Herrn Dr. Karl Polanyi
mit besten Grüßen
Felix Kaufmann

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will occur at the moment in which the supply of the product of the current productive series gives out.

Our theory calls for little modification. When marginal production is not forthwith abandoned, the reduction of supply will not occur before the end of the entire current productive series ; and in the case of capital type 3 *A b*, current production will not be abandoned unless the tax per unit is greater than the price per unit of the finished product.

26. Nor is it necessary to modify our theory substantially, if it is supposed that the different firms use capital goods with productive series of various lengths. The difference in length of the period must be due to the greater or less duration of the productive processes, or to the greater or smaller number of processes of which the series is composed. If the tax is greater than the marginal unit cost, all firms will find it profitable to abandon marginal production forthwith. The duration of the productive series is irrelevant for the duration of the shifting process, and the argument developed in paragraph 23 is fully applicable.

If the tax is less than costs, all the firms will prefer to complete the current process. If the goods produced in each process are all sold at the end of it, the shifting will occur at the end of the first process in that productive series of the firm with the shortest series, which follows the moment of impact.

If, however, the total supply must at every moment be considered as the sum of the supplies of a certain number of firms with different productive series, shifting will take place only when the supply of the marginal product of the firm with the longest productive series is exhausted.

Essentially the same is the case if firms which use productive series of equal duration begin them at different points of time.

MAURO FASIANI.

Genoa.

(Translated from the Italian by A. P. Lerner and Ursula K. Webb.)

The Concept of Law in Economic Science

THE question whether there exist laws of human behaviour of a rigidity comparable with that of the laws of Nature has played an important part in methodological controversies in the social sciences in general and in economics in particular. We can distinguish four main groups of theories. The first and the second group answer these questions in the affirmative, the third in the negative, and the fourth occupies an intermediate position.

I.—The first group, that of the behaviourists, maintains that it is wrong in principle to draw a sharp line of division between the method of the social sciences, or indeed of any "Geisteswissenschaften," on the one hand and the method of the natural sciences on the other, and that the methods developed in the natural sciences, and especially in physics, the queen of the natural sciences, are the only scientific methods. Thus the social scientists have the difficult but by no means hopeless task of systematically remodelling their sciences as natural sciences of human behaviour. This will involve incidentally the exposure and consequent elimination of a number of pseudo-scientific problems, but the positive content of the sciences will find expression in strict laws similar to those of the natural sciences.

As a justification for this point of view one is referred to the "inter-subjectivity," and in particular the communicability of knowledge as a necessary condition of its scientific character, coupled with the thesis that only objective experience, particularly of the kind utilised in physics, possesses this inter-subjectivity. But while the external world can be experienced by all men alike, no one has access to another person's mind. Hence introspection cannot be checked by experience of a similar kind, and it is, therefore, in accordance with the postulate of inter-subjectivity, unscientific. Indeed, the more radical of the behaviourists deny all meaning to a concept of the *alter ego* which contains other than "objective" elements, i.e. such as refer directly or indirectly to his body. They argue that the meaning of an empirical statement is nothing but the indication of the facts, the occurrence or non-occurrence of which decides whether the statement is to be considered true or false. But since we cannot see into the mind of the (hypothetical) other person, all data referring to him which we are able to gather, communicate or verify are connected directly or indirectly with his body; for which reason it is meaningless to see more in the *alter ego* than the complex of all such data belonging to the physical world.[1]

The path and goal of the social sciences are thus prescribed. Their point of departure must be the observation of human behaviour and of its biological and physiological determinants. Their aim must be, through statistical

[1] The refutation of this line of argument consists in the realisation that the experience of another person's ego is not exhausted by the complex of spatio-temporal data, but that these latter are inter-woven in a specific manner with elements of inner consciousness and experience of one's own physiological processes.

observation of *typical* human behaviour, to discover the natural laws of social activity, and this work must be associated with the results of those natural sciences which deal with the human body and above all the human brain. The earlier supporters of this naturalistic position regarded natural laws as exact and universally valid, this exactness being guaranteed in their opinion by the mathematical form in which they were expressed. Its more recent supporters, having a much better grasp of the significance and the limitations of mathematics, have indeed given up this idea of necessary law. Nevertheless, the method of abstract natural science still appears to them to be the only scientific method.

II.—A diametrically opposite view is taken by those thinkers who wish to establish the social sciences as independent generalising (nomothetic) sciences in their own right. Here also the idea of absolutely rigid laws often forms the guiding principle, but these laws are supposed to derive their validity from a source, different from that of the laws of Nature, namely from inner experience. If one attempts to subject the laws of the soul and of the mind to the categories and laws of natural science, one is condemned to futility from the outset. The "Geisteswissenschaftler" does not envy the natural scientist the exactness of his laws and his mathematical method. For the latter, consisting as it does in the measurement of phenomena, is applicable only to measureable, i.e. to spatial phenomena. Laws of this kind are fundamentally alien to the nature of Man. He can only register their validity without comprehending it; on the other hand, the inner experience of one's own thinking and feeling, and the "penetration" (*Einfühlung*) into the *alter ego* lead to laws whose validity we can comprehend from *ultimate* sources because these sources lie within ourselves. The conclusion is drawn that the methods of the Geisteswissenschaften must be rigidly distinguished from those of the natural sciences. Above all there must be a complete divorce between "*explanation*" in the natural sciences and "*interpretation*" (*Verstehen*) in the Geisteswissenschaften.

III.—In opposition to these two groups, which envisage the task of the social sciences as the discovery or construction of laws,[1] there stands a third group, represented mainly by historians or by thinkers influenced by them, who deny altogether the possibility of the existence of laws in the social sciences. In general, they argue that while the physical world must be considered as the sphere of rigid causality, the psycho-physical world, and therefore the social world, is the sphere of free will and therefore of indeterminacy. It is, indeed, possible by careful study of the historical process to discover rules or tendencies within the sphere of social history, but to regard these tendencies as universal laws is to misunderstand the essential nature of human behaviour.

This group concurs with the second group in rejecting the application to the social sciences of the methods and laws of the natural sciences, but differs from it in denying completely the possibility of a nomothetic social science.

IV.—Finally, the "south-west German school" of Neokantians (Windelband, Rickert, etc.) hold that while for the most part the natural sciences aim

[1] We cannot enter here into the disputed question whether or to what extent laws are "discovered" or "invented."

at "generalisation," and the Geisteswissenschaften[1] at "individualisation," there are nevertheless also generalising or "nomothetic" Geisteswissenschaften (e.g. Economics) and individualising or "idiographic" natural sciences (e.g. Geology).

The aim of the nomothetic sciences is to establish propositions which shall be as general as possible. This tendency finds its expression in the structure of their concepts and in their formulation of laws. The aim of the idiographic sciences, on the other hand, is the selection and presentation of the most significant facts, the degree of significance being determined by "Wertbeziehung" for example according to their importance for the political or cultural situation of to-day.

The doctrines outlined above are for the most part permeated with antiquated and erroneous concepts, both of the logical and mathematical laws and of the laws of the abstract natural sciences which appear either as the guiding principle for social sciences or as a contrasting principle. In particular misconceptions as to the nature of mathematical propositions and concepts have played a very large part in methodological discussion in the social sciences—particularly in Economics—as indeed in the development of philosophical thought generally.

The propositions of mathematics, with their precision and their apodictic validity, were regarded as providing a model for scientific laws of all kinds, for it was not realised that apodictical validity was incompatible with the nature of statements about facts. Until the influence of such misconceptions has been eradicated, the problems connected with the laws of social science cannot be stated clearly. It is impossible here to reproduce the somewhat complicated analysis which leads to the rejection of the older view: we must confine ourselves to a short statement of the more important conclusions.

1. No proposition in logic or in pure mathematics[2] tells one anything about reality; one can never learn from it whether a particular event is occurring, has occurred, or will occur at a definite time and place. The service of Logics and Mathematics is to translate implicit assumptions into explicit form. Logical and mathematical propositions are therefore analytical.

2. All the propositions of pure mathematics are implied in the constitutive principles of the series of integers. These principles are as follows: (1) There is a first integer. (2) Each integer has one and only one (immediately) following integer. (3) Each integer except the first, has one and only one immediately preceding integer. (4) If a proposition is true of the first integer and is of such a form that if it is true of any integer, it is also true of the following integer, then it is true of every integer. (Peano's Axioms.)

[1] With the natural sciences Rickert contrasts not "Geisteswissenschaften" but "Kulturwissenschaften."

[2] More subtle analysis shows that logical and mathematical propositions differ in structure. This leads to the distinction between logical propositions as tautologies and mathematical propositions as equations.—Wittgenstein, *Tractatus Logico-Philosophicus*, London, 1922, Kegan Paul.

In particular, therefore, there is no fundamental distinction to be drawn between elementary and higher mathematics such as is often thought to be marked by the infinitesimal calculus. Moreover, all propositions concerning negative, fractional, irrational and complex numbers are nothing but symbolic reformulations of propositions relating to integers (1, 2, 3......). Thus, e.g. the expression $a = \frac{2}{3} b$ is identical with the expression $3a = 2b$.

The calculus of probability also belongs to Pure Mathematics as a theory of Combinations. It too—contrary to the usual view—can make no statements about reality. In this connection the failure to distinguish the *law of large numbers* from the *Bernouilli-Poisson theorem* has led to much confusion.

Finally, as regards geometry, nobody doubts to-day that the so-called propositions of pure geometry are, in fact, not propositions at all, but " propositional functions " which become propositions only when data of experience are introduced in the place of the variables. Thus the meaning of the proposition " two straight lines determine a point " is, in pure geometry, that to $2x$ there belongs one y. But this propositional function with two variables only becomes a proposition—and therefore true or false—when data of experience are inserted for x and y.

A set of data which turns a system of propositional functions into true propositions is called a " model " of these propositional functions. It has been shown that the world of modern physics (i.e. of the general relativity theory) is not a model of Euclidean geometry, unless one " saves " it by the introduction of certain auxiliary physical hypotheses. On the other hand, " geometries," that is to say systems of certain types of propositional functions, can be discovered, with the help of which natural phenomena can be described without bringing in such auxiliary hypotheses. For these geometries, Euclid's " parallel postulate," according to which only one straight line drawn through a given point can be parallel to another given straight line, does not hold. It has, therefore, been demonstrated that the assumption that Euclidean geometry is an *a priori* constituent of physics cannot be maintained.

What is the position with regard to empirical laws? One must realise, first of all, that every law of this kind contains an anticipation, i.e. an assumption regarding some future occurrence. The simplest way of disentangling the essential problems connected with the concept of empirical Law is to distinguish between the three following questions:

1. Of what type are the assumptions which form the content of the law under consideration?
2. On what are these assumptions based?
3. What are the criteria for proving or refuting the assumptions?

I.—An empirical law does not make the assertion that something happens, but that it happens *under certain conditions*. As a first approximation we can formulate all such laws as follows: If events of the type $E_1, E_2 \ldots\ldots E_n$ occur, then events of the type $F_1, F_2 \ldots\ldots F_n$ will occur also. If the E events and the F events are simultaneous, we have laws of co-existence; if they are not simultaneous, we have laws of causation. Here it is necessary to bring in a further principle which has often been overlooked, and which we shall call the principle of *finite formulation*. It consists in the demand that a space-time or personal-time framework shall be given within which both the E and the F events occur. The statement that if the E events have occurred at some specific place and time, then the F events will occur at some place and time unspecified is of no scientific value—in particular because it would exclude the possibility of refutation. The framework may be drawn as wide as is required, but it must always have limits of some kind. We are referring essentially to our first point when we speak, on the one hand, of the purely hypothetical character of empirical laws, and on the other of their dependence on facts. No law can of itself afford any basis for prediction, since it contains merely a conditional assertion about the relation between possible data. It can, therefore, lead to predictions only when coupled with the establishment of facts—that is to say, of the results of observation.

This first point is often confused with points II and III to which we may now turn.

II.—*Genesis of the Laws*. By this must be understood not the description of all the psychological processes which lead to the formulation of a law, but the statement of those facts on which the law is based, or, in other words, from which it is inductively inferred. Before proceeding further we must make an observation of principle about the nature of induction. It is not permissible to contrast deduction with induction, as is often done, on the ground that the former is an inference from the universal to the particular, while the latter is an inference from the particular to the universal, since an inference from the particular to the universal is impossible. In so far as we can speak at all of inductive inferences, they are also inferences from the universal to the particular, but in this case the major premises are, as a rule, only partly conscious. Increasingly successful attempts have been made in recent years to bring out the implicit major premises on which the inductions of physics are based. But in the social sciences, where the inter-connections of phenomena are much more complicated, most of the work of this type still remains to be done. Here we can only indicate that such major premises incorporate certain definite general assumptions as to the uniformity of Nature. Thus the simplest case of an induction (induction by simple enumeration) would appear as the following syllogism. Major premise: if a series of events has repeatedly been followed in a definite spatial and temporal relation by another series of events, then the first series will always be followed in a similar way by the second. Minor premise: A series E_1 has repeatedly been followed by a series F_1. Conclusion: A series E_1 is always followed by a series F_1. (E_1 is the cause of F_1.)

The consideration that the law is "based" on facts reveals the second element of dependence of law on facts, which we have called the empirical Genesis of Law. This second principle, therefore, also asserts the presence of a hypothetical element. For the major premise—concerning the uniformity of natural occurrences in certain respects—involves an assumption which is simply assumed!

III.—The third element in the dependence of law on fact lies in the criteria of its validity, for an empirical law which has once been stated will not on that account be considered valid for ever. It must be continually retested by the facts. Whenever the conditioning events given in the law happen, the conditioned events must also happen if the validity of the previously established law is not to be called in question. Thus a law, even when formulated, remains a hypothesis, and the distinction between established theory and unproved hypothesis represents only a difference of degree, like the distinction between "rigid" laws and mere rules or tendencies.

Such differences of degree are, of course, of the greatest significance in scientific work. They provide a measure of the confidence we have in certain observations, which in themselves are often very indirect and assume the validity of a series of other laws. Thus the Michelson experiment, which led to results in contradiction with the well-established Maxwellian theory of light, was repeated for decades, since people were more inclined to believe that there was a flaw in the observations in question, although all possible precautions in experimentation had been taken, than to suspect that the Maxwellian theory was wrong.

Confidence in the validity of a law may sometimes be so great that any observation which does not agree with it is regarded as fallacious, or at least incomplete. In this case the possibility of refuting the law is suspended. If an observation is incompatible with the law and the correctness of the observation with regard to the relevant factors is above suspicion, additional factors such as disturbances, or changes in data, are assumed to account for the discrepancies. As a result, the following situation arises in which misunderstandings are liable to occur.

The given general proposition has the form of an empirical proposition, both because it asserts the interrelatedness of facts in the same way as a normal empirical law and also because when we examine its origin we find it to hav[illegible] been based on facts. At the same time, however, it appears to be irref[illegible] because any disagreement of the proposition with the facts is inter[illegible] consequence of incorrectness or incompleteness of observation. [illegible] the illusion that there are such things as absolutely valid empiri[illegible]

The situation is further confused in many cases by a chan[illegible] of the terms used. As a result the real origin of the "absolutel[illegible] obscured. Thus the measurement of temperature is based on [illegible]ce that the same circumstances which condition the different degre[illegible]tion of heat and cold, lead also to the expansion and contraction of solid bodies. Temperature is then defined as physical temperature, i.e. to begin with in terms of the expansion of thermometrical substances and later in terms of the mean velocity of the molecules of a gas. In this way the "proposition" that temperature is determined by these physical data becomes irrefutable, like any other definition. But this in itself says nothing as to how, and in what degree, propositions relating to physical temperature can be applied to physiological temperature. This is made quite clear by the following elementary example of different temperature-sensations. If we hold one hand in water of 20° Centigrade and the other in water of 40°, and then immerse both hands in water of 30° we experience different temperature-sensations.

From the above argument we can easily see the heuristic value and the dangers of such "irrefutable laws"—more appropriately called *conventions*, The assumption that certain laws based on definite experiences are universally valid or true rests ultimately upon the conviction of the uniformity and relatively simple nature of the world; a conviction which, more than any other in the field of scientific thought, has often been so magnificently justified. We need only mention Galileo's brilliant hypothesis that the motions of the heavenly bodies and the movements in his experimental laboratory obeyed the same laws, and his still more general assumption that all phenomena of the external world could be referred back to uniform principles. But the danger is that this kind of conviction may stiffen into dogmatism. All observations which might serve to modify the original formulation are either passed over altogether, or fail to receive adequate attention. In this way important data are neglected. The danger is enhanced by the fact that many metaphysicians find such general principles very fruitful soil for their speculations. Thus it may be said that a dogmatic attitude, whether it takes the form of explicit theory or of implicit assumption, can, in definite situations, provide a powerful impulse to scientific research by giving special emphasis to the conviction that a certain line of research is the only right one. On the other hand, in a different situation where a change in the methods of research is called for, the same attitude may constitute a serious hindrance to the progress of knowledge.

The "heuristic postulates" which tell us to apply a particular method are, in so far as they involve the thesis that it is the only right method, also conventions. These assertions, too, may be, and often are, supported by experience, but their conventional character rests on the fact that the possibility of their refutation by experience is suspended.

There is no difficulty in showing the relevance of this general examination [illegible] the nature of empirical and of mathematical laws for the methodology of [illegible] science. A few of the more important consequences may be mentioned: [illegible] points at issue between the historical and analytical schools in [illegible] much of their sharpness when it is realised that the alleged [illegible] in the abstract natural sciences (the application of which to [illegible] attacked by the former and defended by the latter) was falsely [illegible] both sides. Once this is recognised it becomes clear that the di[illegible]ween the two schools of thought consisted essentially in a disagr[illegible] concerning the level of abstraction to which research should be carried, the degree of generality the laws should possess, and the method and extent of verification by individual facts which the laws required. These are methodological questions of the highest importance, but they cannot be settled *a priori* by philosophical insight. A prerequisite to the clearing up of these questions is the realisation by the supporters of the historical school that every method of ascertaining and setting out historical facts always implies some general hypothesis concerning human behaviour.

2. In the same way the question of the application of the mathematical method to economic science, or the extent to which such application is justifiable, is also incapable of decision *a priori*. The claim of the mathematical method to supremacy rests upon the untenable conception of mathematical knowledge according to which it can of itself yield absolutely valid judgments about the physical or psycho-physical world. The diametrically opposed view

that the mathematical method is completely useless for economic science, and can therefore find no place in it, rests on a mistaken identification of the mathematical method with the method of measurement and on a misinterpretation of the part actually played by measurement in the natural sciences. It is asserted that psychological, and hence social, phenomena are not amenable to mathematical treatment because, not possessing spatial characteristics, they cannot be measured. The identification is incorrect, as a simple reference to mathematical studies such as topology (*analysis situs*) or projective geometry at once shows. Moreover, in the natural sciences themselves, as we saw in the case of temperature, the phenomena under consideration may not be measurable, but, nevertheless, the application of the method of measurement is made possible by the device of correlating with the intensive magnitudes extensive magnitudes which are empirically connected with them.

3. A grasp of the nature of empirical laws and of mathematical propositions also provides the right perspective for an appreciation of the relation between statistics and theory in economic science. It yields first the recognition that every attempt to use statistical data, in other words to infer laws from them, presupposes the existence of general laws, i.e. at least a rudimentary theory of some kind. It yields, secondly, the recognition that the mathematical method by itself can create no new objective knowledge. Without the help of general assumptions it is not possible to deduce from a number of isolated empirically given points—however great their number may be—uniquely determined or even finitely determined curves; mathematics cannot provide these assumptions.

4. Finally, the results of our analysis should save us from treating conventions in economic science as though they were absolutely valid laws [illegible] experience. Instances of such misinterpretation in economic science [illegible] means rare. I have analysed elsewhere[1] one of the most impor[illegible] fallacies, which relates to the principle of marginal utility.

But all these attempts to clarify the methodological pr[illegible] the first step towards a more systematic analysis, the aim [illegible] to reveal the significance of typical divergences between the l[illegible] natural sciences and those of economics, and also the part which [illegible]e, played by mathematics in both spheres. It is clear that the use [illegible]ics in physics is of the greatest scientific value. From what has been said above it is impossible to give any explanation of this, or to show whether, and to what extent, the same results should be obtainable in economics. To answer these questions we require a comparative analysis of induction as used in the abstract natural sciences on the one hand and in the social sciences and particularly in economics on the other. Such an analysis is still in its infancy. My article, "Was kann die mathematische Methode in der Nationalökonomie leisten?"[2], contains a few introductory reflections. I hope to carry the analysis further in my book, *Methodenlehre der Sozialwissenschaften*, which is to be published next year.

Vienna FELIX KAUFMANN

[1] "Logik und Wirtschaftswissenschaft," *Archiv für Sozialwissenschaft und Sozialpolitik*, Vol. LIV, pp. 614-56, 1925, p. 649 ff.; and "On the Subject-matter and Method of Economic Science, *Economica*, 1933, pp. 381-401.

[2] *Zeitschrift für Nationalökonomie*, 1931, pp. 754-79.

The Nature of Indifference Curves

1. The problem of individual demand for consumers' goods can be treated in a perfectly simple manner as long as the individual is concerned with only one good at a time. In this case, functions of one variable and curves in two dimensions are sufficient. But there is no indication, in this simple theory of choice, of any method of extension to more general and important cases where two or more consumers' goods enter together into the individual's calculations. In fact, the main difficulty encountered in the development of the theory of choice is to make the jump from one variable (one consumers' good) to two variables (two consumers' goods). Entirely different treatments are required in the two cases, but, once the theory has been established for two variables, the extension to the general case of any number of variables follows almost automatically. The only difficulty about this final extension lies in the fact that convenient geometrical representation becomes impossible, the number of physical spatial dimensions being limited. In this article, therefore, an attempt will be made to establish the theory of individual choice in the case of two variables, a case in which analysis can be supported and illustrated by quite simple geometrical representations.

The difficulty of making the jump from one to two variables is patent. If the total and marginal utility of an individual depend only on one consumers' good, then the amount of the good, and the expenditure at a given price, necessary to give a definite utility "level" follow at once ... the utility function. *There is no question of the balancing of* ... On the other hand, when at least two goods enter into the ... calculations, the problem of choice and of the balancing of ... ars at once. Various combinations of consumers' goods ... lity to the individual, a fact which leads at once to the ... ence curves. Consumers' goods can be substituted one ... variety of ways and the relationships between them, i.e. th... ntary or competitive nature, are of fundamental importance. It is the existence of mutual relationships between goods, finding expression in the various forms assumed by indifference curves, that distinguishes the general theory of choice from the simpler and more artificial theory which serves in the case of one consumers' good only.

2. For an individual considering various combinations of two consumers' goods X and Y, it is assumed that there exists a *utility function*, $u=\phi(x,y)$ which gives the individual's total utility (u) as dependent on the amounts (x and y) possessed of the two goods. The utility function is represented geometrically by a surface referred to three axes (OX, OY and OU) mutually at right angles—the *utility surface*. For convenience, the axis OU can be taken as vertical and the plane OXY as horizontal. It is then possible to refer to utility "heights" and "levels," and the utility of a combination consisting of amounts x and y of the two goods is represented by the height

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