MEASUREMENT AND THE MATHEMATICAL ROLE OF SCIENTIFIC MAGNITUDES

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There are two main currents in the philosophy of measurement. One of them is the well known 'operational approach' developed by Bridgman and Dingle[1]. The other view has received no definite name.[2] It will be called here 'the mathematical approach to measurement', since it stresses the relation between measurement procedures and mathematical operations. This approach has been developed, with some variations, by Helmholtz, Campbell, Hempel, Ellis and other authors.[3]

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[1] P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927); P. W. Bridgman, *The Nature of Physical Theory* (Princeton: Princeton University, 1936); P. W. Bridgman, *The Nature of Some of Our Physical Concepts* (New York: Philosophical Library, 1952); H. Dingle, 'Science and the unobservable', *Nature* 166 (1950), pp. 21-28; H. Dingle, 'A theory of measurement', *British Journal for the Philosophy of Science* 1 (1950), pp. 5-26.

[2] It has been called 'the classical view of measurement' by Stevens, in: S. S. Stevens, 'Measurement, psychophysics, and utility', in *Measurement: Definitions and Theories*, edited by C. W. Churchman and P. Ratoosh (New York: Wiley, 1959), pp. 18-63. But this approach has also received other names. It has been called 'the representational theory of measurement' by Adams. See: E. W. Adams, 'On the nature and purpose of measurement', *Synthese* 16 (1966), pp. 125-169.

[3] H. von Helmholtz, *Counting and Measuring* (New York: Van Nostrand, 1930); N. R. Campbell, *Foundations of Science* (New York: Dover, 1957); N. R. Campbell, *An Account of the Principles of Measurement and Calculation* (London: Longmans, Green, 1928); C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago, 1952); B. Ellis, *Basic Concepts of Measurement* (Cambridge: Cambridge University, 1968).

The operational approach became popular among physicists and other scientists, but has never been very popular among philosophers, perhaps because it does not properly provide a *theory* of measurement, as Bridgman himself was aware of.[4] Since the specification of a measurement procedure is, according to operationalism, conventional and nearly blind, this view cannot account for the mathematical structure of science, as it will be seen below.

The mathematical approach to measurement has received a greater attention and acceptance from philosophers. Nevertheless, some of its basic assumptions have received a few criticisms that have not been answered. In this paper we shall study some of the features of this approach, endeavouring to show its value, and answering to some objections. We shall focus our attention upon three aspects:

(i) The mathematical approach to measurement should be regarded as a set of 'desiderata' which are useful whenever fulfilled, but that do not forbid any kind of measurement procedure;

(ii) The mathematical approach recommends that empirical operations isomorphic to arithmetical operations should be searched for; and only when this condition is satisfied can we understand the application of mathematics to science, and only in this case there may arise something which could be called 'scientific intuition' of the phenomena;

(iii) Whenever the magnitudes studied in some field are measurable by procedures that obey the 'desiderata' of the mathematical approach, it is likely that mathematically simple quantitative laws will be found to hold between these magnitudes.

I

Let us first study the methodological view used in this paper.[5] Any study of measurement procedures – indeed, the same holds for any epistemological study – may use one or several of the following approaches:

(a) analytical or formal study – consisting in the formulation of analytical sentences about measurement, such as tautologies, definitions, classifications, etc., which can neither be submitted to empirical tests concerning their truth-value, nor have an axiologic value.

(b) empirical study – consisting in the formulation of descriptions

[4] P. W. Bridgman, 'Operational analysis', *Philosophy of Science* 5 (1938), pp. 114-131; P. W. Bridgman, 'Remarks on the present state of operationalism', *Scientific Monthly* 79 (1954), pp. 224-226.

[5] The general principles used in this paper have been discussed in: R. de A. Martins, 'Abordagem axiológica da epistemologia científica', *Textos Seaf* 2 (1980), pp. 38-57; R. de A. Martins, "A situação epistemológica da epistemologia", *Revista de Ciências Humanas* (UFSC) 3 (5) (1984), pp. 85-110.

and general laws about measurement procedures employed by scientists, producing statements that can be tested by being confronted to the historical or actual practice of scientists.

(c) axiological study – consisting in the proposal of norms and/or 'desiderata' about measurement, which cannot undergo empirical tests, but have an axiologic value, being different from neutral analytic sentences.

Most or all of the papers on the philosophy of measurement include a combination of the three approaches. The ultimate aim of a measurement theory seems to be axiologic: to suggest rules to be followed in measurement, or to discuss the value of some procedures.

But in order to reach this objective, some analytic study (proposing concepts, classifications, making inferences from them, etc), and empirical data or generalizations (what the scientists have usually done in the past, or what they usually do now, etc) are used.

An objective and clear discussion of analytic and empirical studies is possible, and it seems possible and easy to reach an agreement concerning these matters. But it becomes very difficult to reach an agreement when axiologic problems are at stake. The difficulty here is the same that always occurs within ethics: the question cannot be settled by simple convention, because it is not a neutral question, and people differ about the solutions; and recourse to empirical data is useless, since an 'ought' cannot be derived from an 'is'.[6]

Although no method has been discovered for reducing axiological discussions to scientific discussions, a kind of policy can be used that may have success in eliminating – or, at least, reducing – axiological controversies. It is very difficult to make rigid distinctions about what is a 'good' or a 'bad' procedure; and borderline situations are specially difficult; but it is not so difficult to reach an agreement about the general lines which should direct valuable procedures, and often an agreement can be reached, in practice, about the 'desiderata' that should be pursued. Let us elucidate this point.

Whenever we propose axiological norms for the construction of measurement procedures, we may either state rigid prohibitions and demands (strict requirements) that must always be obeyed, or we may state 'desiderata' – a set of conditions that we should endeavour to satisfy, and that enhance the value of the measurement process when they are fulfilled. It is very difficult, perhaps impossible, to reach an accord about

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[6] Although within the tradition of logical positivism it was generally accepted that propositions about value cannot be derived from matter-of-fact knowledge, this point has been submitted to discussion in recent times. See: Kai Nielsen, 'On deriving an ought from an is: a retrospective look', *Review of Metaphysics* 32 (1979), pp. 487-514. But the basic idea behind the distinction still seems to hold.

strict requirements in any field, and measurement theory is not an exception. What makes this unanimity so difficult?

At least in part, the reason is this: when we evaluate a scientific procedure, we are by extension also evaluating scientists. If we criticize or try to prohibit a kind of procedure that is used by some scientists, they will feel that their status is being attacked, and they will fight to defend their procedures. I propose the following empirical generalization as true: "If someone proposes that any scientific measurement process *must obey* conditions (a), (b), ... , (n) – conditions that include both prohibitions and requirements; and if there are procedures that do not obey all of the conditions (a), (b), ... , (n); and if these procedures are used and called 'measurement' by people who call themselves 'scientists'; then a reaction from this group can be anticipated, and they will not agree with the proposed norms; and the reaction will be stronger the more committed to their procedures they are, and even stronger when they are unable or seldom able to fulfill the conditions (a), (b), ... , (n)". A psychological explanation of this reaction can be provided. I think that everybody knows some instances of this kind of reaction, either inside or outside science.

Even in seemingly neutral analytical studies, the same kind of problem may arise. If we are trying to *define* 'scientific measurement', and if 'scientific' is an adjective that carries with it an implicit worth (as it usually does, at least for scientists), then the definition which seems to be neutral is, at the bottom, an axiologically valued proposition. The same difficulty in reaching a consensus will be observed here. Many of the controversies within measurement theory may be explained by the occurrence of this kind of situation.

Let us take, as an instance, a very strange question that has been discussed some time ago: is every measurement a production of numerical data, or is mere classification also to be included within measurement?[7]

When talking about measurement, physicists usually think about numerical data; and since we have two different words – 'to measure' and 'to classify' – why should we not use both, with different meanings? Why collapse two different things into a single word? With these and similar arguments, I think that physicists would reach the conclusion: all measurement is the production of numerical data.

But among psychologists the reaction would be different. Psychologists

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[7] Adams has openly defended the view that measurement need not be the assignment of numbers: E. W. Adams, *op. cit.* p. 129. But even the inclusion of 'nominal scales' among the scales of measurement, advocated by Stevens, is, after all, the inclusion of classification within measurement: S. S. Stevens, 'On the theory of scales of measurement', *Science* 103 (1946), pp. 677-680. See also: W. W. Rozeboom, 'Scaling theory and the nature of measurement', *Synthese* 16 (1966), pp. 170-233.

employ several tests that do not lead to quantitative data, but provide a classification of the tested people; nevertheless, they have chosen to call their testing material 'measuring instrument', and their test procedures 'measurement'. If in the definition of 'measurement' we include the condition that it must be a process that leads to numerical data, then psychologists will feel that their work is being cast to a lower status, and will react, as they have reacted in this very case.[8]

The trouble here is the valorization of the expression 'measurement'. The scientific community has introjected the famous dictum ascribed to Lord Kelvin and inscribed at the façade of the Social Science Research Building at the University of Chicago: 'If you cannot measure, your knowledge is meager and unsatisfactory.'[9] The word 'to classify' does not carry the same psychological impact or value associated with the word 'to measure', and hence psychologists (and other scientists as well) prefer to call their tests 'measurements'. Now, this is a 'human, very human' attitude, but a silly one. Some people that defend the idea that any classification is a kind of measurement remind me of the story of the mouse that proposed that all mice should cut their tails; it had very good arguments, but once the other mice discovered that the real motivation of that mouse was that it had lost his tail in a mousetrap, all its arguments fell to the ground.

It seems to me that we should distinguish between (numerical) measurement and (qualitative) classification procedures. If we choose to include everything under the same name of 'measurement', we must distinguish between 'qualitative measurement' and 'numerical measurement', and it will also be necessary to state that numerical measurement has a greater scientific value than qualitative measurement. Why? Because everything that can be done with qualitative data can also be done with numerical data, but the converse is not true. Numerical data are much more powerful and useful than qualitative data, and therefore have a greater value.[10]

Once the psychological motivation behind the epistemological discussion is understood, we may also find a way out of this discussion. We do not need to make those people, who cannot produce numerical data, unhappy. Let us tell them: "All right, you may use the name 'measurement' for your classification procedures, if you choose to do so; we do not impose strict requirements or prohibitions upon this concept. But the relevant matter is this: it is desirable to develop procedures that yield numerical

[8] Not every psychologist reacts in this way, of course.

[9] Kuhn has not been able to find this sentence in Kelvin's writings; but other similar sentences occur in his works. See T. S. Kuhn, 'The function of measurement in modern physical science', *Isis* 52 (1961), pp. 161-193.

[10] See Rozeboom (*op. cit.*) and Hempel, *op. cit.* pp. 54-57.

data; and whenever a procedure is created or transformed so as to produce this kind of data, this transformation is to be hailed as an improvement of the procedure". Saying this, we state that the production of numerical data is a 'desideratum' of scientific measurement, but we do not forbid anything. It seems to me that it is easier to reach an agreement about this kind of proposal than about prohibitions or strict requirements. If agreement is desirable, then I think that the policy of proposing 'desiderata' is a useful one.

I propose that every axiological problem within measurement theory be answered by the choice of a 'desideratum'. Let us propose neither prohibitions nor strict requirements; let us propose neither definitions nor classifications that carry with them a valorization of their classes and imply that the practice of some scientists has no value. Instead, whenever value decisions are implicitly or explicitly at issue, let us propose a 'desideratum' — an ideal to be pursued, but which does not prohibit or take away the value of anything. In this way, we may hope to reach an agreement about the discussed questions.

I do not know whether the proponents of the mathematical approach to measurement have understood their theory as a set of restrictive norms, or as a set of 'desiderata'. But sometimes the approach has been interpreted as restrictive.[11] In order that it may be accepted by everyone, I propose that it should be interpreted as an ideal to be pursued, imposing no limitation upon measurement procedures.

II

Within physics, the most important scientific laws are clothed in a mathematical formalism. Are mathematical laws[12] desirable? and what conditions allow us to build within science its quantitative laws?

I think that every scientist agrees that it is useful to find quantitative scientific laws. I do not suggest that science should be restricted to mathematical laws; I am just stating that this kind of law has a great scientific value. Quantitative laws are better than qualitative or semi--quantitative laws,[13] since everything that can be done with qualitative laws can also be done with quantitative laws, and the converse is not true.[14] But

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[11] R. D. Luce, J. W. Tukey, 'Simultaneous conjoint measurement: a new type of fundamental measurement', *Journal of Mathematical Psychology* 1 (1964), pp. 1-27, at p. 2: 'For four decades this emphasis has had a profound influence. In many respects this has been healthy, but in others it may have been unduly restrictive'.

[12] I here mean those laws that are expressed by algebraic functions; even qualitative relations, of course, might be represented by the symbolism of set theory, and could be called mathematical laws.

[13] Semi-quantitative laws are those that express relations between orders: 'the gravitational force between two bodies decreases when their distance increases.' This sentence is not equivalent to the quantitative law of gravitation.

[14] We implicitly assume that if A is more powerful than B, then A is more desirable than B. Perhaps this may be denied.

qualitative laws are not to be forbidden in science; specially when a quantitative law is lacking, the corresponding qualitative law is indeed very useful. The formulation of mathematical or quantitative scientific laws is a 'desideratum', but it is not a strict requirement or imposition.

Quantitative scientific laws require the arithmetical manipulation of scientific magnitudes. Within quantitative laws, magnitudes are compared or equalized, and they are added, multiplied, divided, and so on. What is the meaning of those operations, and when are they justified?

There are cases where the meaning of these operations seems plain and non-controversial. We know what it means to say that the weight of a body is twice that of another; we know what it means to add two lengths, or to find the mean value of several measures of a period of time. But it is very difficult to give any reasonable meaning to statements such as '*A* has twice the intelligence of *B*', or 'since John has got the scores 5, 6, and 10, his final grade is 7'. We understand the meaning of arithmetical operations applied to some magnitudes, but not to others.

I shall not repeat everything that has already been said about this subject, nor repeat the eternal comparison between mass and hardness measurements.[15] Nagel,[16] for instance, has discussed the importance of the correspondence between empirical operations and arithmetical operations:

> If the search for mathematical equations is the aim of physics, other activities such as experiment, classification, or measurement are subservient to this aim, and are to be understood only in relation to it. Consequently, if we inquire why we measure in physics, the answer will be that if we do measure, and measure in certain ways, then it will be possible to establish the equations and theories which are the goal of inquiry. (p. 314)

It seems that Nagel gives a scientific value only to mathematical laws and to those activities that lead to them. This is too restrictive, and is in disaccord with the policy used in this paper. But I think that he has shown why the 'desiderata' proposed by the mathematical approach to measurement for fundamental measurement[17] are important. I think that Adams has misunderstood this point. He states that the proponents of this

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[15] See, for instance: J. J. D. Smart, 'Measurement', *Australasian Journal of Philosophy* 37 (1959), pp. 1-13.

[16] E. Nagel, 'Measurement', *Erkenntnis* 2 (1931), pp. 313-333.

[17] For the distinction between 'fundamental' and 'derived' measurement, see the works cited in footnote 3.

view of measurement do not present any argument that measurement systems should satisfy the basic assumptions of the mathematical approach.[18] But the paper by Nagel cited above, and that is also cited by Adams himself, does certainly present arguments for the requirements of the mathematical approach. We may question the *validity* of these arguments, but not their *existence*. Adams also criticizes the mathematical approach saying that although its rules could be justified by showing that some objectives are gained when the rules are followed, yet the same objectives may be gained using other measurement procedures.[19] This may be true, but he did not show it. Given that the 'desiderata' of the mathematical approach are instrumental in producing quantitative scientific laws, Adams should have proved that this same objective could be reached by other means. But he only shows that there are many things (mainly practical applications) that may be done without quantitative laws, and that in those cases we do not need a quantitative measurement that follows the rules of the mathematical approach. He did not propose another theory that has the same power as the mathematical approach.

Much of Adams' criticism only shows that the rules of the mathematical approach should not be regarded as restrictive – and this agrees with the view of this paper. He also presents some weaknesses of the usual formulation of the mathematical approach, such as the problem of the constancy of objects in measurement; I have elsewhere discussed this aspect of the theories of measurement.[20]

It seems that Adams' criticism has also been a reaction against the mathematical approach viewed as a restrictive theory that forbids psychological procedures. Let us analyze this point.

Consider the evolution of temperature measurement within physics. When temperature was measured only by liquid-indicator thermometers, the theory of thermal phenomena could not advance much. Mercury thermometers did provide precise, reproducible numerical data; but what was the meaning of these numbers? What meaning could be given to equal differences of temperature? What meaning could be given to a temperature being twice as high as another? Now, compare this with the modern thermodynamic scale of temperature. Everything has changed, and

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18 Adams, *op. cit.* p. 129.

19 '... I will argue that, given certain objectives to be gained by measurement, there is no reason why the five assumptions should be satisfied: these objectives *may* be gained using procedures of measurement conforming to the representational conception of measurement, but they may be gained as well in other ways too.' Adams, *op. cit.* p. 129.

20 R. de A. Martins, 'Experimental tests of isometry hypotheses', *British Journal for the Philosophy of Science* 33 (1982), pp. 296-304.

we can understand what a temperature ratio means.

Nowadays, IQ tests provide numerical data that are at their best equivalent to the old indications of mercury thermometers. Unless we gain some knowledge about the empirical or deeper meaning of IQ addition, IQ differences, or IQ ratios, the theory of intelligence cannot go very far, and we will not be justified in building quantitative laws where IQ enters as a parameter submitted to arithmetical operations.

IQ measurements – if measurements they are – do not fulfill the 'desiderata' of the mathematical approach to measurement. This does not forbid IQ tests, but shows in which direction we should strive to improve these tests, in order that a quantitative theory of intelligence may be elaborated.

But if we interpret the mathematical approach as a prohibition of any fundamental measurement that does not satisfy its requirements, then Adams is justified in attacking it, because it would forbid a useful practice within psychology.

Some time ago, it seemed that it would be forever impossible within psychology to create an empirical analogue of arithmetical properties. If this were true, then psychology would forever have great theoretical limitations. But it has been found that several hypotheses about the arithmetical properties of psychological causes may receive a clear and simple empirical identification, and the procedure of 'conjoint measurement' has broken the old mathematical limitation of psychology.[21] It is not difficult to foresee a very fruiful development of mathematical laws within theoretical psychology that is bound to arise from this recent break-through. Now psychologists will have more reasons to study and defend the mathematical approach, than to attack it.

But there is also another aspect of the importance of finding empirical equivalents to arithmetical operations. It is the possibility of development of an insight of the inner working of phenomena – something that might be called 'scientific intuition'.

Let us first describe what we mean by 'scientific intuition'. We sometimes find in the metascientific literature (specially in biographical literature) statements such as: '*X* was not guided by the mathematical formalism; he had a physical insight of the whole situation.'[22] Although

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[21] See: N. H. Anderson, 'Algebraic rules in psychological measurement', *American Scientist* 67 (1979), pp. 555-563, and references therein.

[22] Recalling his first conversation with Niels Bohr, in 1922, Heisenberg stated: '... his knowledge of the interrelations did not derive from a mathematical knowledge of the basic suppositions, but from an intense study of phenomena, which made it easier for him to grasp the relations by intuition than to deduce them.' W. Heisenberg, *Schritte ueber Grenzen* (Muenchen: R. Piper, 1971); spanish translation: *Más Allá de la Física* (Madrid: Editorial Catolica, 1974), p. 46.

this non-mathematical insight may appear in different degrees, most physicists develop it to some extent. Physics students do usually become amazed at their teachers' ability in reaching quantitative conclusions about physical situations without doing a single explicit mathematical operation. At first inspection, it seems strange that results, that the students can only reach through the manipulation of formulae, can be found without the equations. The teacher has used his 'physical intuition'; but what is this? The name is misleading, since it seems to point to a strange and non-rational capacity of the physicist. What really happens is that this teacher knows the empirical counterparts of the mathematical operations, and he can operate with *images* instead of *equations*, and reach the same results because there is a correspondence between their properties. When someone is well acquainted with the empirical facts, it is easier (and safer) to operate with the images of the physical operations than with the isomorphous mathematical calculus. If there were no correspondence between empirical operations and mathematical operations, this 'scientific intuition' could not exist.

Thus, in the scientific fields where the 'desiderata' of the mathematical approach to measurement are satisfied, the scientist may obtain an insight into the studied phenomena, and this seems an additional advantage of this approach.

III

There is still another important aspect of science that arises whenever the requirements of the mathematical approach are fulfilled: we may find *simple* mathematical laws. We shall here refer to the simplicity of the algebraic formalism of quantitative laws; other aspects of the simplicity of scientific laws[23] will not be dealt with here.

It seems that scientists do attach a greater value to simple laws than to complex ones. Whenever a quantitative law is found, we are glad if it is a simple one, that is, with as few constants as possible, and with a simple functional structure. The finding of simple quantitative laws is a scientific 'desideratum' – but, let us repeat, complex laws are not forbidden.

Why are simple laws desirable? They give us a kind of aesthetic pleasure; they are easier to understand, to use and to recall; they seem

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[23] R. Rudner, 'An introduction to simplicity', *Philosophy of Science* 28 (1961), pp. 109-119; D. J. Hillman, 'The measurement of simplicity', *Philosophy of Science* 29 (1962), pp. 225-252; H. R. Post, *British Journal for the Philosophy of Science* 11 (1960), pp. 32-41; K. S. Friedman, 'Empirical simplicity as testability', *British Journal for the Philosophy of Science* 23 (1972), pp. 25-33.

'truer' or 'more fundamental' than complex laws; etc.[24] It is very difficult to say exactly what is really important in simple laws, but I think that there is a consensus about the desirability of simple laws, and I shall use this agreement without trying to account for it.

The existence of simple quantitative scientific laws seems to have a multifarious explanation.[25] Some simple laws, such as Hooke's law, are just first-order approximations, and their apparent simplicity is due to the use of a mathematical artifice. Other laws derive their simplicity from symmetry and homogeneity conditions imposed upon the subject. One instance is the law of decrease of light intensity with distance from a point light source in vacuum. But at least part of the simplicity of physical laws is due to the way physical magnitudes are conceived and measured. Ellis[26] has already discussed the relevance of measurement procedures for the simplicity of physical laws, but I think that something may be added to his considerations.

When we relate derived magnitudes[27] to fundamental magnitudes through simple mathematical relations, we are freely imposing some simplicity upon the theorems derived from these definitions. If, instead of defining density as $\rho = dm/dV$, a more complex relation was used, it is obvious that derived formulae would become more complex. But here the simplicity is introduced in an artificial and straightforward way. It is not so easy to see how the structure of measurement of fundamental magnitudes can account for the simplicity of the laws in which they occur.

In some fields of physics it is easier to see how relevant the structure of measurement is for the resultant scientific theory. In special relativity, physicists are greatly concerned with measurement processes, and are aware of their consequences for the theory. It is widely known that Einstein took a great care in describing the way of measuring the length of a moving body and the time when an event occurs, relatively to moving reference frames. It is also well-known that from this measurement specification, some definitions, and the postulates of relativity, the laws of space-time transformation (Lorentz transformations and derived laws)

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[24] For the aesthetic aspects in science, see: S. Chandrasekar, 'Beauty and the quest for beauty in science', *Physics Today* 32 (1979, july), pp. 25-31; M. Maruyama, 'Epistemologies and esthetic principles', *Cybernetica* 21 (1978), pp. 273-282.

[25] See A. Einstein, 'On the method of theoretical physics', *Philosophy of Science* 1 (1934), pp. 163-169; H. Margenau, 'Einstein's conception of reality', in *Albert Einstein: Philosopher-Scientist*, ed. by P. A. Schilpp (New York: Harper and Brothers, 1959), pp. 245-268.

[26] Ellis, *op. cit.* pp. 81-86.

[27] For the concept of derived measurement, see the works cited in footnote 3.

result. This means that some of the most important theorems of special relativity are straightforward consequences of the measurement procedures imposed upon some physical magnitudes.[28]

But let us remain within classical physics. The important question is: how does the measurement process chosen for the fundamental magnitudes of physics lead to simple quantitative laws?

The fundamental magnitudes of physics, which have a direct and independent measurement process, are extensive magnitudes.[29] Extensive magnitudes are those for which there is a physical process of concatenation with properties equivalent to those of mathematical addition: using two systems A and B, we may produce a third system C such that the value of the relevant magnitude in C is the sum of the values of that magnitude in A and B.

We add time intervals by making the end of the first interval coincide with the beginning of the second. We add masses by justaposing the two bodies. We add lengths of two sticks placing their extremities in a straight line and making the end of one stick coincide with the beginning of the other; and so on. These properties are not *derived* from a theory: they are imposed by us upon our concepts. Why have we imposed upon our measurements exactly these and not other different specifications? Ellis[30] has discussed this problem, and he has shown that if lengths were added by placing the sticks perpendicular to one another, the 'desiderata' of the mathematical approach would still be fulfilled; but this alternative is not used by us. Why do we use exactly the concatenation processes that we actually use?

This question has several answers. If we ask about the *conscious* motivation that historically produced this choice, I think that the answer is: none. If we ask why we accept this choice as natural and good, and preferable to other alternatives, I think that the answer is: mainly because of habit and social influence. But if we ask whether there is some rational scientific motivation that could justify this choice, and that could have acted as an unconscious historical motivation, I think that the answer is: this choice leads to simple quantitative laws among the fundamental magnitudes of physics. We choose the same concatenation process for

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[28] See: A. Einstein, 'Zur Elektrodynamik bewegter Koerper', *Annalen der Physik* 17 (1905), pp. 891-921; see also: R. de A. Martins, 'Use and violation of operationalism in relativity', *Manuscrito* 5 (1981), pp. 103-115; R. de A. Martins, 'Force measurement and force transformation in special relativity', *American Journal of Physics* 50 (1982), pp. 1008-1011, and references therein.

[29] For the concept of extensive magnitudes, see the works cited in footnote 3.

[30] B. Ellis, 'Some fundamental problems of direct measurement', *Australasian Journal of Philosophy* 38 (1960), pp. 37-47.

several magnitudes, or we choose concatenation processes that are subsets of other concatenation processes, and this produces the simplest kind of quantitative laws: direct proportionality among these magnitudes.

Let us consider an instance: for the set of systems usually called 'wires', the same physical process corresponds to length addition and to electrical resistance addition: we just join the extremities of the wires. It follows from this choice of the same concatenation process that, for any homogeneous wire, the electrical resistance is directly proportional to its length.

The concatenation process corresponding to mass addition is different from the process for length addition. To sum masses we just place the bodies together in any way. But the concatenation process for length addition is a subset of that for mass addition. Hence, whenever we add the lengths of two bodies, we also add their masses. Therefore, although the concatenation processes for mass and length are not the same, the mass of homogeneous wires must be directly proportional to their lengths.

It is this simple proportionality that allows us to define some simple derived magnitudes for homogeneous bodies. Both masses and volumes add by simple justaposition; hence, for homogeneous substances, the mass is proportional to the volume; therefore, for homogeneous substances, we can define the density as the ratio of mass to volume, because this ratio is a constant.

Every scalar that obeys a conservation law adds by simple justaposition: mass, energy, electrical charge, the volume of incompressible fluids, etc. Several magnitudes are added when the systems are joined in series: lengths, electrical resistance, electrical potential difference, temperature differences, etc. The existence of a small number of different concatenation procedures, and the simple relations between these concatenation procedures, lead to simple mathematical relations between the fundamental magnitudes associated to them.

I think that I have shown that the actual processes of concatenation that we use are instrumental in the production of simple mathematical laws between the fundamental magnitudes of physics. Since the production of simple scientific laws is a 'desideratum', then the use of the concatenation processes that we use must be considered very useful.

IV

The basic points of this paper may be summarized as follows: the main criticisms against the mathematical approach to measurement can be dismissed by emphasizing that its requirements are 'desiderata', not strict impositions or prohibitions. The importance of the mathematical approach to measurement can be shown by its consequences: it allows us to create mathematically simple quantitative laws, and to develop a scientific intuition concerning the relevant phenomena.

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ABSTRACT

The mathematical or theoretical approach to the theory of measurement (opposed to the operational approach) is usually accepted by philosophers, at least in its general lines. Some recent criticisms against this theory can be answered by qualifying the requirements of the theoretical approach as 'desiderata', not as strict impositions or prohibitions. Besides, it is shown that the use of this approach is instrumental in creating mathematically simple quantitative laws and allowing the development of a 'scientific intuition' concerning the relevant phenomena.

RESUMO

Os filósofos geralmente aceitam pelo menos as idéias gerais da abordagem matemática ou teórica da teoria da mensuração. Essa abordagem opõe-se ao operacionalismo. Algumas críticas recentes contra a abordagem teórica podem ser respondidas qualificando-se as normas que ela propõe como "desiderata", e não como imposições ou proibições estritas. Além disso, mostra-se que o uso dessa abordagem ajuda a criar leis quantitativas matematicamente simples, e permite o desenvolvimento de um tipo de "intuição científica" sobre os fenômenos relevantes.