense the steam. Add the improvements by wheelworks, "parallelogram" and "regulator."

Watt's observations, though not explicitly translated into formulations expressed as the laws of the "science of heat," but as the rules for building "fire engines," surely have not the synthetic quality of Carnot's formulations, but find their place of honour in thermodynamic science:

its η, by the end of the 18th century, was a little superior to the already mentioned ratio and represents the validity of Watt's observations if we compare it with Carnot's ratio. Always it is easy to see numerically expressed in the field of "fire" engines progress realized in the middle of the 19th century, by Otto and then Diesel internal combustion engines, reaching from 0,25 to 0,35, etc., not to mention turbines.

The efficiency of the fulfilment of the Barsanti internal combustion engine compared with other foreign analogous ones, can give us an idea, apart from the gaps between the capacities of realization and theoretical propositions, about the condition of knowledge in this specific field. Then (technological or not) science is not the only one synthetized by great propositions and treatises, but that of everyday research work and tests we should call practical (or praxis), whereas a test inserted in a theoretical work (confirmation or verification, falsification) in its turn, very generically, answers the practical needs of men.

The phenomenology of knowing interlaces with the one of doing, sometimes in nearly inextricable (if we like, we can call them dialectic) ways, as well as the so-called external facts of science interlace with the internal ones: too much discrimination when there are no useful (for instance heuristic) may cause the loss of the sense of concrete unity. Besides the validity of a scientific proposition in relation to its applicative efficiency (this too is included in questions, requests) on the ground of diverse cognitive demands (from astronomic forecasts of biological and physical phenomena of every kind), one can value its Heuristic effectiveness in relation, on the one hand, to its simplicity ("to indicate in short") and, on the other hand, to its verifiable fecundity ("to indicate extensively"). These are evidently correlated terms and both qualities are included in theoretical propositions so that they could be called its scientific "pregnancy", but while the former is mostly verifiable by its purely theoretical conceptual analysis, the latter is more and more ascertainable and is the easiest to falsify. The term "pregnancy" or fecundity integrates itself with that of "convenience" and it depends on the physicist's humanistic sense to formulate with "polish" what Toraldo di Francia calls the "isolated laws" of the type I.6 that is,

$$f_1(a_\eta) = 0$$

it is possible to find a number a comprised between $a_\eta - E_\eta$ and $a_\eta + E_\eta$ so that $f_1(a_\eta')$ is exactly equal to 0, indicating by the former part of the equation the function of all variables a_η, such as the Galilean laws on the fall of bodies; in the history of classical physics we reach more complex or pregnant theoretical propositions, up to Einstein. From these a certain number of those laws of classical physics can be deduced. The progress in this direction (i.e. many regularities or typical functions enunciated in a closer and briefer way), with no measurable difficulty or specifiable in quantitative ways, as it were, corresponds only in part with Francis Bacon's Renaissance programme of deriving "axioms from senses and from the particulars, rising in regular and calm ascent so as to attain the most general axioms as final aim."

Apart from the question about how many theoretical or subjective elements pre-exist to experience (so as to perform not only its choices but also its meaning, so that experience appears a mere comparison with theory and a verification of itself at least in the meaning of nonfalsification), a physical theory *T* (at the lower limit, a law) is to Toraldo a set of observations or axioms, from which one or several laws of the above-mentioned kind can be logically deduced. We say at once that we adopt Toraldo's definition because it permits us to better understand his thought with a view to utilizing his proposals about the measurement of scientific progress as usable within our quantifying process; but Toraldo's definition indicates in the history of physics the arrival-points of previously more uncertain and one-sided propositions, only in the end reduced to the essential, as they say, and axiomatized, i.e. purified by any indeterminate and logically impure element. According to Toraldo, with a physical theory *T* it is always necessary to connect a field or domain of validity *D*. So the notion of validity as pregnancy or comprehensiveness or power is visibly quantified: this notion is as large as its field or domain. The *D* of *T* is constituted by the class of all the physical phenomena for which we know *T* makes forecasts in accordance with experience, by a description which may vary in the long run, i.e. historically, by virtue of the precision of the measuring apparatus used and of the intervals of value admitted for the different parameters involved. For the sake of brevity we leave out of consideration the problem of inductive inference, of the way in which physicists can pass from their limited (however great) experiences to a general assertion, i.e. by passing from the *majority* to the *whole*: that is to say we leave out the problem of certitude that *T* exists always within *D*, aware that the invariance of the laws in space and time is only a postulate, or, better, a preliminary condition of our argument at certain scientific levels, though we know that more scientific or critical ones exist, for instance, when we formulate the possibility of doubt with regard to this, or even when the opposite condition takes place, as historicism has suggested.

Another preliminary condition is that the law is as much expressed as possible in probabilistic terms, in view not only of random choices of the conditions of a macroscopic phenomenon and fixed or randomly chosen conditions of a microscopic phenomenon. An elementary probabilistic caution would be the one worded by the author about the probability of facts considered repeatable, through the uncertainty formula $u = \frac{}{\omega + a}$, in which the entities ω are self-renewing a. This caution should be adopted, when possible, to avoid the disparity between the generality of achieved (verified) facts and facts begun but not yet concluded. But also with this index of uncertainty we are very far from the basic lines of historicism, because we should determine other indexes, as, for instance, that of specific variability, in order to infer from the analogous to the identical or to assume the analogous as identical (always unconventionally).

But leaving all this aside, let us envisage just the importance of the domain D for the progress of a physical theory T and its measurability.

The improvement of measurement consists not only in higher approximation expressed by the usual units, nor, for instance, in resorting to new systems and new correlated units of measure which allow "sharper" measurements. This improvement is an element of that progress from "approximation" to "precision" pointed out somewhat summarily by A. Koyré, easily quantifiable just because it concerns the measures as they are introduced and applied. If then an experience E does not fall within T and, on the contrary, seems to falsify it, the physicist, by using his intuition and logic, can elaborate a T' which will include E, besides some experimentation for confirmation.

In such a way we shall have T_1, T_2, T_3, ... valid in larger and larger domains, at least in terms of the general principle of correspondence, i.e. in D_1, D_2, D_3 ... . The necessity of inserting new Es (experiences) in contrast with T, can cause a hypothetic proposition H to rise provisionally, to be added and then absorbed by T for reasons of economy: they can include, we think, the trends to "unification" and "harmony" (terms recurring in Toraldo, but not explicitly explained). The congeries of the electromagnetism laws during the 19th century was due to the Maxwell equations unified and the outcome was a new domain of unified theory (which, however, does not contradict previous theories), so "powerful" as to include electromagnetic waves, subsequently, with Hertz. With his special relativity theory, Einstein widened the T with his D, which he had inherited from Maxwell and Hertz, so as to include both the already existing hypotheses H of electric charge moving within a magnetic field and being acted upon by a certain force, and the Lorentz and Fitzgerald length contraction necessary to explain the outcome of the experience of Michelson and Morley. The T of Einstein, of course, which could be called T_3, if we call T_1 the one preceding Maxwell equations expressible by T_2, has a domain D_3 so powerful as to include new phenomena like time dilatation and the equivalence of mass and energy. Einstein himself, extending T_3 to T_4 with the general relativity theory, succeeded in including in it also the hypothesis of the equality of inertial and gravitational mass. The respective domain D_4 was then confirmed by the results of 1) the experiments on the real precession of Mercury's perihelion, 2) the effective deflection of luminous rays in a gravitational field, and 3) the effective red-shift of spectrum lines in a gravitational field. [4]

Therefore the "historical scale of theories" can be conceived of as an enlargement of D, which involves reductions in propositions, that is, increases in power at least within the limits in which the enlargement of a previous theory or hypothesis is justifiable by the coherence of linguistic terms, by their translatability, as happens between classical and relativistic physics. But many other kinds of "historical scales" exist.

The problem seems to be harder for the historian when translatability is uncertain or even impossible, because in such cases the homogeneity of the terms to be compared, a presupposition of any quantitative operation, is given up. [5]

We must state at once that such obstacles are extremely difficult to find in the last four centuries of the history of physics: in fact they all

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[4] While in the history of technology the specification of the technical means to be compared in order to ascertain the progress or the increase of rationality (i.e. of efficiency) is relatively easy, the analogous operation in the history of sciences is more difficult: in fact the former avails itself of the theory of technosystems, i.e. of directed technical systems directed towards a more or less general aim (for example, motor transport by wheels) and the kind of technical means to be employed (for example, internal combustion engines which can use different fuels); this we can read, for instance, in the treatises of technical physics, or in the headings of patents. In contrast, in the history of science, it is necessary to turn our attention mainly to the types of cognitive problems (for instance, the dimensions of the Earth) in which we would place the pertinent attempts at answering (for instance, astronomic considerations or remarks of geographic explorations or of triangulation, gravitation, etc.). The efficiencies of such answers will be appreciated by the usual method expounded by the theory of technosystems, which is simultaneously a particular case and an improvement of T. Kotarbiński's *praxiology*.

The genetic collocation both of a problem and of an explicative argument must agree with the formal one, with the object of a more appropriate characterization to avoid strongly any possible heterogeneous mixtures, a thing easier to be done in the history of mathematics and physics.

[5] With regard to this, in addition to what has been said in the preceding note, we must recall the "philologic" or, more generally, the historical importance of the proposal of E. Bellone (*Crolli, costruzioni e dizionari*: *congetture sulla storia della scienza*, "Scientia," 1976), who rightly warns against some false ways of practising the history of sciences, such as those based on a "breakdown" theory or on a certain aprioristic "construction," as scientists were the perfect agents of a methodological, rationally inescapable strategy, according to a philosophy of science. The dynamic of knowledge is framed into practice and not reduced to a single logical structure, least of all of the formal kind.

belong to modern and contemporary history. Let this be clear: some terms may appear "untranslatable" today, for they lost any sense in the sphere of our science, that is, in our opinion, they have no more significance, from the viewpoint of scientificism in the cultural heritage accepted now, or to put it differently, they are obsolete terms or belong to an almost completely different kind of scientificism. Over the past millennia various criteria of scientificism have been suggested, which could explain, each in its own way, a series of phenomena, and even produce and predict them with a certain exactness. The Ptolemaic system, for instance, could "serve" both for practical needs in some cases not different from ours (in other cases it proved erroneous and powerless) and "to give explanations" in conformity with the criterion of scientificism of its time (that of Aristotelian physics). Despite contrary appearances, the characters of practical needs and scientificism criteria are closely related: in the Ptolemaic system, knowledge was approximate, both from the theoretical and practical points of view, and the practical limitations also condition its theoretical limitations, as reflected for instance in the measurement of time.

In today's way of thinking, other terms have only a "fantastic character" and therefore their translatability appears minimal or even nonexistent. But let us not forget that they, too, lived in a generically experimental context of logic and practice, however slight, and although today we repudiate them theoretically, we must always consider them in connection with the criterion of scientificism of their time and evaluate them, for example, in relation to the degrees of extraorganic or superorganic or cultural evolution, as we prefer to call it.

Therefore the quantification of scientific thought can be accomplished taking into account various parameters, among which the economic criterion seems to be fundamental as an index of rationality: that is, the economic criterion as valid in the given domain, in the external economy which is strongly related to the given scientific domain, and, lastly, economy as a concrete possibility of applications.

Here the debate could reestablish the dualism between science and technical applications, physical theory, for instance, and the engineering sciences. But where a gap is determined, we are mostly in the presence of pseudopropositions: when we say, for example, that now it is theoretically (= scientifically) possible for men "to fly" to Mars, the term is inexact, both from a scientific and a technical point of view. Our physical, biological and other sciences have not yet solved a series of so-called technical problems involved in a "flight" to Mars. "Technical physics" is doubtlessly a science and a technology.

The above-formulated economic criterion in the quantification of scientific progress has an obvious importance, even if its applicability is subordinated to "historiographic experimentation", an idea up to now rarely put forward. One of the few outstanding historians of biology and medicine to have done so was L. Belloni, who suggested the repetition of experiments by means of instruments that are also historical. For the history of sciences and technology it is not only a matter of having at our disposal museums with very large collections of tools and machines, historical means of scientific production, such as, for instance, steam engines, electrical apparatuses, but they have to be re-operated in reconstructed historical conditions. Only so shall we be able to establish in a philologically correct way the essential data, for instance, of efficiency for quantitative historiography: from the Galilean telescope to the Barsanti internal combustion engine or the Galileo Ferraris rotating magnetic field.

But these "economic" data, in the above-defined connotation, cover only one aspect of the internal history of sciences and technology. We must also examine the efficiency of propositions called theoretical, and thus the methods proposed by Toraldo find their application.

In conclusion, all data are to be included in a wider quantitative interpretation which, as suggested, may be linked with that of evolution: the evolution of matter in the universe pertains strictly to the research domain of physicists, for the time being on the basis of preliminary classifications of matter particles, etc., and with their distribution across the universe in different "states" with quantitatively describable physical-chemical processes.

Organic evolution has so far been quantifiable within the limits of biochemical and genetic processes, usually assumed to be more like samples, schemes or examples. However, all historiographic work (as in other sciences) proceeds by specimens, samples, while extraorganic evolution (in Italy more commonly known as cultural evolution) is the traditional ground of historiography as defined at the outset.

The general view of extraorganic evolution, divided by us into superorganic and hyperorganic evolution, can profit from a periodization (which we proposed elsewhere) into "degrees" (hence numerable) of evolution, connected with the capacity progressively acquired by man for strengthening his own organism, so that he replaces it at least partly, even in his highest (for instance, decisional) faculties. This periodization is the most traditional operation of quantification performed by historians, but is limited to the chronological data of its prevailing characteristics—expressed by the number of time units. The afore-said considerations on evolution fall within the framework of theories of automation, which, in its highest degrees, would seem to reach analogous results that, in forms of negentropy, are similar to those of intelligence, but which is nevertheless subject to the generally prevalent entropy—the historical conditioning and other specific conditioning.[6]

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[6] It is easy to see in biophysical studies, for instance, of I. Prigogine (*Time, Irreversibility and Structure*, in: *The Physicist's Conception of Nature*, 1973 (cf. I. Prigogine, I. Stengers, *La nuova alleanza*, "Scientia," 1977) how evolutionistic and historicistic theories influenced the present-day reflections on thermodynamics and the production of entropy in closed or open systems and stimulated research on the exchanges between systems and environment, on equilibria, structures, fluctuations and differentiations. Sound considerations sprang from it in rather different fields such as sociology and economic science.