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IS SCIENTIFIC OBJECTIVITY POSSIBLE WITHOUT MEASUREMENTS?

According to a widely accepted opinion, the most typical characteristic and even the constitutive element of science is measurement, i.e., those processes of measuring upon which science is based. For a long time this has caused a general orientation of disciplines seeking to call themselves "science" toward a certain form of quantification; in order to achieve the prestigious title of "science" some form of measurement, of whatever kind, had to be introduced into the area of study.

The most striking example of this phenomenon occurs in the human sciences such as psychology and sociology where a kind of inferiority complex has created a general tendency to imitate the methods of the so-called "exact" sciences and to establish a series of scales and criteria of measurement.

There is the danger that such a widely held and solidly established opinion can become indisputable. However, if we attempt to approach it from a theoretical point of view, a critique of this opinion is necessary. What are the reasons for the prestige of measurement? How can we explain or justify this idea? One

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of the most obvious reasons, important but not unique, for the distinctive position accorded to measure is of an historical nature.

One need only consider the development of "modern science" (i.e. since the Renaissance) particularly in its most dynamic and characteristic disciplines such as physics and chemistry. This development sprung from an important theoretical base and led to an astonishing and practically unlimited series of technical applications. In light of this headlong evolution, which in the brief interval of three centuries has changed radically not only our perspective on the world but even our very way of life, it seems impossible not to recognize that the introduction of the method of measuring constituted a decisive factor. For from that moment forward it was possible to formulate "exact" mathematical laws; their abstract and general nature permitted an understanding of the physical reality which underlies broad theoretical syntheses and simultaneously allowed nature to be conquered by technology, particularly since these laws could be adapted to any particular case capable of producing numbers which could be substituted for variables in equations. If this is so, how can it be doubted that measurements form the keystone of all scientific research in whatever area?

We might add that another factor reinforced this tendency toward quantification and mathematization: the normative and even paradigmatic role physics twice played in scientific reflection. The first time was in the 18th century when Newtonian mechanics in its marvelous development became identified with the very ideal of knowledge to such an extent that Kant adopted it explicitly as a model for all cognitive research in his critical philosophy. Then in the 20th century with the revolutions of the theory of relativity and the quantum theory, physics became the ideal model not only for the scientific world, but also for philosophy and even the public at large; so much so that physics seemed *the* science par excellence. Since physics obviously strongly leans upon mathematization and measurement where everything can be reduced to the quantitative and the measurable, it was natural that there was an immediate and spontaneous transferal of conditions proper to the model (physics) to other sciences.

It is easy enough to outline these historical explanations; but this only touches on the question of fact and not at all on that of right. In other words are there other causes, other reasons for the privileged place occupied by quantification and measurement, or is this place simply the result of an historical consequence which, as such, can and should be eliminated as soon as the historical contingency is established which brought about the present situation?

For those who assert the privilege of measure do not believe it to be determined by simply historical reasons; on the contrary they judge it to be based in the methodology itself. Only measurement can allow us to surmount the limitations, the ambiguity and the imprecision of subjective and personal judgments concerning diverse aspects of reality. Thus according to this opinion quantification and measurement constitute the necessary means for overcoming subjectivity and for creating scientific objectivity. This is the methodological thesis which we must discuss, even though we should keep in mind at the outset that this thesis is based more on historical presuppositions than on epistemological analyses.

There is a precedent to this thesis which is easily recognized in a famous philosophical doctrine. The first clear and explicit formulation of this appears in a well known passage of the *Saggiatore* of Galileo and it was frequently reformulated by philosophers of the 16th and 17th centuries. This is the doctrine of the so-called primary and secondary qualities. By primary qualities were intended form, extension, number, location in space and time, quantity of matter, weight, etc. These correspond to the characteristics of reality which can be reduced to a mathematical formulation. They were thought to belong intrisically to an obiect and to exist independently of any subjective judgment. The secondary qualities included colors, odors, roughness, temperature (hot or cold). These correspond to characteristics which did not seem necessarily proper to the object itself, but which result from the object's encounter with some kind of organ of sense, characteristics which appear derived and subjective.

From this issued a kind of identity between mathematical and objectively permanent qualities on the one hand, and, on the other, between non-quantifiable and purely subjective qualities.

The latter, said Galileo, disappear once the perceiving animal is removed. This seemed to indicate their non-equatability with the real, while primary qualities are part of the intrinsic structure of the real and they alone merit study.

It is easy to see how this philosphical dogma (and it was generally agreed in retrospect that this had become a dogma) brought with it as an immediate consequence the favoring of measure. Measurement became the natural instrument for expressing the " primary qualities " essential to the real object upon which was super-imposed the illusory mask of the " secondary qualities. " However, even after this conception lost its dogmatism and was purged of its gratuitous dualism at once gnoseological and metaphysical, it still retained its strength in the area of methodology. For it was still believed that only measurement allowed the possibility of achieving objectivity; it was the only methodological instrument capable of eliminating idiosyncratic and particular subjectivity always more or less misleading.

Let us see if such a general statement can be justified. Certainly no one can deny the value of measurement for eliminating those misleading subjective elements which can crop up in descriptions of reality. But it is necessary to determine if measurement is in fact or in principle the only instrument available to science for this end. Once again, it would seem that we are faced with a purely historical situation: we are conditioned by a certain manner of thinking which has been impressed on us in consideration of the enormous success brought about by the measuring method in certain areas of research.

But this should not signify an automatically exclusive or superior position any more justified than the position given to mechanics at the end of the 19th century. For it was then that it began to be suspected that this discipline, highly valued among all the branches of physics, could not remain the conceptual framework for all of physics, nor could it guarantee a logical foundation for physics. It is the same with the method of measurement and quantification: perhaps after all the astonishing services it has rendered in various scientific disciplines, the limits of measurement are also now becoming apparent.

To see this more clearly, let us try to examine the means which science can use to avoid the trap of subjectivity and to circumscribe its area of inquiry.

With regard to the trap of subjectivity, let us see why a simple accounting of personal perceptions is seen as useless for science. Normally it is said that a subjective estimate is vague and unreliable, which we will see shortly is not always the case. The decisive reason for this is that a subjective estimate always remains as such inexorably "private" and difficult to communicate. For example, what could be more vital, clearer, more obvious and more subtle than an individual's perception, let us say mine, of the soft green of a blade of grass. I can hardly re-create even a fragment of this rich ensemble of information by citing the wave length of light which corresponds to the green of the grass. We could therefore say that in this case the perception of an individual is more *precise*, more exact, richer than the quantitative expression obtained by a measuring instrument. (This is not always so, however.) It is undeniable that this abundant wealth of information remains always imprisoned within the limits of my consciousness and my perceptions, while the numerical wavelength is a communicable piece of information and consequently constitutes a "public" datum. Science, however, attributes a great importance to bits of information of this kind; they are valued not only for their supposed exactitude, but because they are completely intelligible without recourse to any personal perception. Thus in the case of the blade of grass, measuring the wave length of the green is an operation which forces open the "privacy" of my perception and makes of it a fact accessible to others.

At this point it is necessary to isolate exactly the factor causing this transition from "private" to "public." It is possible, erroneously, to think that this transition occurs because of the use of numbers whose abstract and universal nature puts them beyond the limits of individual consciousness and makes them understandable by everyone.

We shall see that this is not so, for subjectivity actually is overcome by the use of an *operation*. That it is a measuring operation however is of secondary importance; the determining factor is that it is an operation. As a matter of fact, the history of philosophy shows us the total futility of the long series of efforts which attempted to break the barriers of individual knowledge by using *only* cognitive means. This is absolutely true: agreement between different persons (i.e. intersubjective) concerning whatever kind of notion, either empirical (as in the case of a color) or abstract (as in the case of a mathematical concept) cannot occur at all by virtue of a mutual ascertainment of this notion which is contained in the individual consciousness. In other words, one cannot perceive the perceptions of others nor conceptualize their concepts in order to compare them to one's own. On the other hand it is possible to *employ* certain concepts in a uniform manner, for the *use* of a concept can be the object of observation, and one can establish an agreement on usage. And this occurs only because in using concepts we must necessarily use *operations* which are not part of consciousness, but which derive from empirical, observable facts which permit an intersubjective agreement on the meaning of certain notions. Of course this process always retains a cognitive aspect; it is necessary to ascertain the operation employed and one always aims toward acquiring the "knowledge" of a concept. However, the fact remains that the "public" dimension of this cognitive activity is assured by a practical element.

From this we can conclude that, in order to overcome subjectivity we must establish operations capable of guaranteeing a uniform use of those "predicates" which in turn guarantee intersubjectivity. If certain of these operations *can* sometimes result from operations of measurement, still measurement itself does not seem to be an essential condition. It is not absolutely necessary, therefore, and even if in certain cases it is desirable, this desirability results from reasons that do not directly affect the question of intersubjectivity.

How each science goes about determining its proper area of interest is a problem requiring careful analysis. Here we shall limit ourselves to an outline and to the observation that the choice of a specific object for a science does not derive from a selection of particular "things" with which a given branch of science will occupy itself, but rather this choice depends on the determination of a specific "point of view" from which the science in question considers all things. "Point of view" here means in practice the proposition of diverse specific "predicates" which constitute the technical language used by that science to speak of reality. For it is precisely the totality of these predicates which effects the particular "cross section" of reality which constitutes the distinct scientific object of the science in question. Some of these predicates connect scientific discourse to an experimental foundation. Others permit the construction of a theoretical discourse which is related explicitly (although indirectly) to the experimental foundation. We can call these then the "basic predicates" for their distinctive characteristics depend on the fact that every proposition which they (and only they) constitute must be immediately accepted or rejected for this science on the basis of a factual (or empirical) consideration. Thus we see that the factual (or empirical) character of a proposition is not absolute, but totally relative to the criteria admitted by this science on a factual or empirical level. This in turn means that each of these predicates must be subject to an effective control procedure which permits us to decide each time the predicate is attributed to a given " thing " if yes or no this attribution can be accepted. The procedures to which are subject predicates which exercise the role of criterion for an empirical judgement will of necessity be of an *operational* nature.

From this we can conclude that in order to effect that delineation of reality which constitutes its proper area of interest, every empirical science needs to introduce basic predicates defined in an operational manner which permit the science both to specify its data and to decide if its empirical propositions are true or false.

Thus there is a perfect superposition of criteria that are operational (necessary to guarantee the intersubjectivity of scientific discourse) and of these same criteria inasmuch as they are tools necessary to render precise the structure of the group of objects specific to a science. In other words, we can note that the conditions which permit the definition of the objects of a science are the same as those which permit knowing these objects in an intersubjective manner. They are of an operational nature and constitute the very foundation of scientific *objectivity.* In both cases we must have recourse to operations, but without ever adding as a supplementary condition that these must be operations of measurement. It follows thus that scientific objectivity is based on the fact of operations, but not necessarily on measurement.

We shall give a few examples before sketching an analysis of the reasons which argue in favor of quantifying and measuring in order to appreciate their advantages and at the same time to examine under what circumstances they may be employed without risking a deformation of the objectivity which interests us.

In the course of their history as well as in their present state, the sciences offer us examples of predicates which can be held as defined in function of an operational criterion but without in any way implying a use of measurement. Thus the notion of "historic fact" can be defined as that of an event which can be immediately verified by "documents," presuming of course that the historian is capable of judging the documents and of using them to create a reliable statement. (Just as a physicist must know his instruments in order to use them in a way that inspires confidence.) But it is evident that knowledge gleaned from documents is not, generally, of a quantitative or measurable nature.

If we move on to biology, we notice that it was possible after centuries of groping movements to give some kind of precision to a concept as delicate as that of a species thanks to an operational criterion according to which individuals capable of generating fertile individuals are considered members of the same species. This criterion is not quantitative, just as the majority of biological concepts are not quantitative. In chemistry, an operational criterion which is purely qualitative, litmus paper, is still deemed sufficient for determining the acidity or non-acidity of a body.

Physics itself offers examples of predicates of a purely qualitative nature, such as "electrically charged," "negatively charged," etc. which originally were associated with procedures which had no connection with measurement such as the attraction or repulsion of objects to materials electrified by rubbing. However, even in the physics of today, the application of a magnetic field is often employed as the qualitative criterion for separating negative and positive particles.

It is not our intention to insist on particular cases; we do not espouse the obscurantist thesis which says " science can

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do without quantity and measurement." We will attempt to illuminate the extremism of the opposite thesis: "science can do nothing outside of quantity and measure." Thus it will be more interesting and more fruitful to ask ourselves why measurement proves to be so useful since wherever it has been introduced in various areas of research it has brought about decisive advances, even though in principle it is not indispensable for a scientific report.

A first answer is obvious: when empirical data are clothed in a numerical form it is much easier to discover certain "regularities" and hence "empirical laws," to establish correlations and true functional relations. These are all elements which are conducive to the construction of hypotheses and of a "scientific theory."

A second response, perhaps less evident than the first, is fundamental. Through the mediation of measuring processes we arrive at a homomorphic representation of the structure of our universe into a certain numerical structure. Thus it is possible to translate properties, relations and functions verifiable in our universe into *numerical* properties, relations and functions, logically equivalent to observable (but not quantified) properties, relations and functions. As a consequence, each demonstration of one set of properties is valuable for the others. From then on we can adopt the *language* of mathematics to describe our objects with all its versatility and clarity, with its power of synthesis and its instrumental richness. The use of mathematical *language* is of a decisive advantage for every theoretical construction in which certain propositions are destined to play the role of "principles" or of "laws" on the basis of which certain "regularities" must be explained, hypotheses confirmed, previsions verified, etc.

We know that all these operations are only applications of a general deductive methodology which can be sometimes quite complex. But when the initial propositions are of a mathematical nature (i.e. in effect equations) the deductive path is radically simplified since one need only apply correctly the mathematical calculations to obtain "results": explanations, verifications, previsions, etc.

This is the essential reason for the mathematization of scientific discourse, and it is worthy of the most serious consideration. We can note, however, that it is a purely practical and pragmatic reason. Unless one holds that " the structure of reality is in itself mathematical " (which cannot be proven in all the forms of reality), then one must admit that this mathematization is fruitful, not because of its correspondence with the specific structure of objects, but because of the perfect formal structure of mathematical language.

But this raises an observation: it is not because it is numerical, but because it is formal that a mathematical calculation presents this precious characteristic of being exact, explicit and automatic. As a matter of fact, one always calculates by means of rules which show us how to manipulate the signs, and it is only at the end of the calculations that one assigns a numerical value to the results by attributing numerical values to the variables. This shows that in a scientific theory in order to profit from the already-mentioned advantages of a linguistic nature, the essential factor is the formal operation and not measurement properly speaking. Certainly measurement, by giving rise to numbers, can facilitate the introduction of certain formal calculations (mathematical calculations), but there are other formal calculations (such as logical ones) which can exercise the same function.

To be even more precise, we can say that mathematics itself cannot be reduced to just a science of numbers and quantities. We will come back to this point, but already we can see that the numerical aspect is not essential to mathematics as such, or, perhaps, that the numerical aspect represents only one part of the complex mathematical structure. This would then imply that it would be possible to "mathematize" the area of interest of a given science without forcing it into an operation requiring quantity and measurement. It would be sufficient to apply as *language* a sort of " qualitative " mathematics (of which there are several interesting examples today) in order to benefit from formal calculations without submission to measure. In this sense it would be possible to accept the famous declaration of Kant according to which a given discipline is scientific in as much as it is mathematical, as long as " mathematical " is understood in a

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broader sense than that intended by Kant, i.e. in the sense of "formal operation."

This is not the place to insist on the merits of formal operations in the sciences. Let us say only that the formal method does not consist in " formulas " and abstract symbolism, but in the total explication of the presuppositions and the demonstrative procedures used in this or that discourse. Symbolism, when it occurs, facilitates enormously the deductive mechanism, but the absence of effective symbols does not prohibit formalization from fulfilling its role. This could be identified as the " semantic function " of the axiomatic method (to take the most typical example). We know that the axioms of a mathematical theory are today conceived as vehicles for rendering implicitly precise the exact *meaning* of the different notions of this theory, even before serving as the point of departure for possible deductions. However, this function of vehicle must not be limited only to the mathematical disciplines. Even for the empirical sciences it is quite useful to adopt primitive propositions which contribute to fixing in an exact and explicit manner the specific meaning of certain notions, especially those which do not have operational definitions. Formalization conceived thus seems indispensable to every science without, however, this meaning mathematization in the technical sense of the term, nor *a fortiori* quantification or measurement.

In support of this, let us add that measurement and numbers do not function dictatorially even in mathematics (which perhaps the most intransigent partisans of measurement have not yet realized).

Without going into the question of newer and little known disciplines outside the circle of professional mathematicians such as topology and abstract algebra, let us look at the familiar example of geometry. This is probably the oldest and clearest case of an objective and rigorous discipline in which only a very small part has to do with measurement. This is true for elementary geometry (based on Euclid's *Elements*) as studied in secondary schools. Only relatively late does geometry arrive at surfaces and volumes after long and purely qualitative developments (in the sense that they deal with " properties " of figures which do not involve their measurement) and basing itself on these developments. But this is even more true in projective geometry which was able to liberate itself totally from metric geometry already in the course of the last century.

We can go even further with geometry. It is hardly necessary to be a specialist to know that the creation of non-Euclidian geometries and their progressive affirmation between 1830-70 profoundly changed the traditional notion of geometry as *the* science of space *par excellence*. With these non-Euclidian geometries, geometry came to be seen as a plurality of different systems, all equally legitimate. The problem then was to propose conditions which, while respecting the plurality of possible geometries, would at the same time render precise the general characteristics which permit the attribution of the label " geometry " to a mathematical system. It was Felix Klein with his famous " Erlangen program " who brought forward the solution: the title of " geometry " can be given to every discipline which studies those properties of figures which remain unvaried with respect to a given group of transformations. Consequently, each group of transformations identifies a geometry, and these geometries can be classified in a hierarchy of increasing generality. However, we notice that, among these geometries, only one is " metric " (corresponding to a " metric " group of transformations), while the others are not, although these are equally important and interesting as metric geometry, sometimes even more so.

We can determine something comparable to that which occurs in mathematics in general and geometries in particular with regard to the properties of things which are the object of other sciences. These properties can be conceived as "invariants" in relation to certain transformations of an operational nature. Exactly as in the case of geometries, we can note that only certain of these operations can be seen as operations of measurement giving a "metric" character to the predicates which they express. It follows that to impose an indiscriminate measurement on these predicates risks altering their nature.

In this respect, we should recall that the procedures which allow us to associate *numbers* to objects are not necessarily all " measurements " for these objects; it can happen that these procedures are only means of indexing or labeling objects. For there to be measurement, a relationship must be established between a numerical *structure* and the *structure* of the group of objects under consideration. This is not guaranteed by the single fact that the objects possess a numerical label. It frequently happens that the structure which, thanks to our predicates, we are able to reconstruct in the given group of obiects is not homomorphic to any of those structures which we ordinarily express through real numbers. This implies that no effective mathematical theory (which always has to do with one or another of these latter structures) is applicable to our objects, and this reduces to practically nothing (or at best to a purely verbal declaration) the supposed measurement which we thought we had found through the act of numbering.

In other words, if we call *scale* a structure imposed on real numbers for the purpose of utilizing it to effect measurements, we know that only a very limited group of structures can play the role of scale, (as is proven in the general theory of measurement). In general a structure which is capable of doing so will impose on real numbers (denoted by *R*) in addition to the usual relations of identity (=) and of order (<), another special relation, here indicated by a letter followed by variables. Limiting ourselves to the best known scales we can determine that:

1. Ordinal scale: $S_1 = (R, =, <)$
2. Scale of internal: $S_2 = (R, =, <, I)$,
   with $I\ (x, y, z, w) \longleftrightarrow |x—y| \leq |z—w|$
3. Scale of difference: $S_3 = (R, =, <, D)$,
   with $D. (x, y, z, w) \longleftrightarrow x+y \leq z+w$
4. Scale of quotients: $S_4 = (R, =, <, Q)$,
   with $(x, y, z, w) \longleftrightarrow xy \leq zw$

Without going into an explanation of these conditions, we can note simply that each predicate (I, D, Q) is expressed by means of a precise condition, definable in terms of operations which are more or less simple but which in any case are known and explicit and working on real numbers. (In our examples these are only the basic operations of addition, subtraction, multiplication.) Obviously besides these four which we used as examples, there are still other structures qualifiable as " scales " but their number remains rather limited; and, in any case, they

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must always contain one of these special predicates which we used as examples, which cannot be introduced in an arbitrary manner but which must respect certain formal mathematic conditions which have been determined exactly in the general theory of measurement.

Now let us consider a predicate P which we assume to have succeeded in determining in a satisfactory manner for a certain group of objects using operational criteria. As an empirical predicate it is thus exactly defined and " scientifically " in order, but it is not yet said that it is measurable, or, in other words, that it can be converted into "magnitudes"; that can happen only if we can represent it in an homomorphic manner in an appropriate " scale " in relation to which it will become measurable. Since every scale implies, to begin with, a relation of identity and a relation of order, we must first examine the operational criteria in order to know when two objects are "identical" and when one "precedes" the other in relation to P. For example, if our predicate is that of "weight", we can compare the weight of two objects by placing them each one on the plates of a balance. We can then say that both objects have the same weight or that they are identical with respect to weight if the balance remains equipoised. Otherwise we can say that the object that goes up on the balance " precedes " the one that goes down, or that the weight of the first is less than the weight of the second. Analogously if our predicate is the "hardness" of solid bodies, we can agree to compare them on the basis of a scratch test and say that two objects have the same hardness if neither of the two can scratch the other, while the one that can be scratched by the other is said to be less hard.

After this we could attribute numbers to the different objects of our collection and arrange them in order giving the same number to those objects which have shown themselves to be identical in reference to our predicate and smaller numbers to those which precede the others in relation to this same predicate. Thus we would have placed our objects in an ordinal scale and we would have obtained a very elementary type of measurement. The reason for this elementarity is that these numbers are purely *ordinal* and that consequently they express absolutely no intrinsic property of our objects, but only a possiblity of *comparison* among them which cannot be considered as truly "quantitative". To arrive at this latter, we must determine if our predicates possess further characteristics which permit the possibility of submitting them to operational conditions which, from a formal point of view, become isomorphic to the conditions imposed on real numbers in order to obtain one or the other of the richer "scales." (One of these conditions which frequently proves indispensable is that of "additivity" for example.) However, this further movement is sometimes possible (as in the case of "weight" for example) and other times it cannot be verified (as in the case of "hardness" for example). The general theory of measurement has studied at length this type of question and has established a long series of conditions, frequently rather complex, which must be satisfied by the predicates of a group of objects in order that they can become quantities and magnitudes. We shall not take the time to examine these conditions which are of a rather complex technical and formal nature. Here it suffices to say that a large part of the predicates of empirical sciences are not able to satisfy these conditions and hence cannot become magnitudes.

But it is possible to give an idea of the necessary process to which predicates must be subjected in order to convert them into measurable quantities even without entering into technical details. Let us consider, for example, the following two propositions: "object X is twice as hot as object Y"; and "object X is twice as heavy as object Y." Their similar linguistic form gives us the impression that they contain two magnitudes determined in a perfectly quantitative fashion which allows us to say that in both cases the measurement of these magnitudes reveals a value for X which is the double of the value of Y.

However, there is a profound difference in the two expressions, for the first (stated thus) has practically no meaning while the second has a carefully defined meaning. The reason is that, while we can explain the idea of "hotter" by means of the concept of temperature, this only gives us values relative to the scale of measurement employed, and the relations between these values do not remain constant if we pass from one scale to another. For example if the temperature of X is 10 degrees

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centigrade and that of Y is 20 degrees centigrade, one is the double of the other. But if these same temperatures are expressed in Fahrenheit, the values are of 50 and 68 respectively, and the relationship of double no longer obtains. For weight, on the other hand, we can use whatever system of measure (kilograms, pounds, etc.) and will obtain different values for X and Y, but that of X will always be the double of that of Y.

The reasons for this difference are not at all intuitive, and we can extricate them only through a semantic and operational analysis of the procedures which have permitted a definition of the concepts of temperature and weight. Only this analysis reveals the structural characteristics of these predicates and leads us to recognize the differences of a formal order which oblige us to represent them by means of numerical structures which are not isomorphic among themselves.

The point of these two examples is that just because a given empirical predicate is associated with good operational control criteria, it is not necessarily true that this automatically establishes the basis on which to construct magnitudes, even if the predicate can be " clothed " in some way in numbers. Without a sufficient and appropriate analysis, the risk is present instead that our efforts at quantification and measure will terminate in veritable nonsense.

This consideration is, of course, of primary importance in the very frequent cases of " qualitative " and " intuitive " concepts employed in the human sciences for which we are often anxious to find a quantitative formulation. To do this correctly we first must not presuppose that such a formulation " must " exist. Next we should attempt a logical (or rather semantic) analysis of the meaning of the concepts which generally in their intuitive formulation include a plurality of distinct meanings which are not all of the same importance. Each of these meanings can lead to operational criteria of identification and comparison which are normally quite different. Consequently if we decide to rely on one or another of these criteria for our judgments concerning the concept, we automatically favor only one of the aspects included in the original concept and we neglect the other aspects. The risk of forgetting the totality becomes all the greater if one of the semantic components is reducible to measurement and metrification. Then the temptation is almost irresistible to promote this aspect to the rank of "authentic" or at least "scientific" formulation of the original concept, consequently holding this latter to be vague and imprecise and thinking that the other semantic components are only accessory, intuitive, negligible and perhaps even disruptive connotations.

To show how the movement from the intuitive level to a semantic analysis and finally to an operation can lead to significant divergences we need only examine a very simple example of a physical nature. Let us take the notion of " density " as applied to liquids. From an intuitive point of view, density evokes the idea of a certain viscosity, an opacity, a kind of unctuousness, etc. Consequently if we are asked if olive oil is more or less dense than water, we tend to answer immediately that the oil is the denser. Nevertheless, if we pour olive oil into water we see that the oil stays on the surface which is considered evidence that it is actually less dense than water. Our sensory intuitions were thus misleading.

Actually it is another matter altogether. More exactly we have chosen to employ as operational criterion of comparison for density the mixture of liquids which, in turn, is appropriate for the evaluation of density according to only one of its possible meanings, as the relation between mass and volume of a body. It is true that this definition has become " standard " in physics; but we could also use other physical criteria for comparing the density of liquids. For example we could determine that, of two liquids, that one is denser which effects a stronger refraction of a ray of monochromatic light. Using this criterion we see generally that it harmonizes quite well with the results of the preceding criterion, but that there are also exceptions. There are liquids which have a stronger power of refraction than that of other liquids, but which are less " dense " according to the preceding criterion (e.g. water and alcohol).

In conclusion not only is there frequently a difference between the results of an intuitive judgment and those of an operational procedure but also between the results of diverse but equally "scientific" operational procedures. This is due to the fact that each operational procedure derives necessarily from the choice of a particular aspect of the concept under consideration and omits other aspects. In physics the unconsidered aspects were generally taken into account by the adoption of a certain criterion. In the case of our example, the intuitive notion of density has been restricted to the idea of mass per volume, but several other aspects of the intuitive notion have been " rescued " through other technical concepts such as viscosity, opacity, etc.

The danger is that in the more recent sciences one might limit the restrictive operation by favoring a characteristic which is more easily manipulable while at the same time not bothering to "rescue" the other characteristics, either because these did not become evident due to an inadequate semantic analysis, or because it has not yet been possible to attach them to valid operational criteria, or finally because they cannot be converted to measurable quantities.

Up until now we have been referring only to the case of predicates which can be subjected to " direct " measuring processes, i.e. directly associated with the use of some operational manoeuvre. However, we know that in the sciences we frequently have recourse to " indirect " measures. In this case certain magnitudes are measured directly; then from these is derived the value of a new magnitude interesting us because of the existence of some " empirical law " which ties it to directly measurable magnitudes on the one hand, and, on the other, which permits the preservation of the relation of order and other structural properties which should characterize a magnitude. However, it is clear that a certain " quality " or intuitive property can well seem connected to several other qualities and even to several other magnitudes on the basis of different "empirical laws". This implies in general that the quality will behave differently in different situations which determine a relation of order and other structural properties applicable. This means that if we have a certain intuitive quality (the notion of intellect in psychology or development in economy) and if we wish to "measure" it in order to compare its presence in two subjects X and Y, it can happen that we say that X possesses the quality in a greater measure than Y, according to a certain criterion of measurement or that the opposite is true according to another criterion of measurement. This indicates that the logical, seman-

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tic, and empirical networks which tie all these notions together among themselves play a very delicate role and that frequently the fact of selecting one of these networks simply because it allows us to arrive at an indirect "measurement" of the property can lead to equivocal and obscure concepts rather than to precise ones.

It is not a question of polemics. Quantity and measure are of such evident and accepted utility that it is not necessary to come to their defense. Instead it is important to note that we do not have to force the creation of quantity and measurement at all costs, and it should not cause an inferiority complex if measures are not produced. For measure, as we have attempted to demonstrate in the proceding pages, is not always and automatically the condition necessary for attaining strict objectivity and precision in the sciences.

Thus it is more fruitful to attempt an amelioration of our scientific standards on the basis of methodological and formal qualifications which are not dominated by the myth of measure.

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