Economics and Knowledge[1]

By F. A. von Hayek

I

The ambiguity of the title of this paper is not accidental. Its main subject is, of course, the rôle which assumptions and propositions about the knowledge possessed by the different members of society play in economic analysis. But this is by no means unconnected with the other question which might be discussed under the same title, the question to what extent formal economic analysis conveys any knowledge about what happens in the real world. Indeed my main contention will be that the tautologies, of which formal equilibrium analysis in economics essentially consists, can be turned into propositions which tell us anything about causation in the real world only in so far as we are able to fill those formal propositions with definite statements about how knowledge is acquired and communicated. In short I shall contend that the empirical element in economic theory—the only part which is concerned, not merely with implications but with causes and effects, and which leads therefore to conclusions which, at any rate in principle, are capable of verification[2]—consists of propositions about the acquisition of knowledge.

Perhaps I should begin by reminding you of the interesting fact that in quite a number of the more recent attempts made in different fields to push theoretical investigation beyond the limits of traditional equilibrium analysis, the answer has soon proved to turn on one question which, if not identical with mine, is at least part of it, namely the question of foresight. I think the field where, as one would expect, the discussion of the assumptions concerning foresight first attracted wider attention was the theory of risk.[3] The stimulus which was exercised in this connection

[1] Presidential Address to the London Economic Club, November 10th, 1936.

[2] Or rather falsification. Cf. K. Popper, *Logik der Forschung*, Vienna, 1935, *passim*.

[3] A more complete survey of the process by which the significance of anticipations was gradually introduced into economic analysis would probably have to begin with Professor Irving Fisher's *Appreciation and Interest* (1896).

by the work of Professor F. H. Knight may yet prove to have a profound influence far beyond its special field. Not much later the assumptions to be made concerning foresight proved to be of fundamental importance for the solution of the puzzles of the theory of imperfect competition, the questions of duopoly and oligopoly. And since then it has become more and more obvious that in the treatment of the more "dynamic" questions of money and industrial fluctuations the assumptions to be made about foresight and "anticipations" play an equally central rôle, and that in particular the concepts which were taken over into these fields from pure equilibrium analysis, like those of an equilibrium rate of interest, could be properly defined only in terms of assumptions concerning foresight. The situation seems here to be that before we can explain why people commit mistakes, we must first explain why they should ever be right.

In general it seems that we have come to a point where we all realise that the concept of equilibrium itself can be made definite and clear only in terms of assumptions concerning foresight, although we may not yet all agree what exactly these essential assumptions are. This question will occupy me later in this paper. At the moment I am only concerned to show that at the present juncture, whether we want to define the boundaries of economic statics or whether we want to go beyond it, we cannot escape the vexed problem of the exact position which assumptions about foresight are to have in our reasoning. Can this be merely an accident?

As I have already suggested, the reason for this seems to me to be that we have to deal here only with a special aspect of a much wider question which we ought to have faced at a much earlier point. Questions essentially similar to those mentioned arise in fact as soon as we try to apply the system of tautologies—those series of propositions which are necessarily true because they are merely transformations of the assumptions from which we start, and which constitute the main content of equilibrium analysis[1]—to the

[1] I should like to make it clear from the outset that I use the term "equilibrium analysis" throughout this paper in the narrower sense in which it is equivalent to what Professor Hans Mayer has christened the "functional" (as distinguished from the "causal-genetic") approach, and to what used to be loosely described as the "mathematical school". It is round this approach that most of the theoretical discussions of the past ten or fifteen years have taken place. It is true that Professor Mayer has held out before us the prospect of another, "causal-genetic" approach, but it can hardly be denied that this is still largely a promise. It should,

situation of a society consisting of several independent persons. I have long felt that the concept of equilibrium itself and the methods which we employ in pure analysis, have a clear meaning only when confined to the analysis of the action of a single person, and that we are really passing into a different sphere and silently introducing a new element of altogether different character when we apply it to the explanation of the interactions of a number of different individuals.

I am certain there are many who regard with impatience and distrust the whole tendency, which is inherent in all modern equilibrium analysis, to turn economics into a branch of pure logic, a set of self-evident propositions which, like mathematics or geometry, are subject to no other test but internal consistency. But it seems that if only this process is carried far enough it carries its own remedy with it. In distilling from our reasoning about the facts of economic life those parts which are truly *a priori*, we not only isolate one element of our reasoning as a sort of Pure Logic of Choice in all its purity, but we also isolate, and emphasise the importance of, another element which has been too much neglected. My criticism of the recent tendencies to make economic theory more and more formal is not that they have gone too far, but that they have not yet been carried far enough to complete the isolation of this branch of logic and to restore to its rightful place the investigation of causal processes, using formal economic theory as a tool in the same way as mathematics.

II

But before I can prove my contention that the tautological propositions of pure equilibrium analysis as such are not directly applicable to the explanation of social relations, I must first show that the concept of equilibrium *has* a clear meaning if applied to the actions of a single individual, and what this meaning is. Against my contention it might be argued that it is precisely here that the concept of equilibrium is of no significance, because, if one

however, be mentioned here that some of the most stimulating suggestions on problems closely related to those treated here have come from his circle. Cf., H. Mayer, "Der Erkenntniswert der funktionellen Preistheorien," *Die Wirtschaftstheorie der Gegenwart*, Vol. II, 1931; P. N. Rosenstein-Rodan, "Das Zeitmoment in der mathematischen Theorie des wirtschaftlichen Gleichgewichts," *Zeitschrift für Nationalökonomie*, Vol. I, No. 1, and "The Rôle of Time in Economic Theory," ECONOMICA (N.S.), Vol. I (1), 1934.

wanted to apply it, all one could say would be that an isolated person was always in equilibrium. But this last statement, although a truism, shows nothing but the way in which the concept of equilibrium is typically misused. What is relevant is not whether a person as such is or is not in equilibrium, but which of his actions stand in equilibrium relationships to each other. All propositions of equilibrium analysis, such as the proposition that relative values will correspond to relative costs, or that a person will equalise the marginal returns of any one factor in its different uses, are propositions about the relations between actions. Actions of a person can be said to be in equilibrium in so far as they can be understood as part of one plan. Only if this is the case, only if all these actions have been decided upon at one and the same moment, and in consideration of the same set of circumstances, have our statements about their interconnections, which we deduce from our assumptions about the knowledge and the preferences of the person, any application. It is important to remember that the so-called "data", from which we set out in this sort of analysis, are (apart from his tastes) all facts given to the person in question, the things as they are known to (or believed by) him to exist, and not in any sense objective facts. It is only because of this that the propositions we deduce are necessarily *a priori* valid, and that we preserve the consistency of the argument.[1]

The two main conclusions from these considerations are, *firstly*, that since equilibrium relations exist between the successive actions of a person only in so far as they are part of the execution of the same plan, any change in the relevant knowledge of the person, that is, any change which leads him to alter his plan, disrupts the equilibrium relation between his actions taken before and those taken after the change in his knowledge. In other words, the equilibrium relationship comprises only his actions during the period during which his anticipations prove correct. *Secondly*, that since equilibrium is a relationship between actions, and since the actions of one person must necessarily take place successively in time, it is obvious that the passage of time is essential to give the concept of equilibrium any meaning. This deserves mention since many economists

[1] Cf., on this point particularly L. Mises, *Grundprobleme der Nationalökonomie*, Jena, 1933, pp. 22 *et seq.*, 160 *et seq.*

appear to have been unable to find a place for time in equilibrium analysis and consequently have suggested that equilibrium must be conceived as timeless. This seems to me to be a meaningless statement.

III

Now, in spite of what I have said before about the doubtful meaning of equilibrium analysis in this sense if applied to the conditions of a competitive society, I do not of course want to deny that the concept was originally introduced precisely to describe the idea of some sort of balance between the actions of different individuals. All I have argued so far is that the sense in which we use the concept of equilibrium to describe the interdependence of the different actions of one person does not immediately admit of application to the relations between actions of different people. The question really is what use we make of it when we speak of equilibrium with reference to a competitive system.

The first answer which would seem to follow from our approach is that equilibrium in this connection exists if the actions of all members of the society over a period are all executions of their respective individual plans on which each decided at the beginning of the period. But when we inquire further what exactly this implies, it appears that this answer raises more difficulties than it solves. There is no special difficulty about the concept of an isolated person (or a group of persons directed by one of them) acting over a period according to a preconceived plan. In this case, the execution of the plan need not satisfy any special criteria in order to be conceivable. It may of course be based on wrong assumptions concerning the external facts and on this account may have to be changed. But there will always be a conceivable set of external events which would make it possible for the plan to be executed as originally conceived.

The situation is, however, different with the plans determined upon simultaneously but independently by a number of persons. In the first instance, in order that all these plans can be carried out, it is necessary for them to be based on the expectation of the same set of external events, since, if different people were to base their plans on conflicting expectations, no set of external events could make the execution of all these plans possible. And, secondly, in

a society based on exchange their plans will to a considerable extent refer to actions which require corresponding actions on the part of other individuals. This means that the plans of different individuals must in a special sense be compatible if it is to be even conceivable that they will be able to carry all of them out.[1] Or, to put the same thing in different words, since some of the "data" on which any one person will base his plans will be the expectation that other people will act in a particular way, it is essential for the compatibility of the different plans that the plans of the one contain exactly those actions which form the data for the plans of the other.

In the traditional treatment of equilibrium analysis part of this difficulty is apparently avoided by the assumption that the data, in the form of demand schedules representing individual tastes and technical facts, will be equally given to all individuals and that their acting on the same premises will somehow lead to their plans becoming adapted to each other. That this does not really overcome the difficulty created by the fact that one person's decisions are the other person's data, and that it involves to some degree circular reasoning, has often been pointed out. What, however, seems so far to have escaped notice is that this whole procedure involves a confusion of a much more general character, of which the point just mentioned is just a special instance, and which is due to an equivocation of the term "datum". The data which now are supposed to be objective facts and the same for all people are evidently no longer the same thing as the data which formed the starting point for the tautological transformations of the Pure Logic of Choice. There "data" meant all facts, and only the facts, which were present in the mind of the acting person, and only this subjective interpretation of the term datum made those propositions necessary truths. "Datum" meant given,

[1] It has long been a subject of wonder to me why there should, to my knowledge, have been no systematic attempts in sociology to analyse social relations in terms of correspondence and non-correspondence, or compatibility and non-compatibility, of individual aims and desires. It seems that the mathematical technique of *analysis situs* (topology) and particularly such concepts developed by it as that of *homeomorphism* might prove very useful in this connection, although it may appear doubtful whether even this technique, at any rate in the present state of its development, is adequate to the complexity of the structures with which we have to deal. A first attempt made recently in this direction by an eminent mathematician (Karl Menger, *Moral, Wille und Weltgestaltung*, Vienna, 1934) has so far not yet led to very illuminating results. But we may look forward with interest to the treatise on exact sociological theory which Professor Menger has promised for the near future. (Cf., "Einige neuere Fortschritte in der exakten Behandlung sozialwissenschaftlicher Probleme," in *Neuere Fortschritte in den exakten Wissenschaften*, Vienna, 1936, p. 132.)

known, to the person under consideration. But in the transition from the analysis of the action of an individual to the analysis of the situation in a society the concept has undergone an insidious change of meaning.

IV

The confusion about the concept of a datum is at the bottom of so many of our difficulties in this field that it is necessary to consider it in somewhat more detail. Datum means of course something given, but the question which is left open, and which in the social sciences is capable of two different answers, is to whom the facts are supposed to be given. Economists appear subconsciously always to have been somewhat uneasy about this point, and to have reassured themselves against the feeling that they did not quite know to whom the facts were given by underlining the fact that they *were* given — even by using such pleonastic expressions as "given data". But this does not solve the question whether the facts referred to are supposed to be given to the observing economist, or to the persons whose actions he wants to explain, and if to the latter, whether it is assumed that the same facts are known to all the different persons in the system, or whether the "data" for the different persons may be different.

There seems to be no possible doubt that these two concepts of "data", on the one hand in the sense of the objective real facts, as the observing economist is supposed to know them, and on the other in the subjective sense, as things known to the persons whose behaviour we try to explain, are really fundamentally different and ought to be kept carefully apart. And, as we shall see, the question why the data in the subjective sense of the term should ever come to correspond to the objective data is one of the main problems we have to answer.

The usefulness of the distinction becomes immediately apparent when we apply it to the question of what we can mean by the concept of a society being at any one moment in a state of equilibrium. There are evidently two senses in which it can be said that the subjective data, given to the different persons, and the individual plans, which necessarily follow from them, are in agreement. We may merely mean that these plans are mutually compatible and that there is consequently a conceivable set of external

events which will allow all people to carry out their plans and not cause any disappointments. If this mutual compatibility of intentions were not given, and if in consequence no set of external events could satisfy all expectations, we could clearly say that this is not a state of equilibrium. We have a situation where a revision of the plans on the part of at least some people is inevitable, or, to use a phrase which in the past has had a rather vague meaning, but which seems to fit this case perfectly, where endogenous disturbances are inevitable.

There is, however, still the other question of whether the individual subjective sets of data correspond to the objective data, and whether in consequence the expectations on which plans were based are borne out by the facts. If correspondence between data in this sense were required for equilibrium it would never be possible to decide otherwise than *ex post*, at the end of the period for which people have planned, whether at the beginning the society has been in equilibrium. It seems to be more in conformity with established usage to say in such a case that the equilibrium, as defined in the first sense, may be disturbed by an unforeseen development of the (objective) data, and to describe this as an exogenous disturbance. In fact it seems hardly possible to attach any definite meaning to the much used concept of a change in the (objective) data unless we distinguish between external developments in conformity with, and those different from, general expectations, and define as a "change" any divergence of the actual from the expected development, irrespective of whether it means a "change" in some absolute sense. Surely if the alternations of the seasons suddenly ceased and the weather remained constant from a certain day onward, this would represent a change of data in our sense, that is a change relative to expectations, although in an absolute sense it would not represent a change but rather an absence of change. But all this means that we can speak of a change in data only if equilibrium in the first sense exists, that is, if expectations coincide. If they conflicted, any development of the external facts might bear out somebody's expectations and disappoint those of others, and there would be no possibility of deciding what was a change in the objective data.[1]

[1] Cf. "The Maintenance of Capital," ECONOMICA (N.S.), Vol. II, 1935, p. 265.

V

For a society then we *can* speak of a *state* of equilibrium at a point of time—but it means only that compatibility exists between the different plans which the individuals composing it have made for action in time. And equilibrium will continue, once it exists, so long as the external data correspond to the common expectations of all the members of the society. The continuance of a state of equilibrium in this sense is then not dependent on the objective data being constant in an absolute sense, and is not necessarily confined to a stationary process. Equilibrium analysis becomes in principle applicable to a progressive society and to those inter-temporal price relationships which have given us so much trouble in recent times.[1]

These considerations seem to throw considerable light on the relationship between equilibrium and foresight, which has been somewhat hotly debated in recent times.[2] It appears that the concept of equilibrium merely means that the foresight of the different members of the society is in a special sense correct. It must be correct in the sense that every person's plan is based on the expectation of just those actions of other people which those other people intend to perform, and that all these plans are based on the expectation of the same set of external facts, so that under certain conditions nobody will have any reason to change his plans. Correct foresight is then not, as it has sometimes been understood, a precondition which must exist in order that equilibrium may be arrived at. It is rather

[1] This separation of the concept of equilibrium from that of a stationary state seems to me to be no more than the necessary outcome of a process which has been going on for a fairly long time. That this association of the two concepts is not essential but only due to historical reasons is to-day probably generally felt. If complete separation has not yet been effected, it is apparently only because no alternative definition of a state of equilibrium had yet been suggested which has made it possible to state in a general form those propositions of equilibrium analysis which are essentially independent of the concept of a stationary state. Yet it is evident that most of the propositions of equilibrium analysis are not supposed to be applicable only in that stationary state which will probably never be reached. The process of separation seems to have begun with Marshall and his distinction between long and short run equilibria. (Cf., statements like this: "For the nature of equilibrium itself, and that of the causes by which it is determined, depend on the length of the period over which the market is taken to extend." *Principles*, Vol. I, 6, 7th ed., p. 330.) The idea of a state of equilibrium which was not a stationary state was already inherent in my "Das intertemporale Gleichgewichtssystem der Preise und die Bewegungen des Geldwertes" (*Weltwirtschaftliches Archiv*, Vol. XXVIII, June, 1928) and is, of course, essential if we want to use the equilibrium apparatus for the explanation of any of the phenomena connected with "investment". On the whole matter much historical information will be found in E. Schams, Komparative Statistik, *Zeitschrift für Nationalökonomie* II/1, 1930.

[2] Cf. particularly O. Morgenstern, "Vollkommene Voraussicht und Wirtschaftliches Gleichgewicht," *Zeitschrift für Nationalökonomie*, Vol. VI, p. 3.

the defining characteristic of a state of equilibrium. Nor need foresight for this purpose be perfect in the sense that it need extend into the indefinite future, or that everybody must foresee everything correctly. We should rather say that equilibrium will last so long as the anticipations prove correct, and that they need to be correct only on those points which are relevant for the decisions of the individuals. But on this question of what is relevant foresight or knowledge, more later.

Before I proceed further I should probably stop for a moment to illustrate by a concrete example what I have just said about the meaning of a state of equilibrium and how it can be disturbed. Consider the preparations which will be going on at any moment for the production of houses. Brickmakers, plumbers and others will all be producing materials which in each case will correspond to a certain quantity of houses for which just this quantity of the particular material will be required. Similarly we may conceive of prospective buyers as accumulating savings which will enable them at certain dates to buy definite quantities of houses. If all these activities represent preparations for the production (and acquisition) of the same amount of houses we can say that there is equilibrium between them in the sense that all the people engaged in them may find that they can carry out their plans.[1] This need not be so, because other circumstances which are not part of their plan of action may turn out to be different from what they expected. Part of the materials may be destroyed by an accident, weather conditions may make building impossible, or an invention may alter the proportions in which the different factors are wanted. This is what we call a change in the (objective) data, which disturbs the equilibrium which has existed. But if the different plans were from

[1] Another example of more general importance would, of course, be the correspondence between "investment" and "saving" in the sense of the proportion (in terms of relative cost) in which entrepreneurs provide producers' goods and consumers' goods for a particular date, and the proportion in which consumers in general will at this date distribute their resources between producers' goods and consumers' goods. (Cf. my "Preiserwartungen, monetäre Störungen und Fehlinvestitionen," *Ekonomisk Tidskrift*, Vol. 34, 1935 (French translation: "Prévisions de Prix, Perturbations Monétaires et Faux Investissements," *Revue des Sciences Economiques*, October, 1935) and "The Maintenance of Capital," ECONOMICA (N.S.), Vol. II, 1935, pp. 268–273.) It may be of interest in this connection to mention that in the course of investigations of the same field, which led the present author to these speculations, the theory of crises, the great French sociologist G. Tarde stressed the "contradiction de croyances" or "contradiction de jugements" or "contradictions des espérances" as the main cause of these phenomena (*Psychologie Economique*, Paris, 1902, Vol. II, pp. 128–9; Cf. also N. Pinkus, *Das Problem des Normalen in der Nationalökonomie*, Leipzig, 1906, pp. 252 and 275).

the beginning incompatible, it is inevitable that somebody's plans will be upset and have to be altered, and that in consequence the whole complex of actions over the period will not show those characteristics which apply if all the actions of each individual can be understood as part of a single individual plan he has made at the beginning.[1]

VI

When in all this I emphasise the distinction between mere inter-compatibility of the individual plans[2] and the correspondence between them and the actual external facts or objective data, I do not of course mean to suggest that the subjective inter-agreement is not in some way brought about by the external facts. There would of course be no reason why the subjective data of different people should ever correspond unless they were due to the experience of the same objective facts. But the point is that pure equilibrium analysis is not concerned with the way in which this correspondence is brought about. In the description of an existing state of equilibrium which it provides, it is simply assumed that the subjective data coincide with the objective facts. The equilibrium relationships cannot be deduced merely from the objective facts, since the analysis of what people will do can only start from what is known to them. Nor can equilibrium analysis start merely from a given set of subjective data, since the subjective data of different people would be either compatible or incompatible, that is, they would already determine whether equilibrium did or did not exist.

We shall not get much further here unless we ask for the reasons for our concern with the admittedly fictitious state of equilibrium. Whatever may occasionally have been said by over-pure economists, there seems to be no possible

[1] It is an interesting question, but one which I cannot discuss here, whether in order that we can speak of equilibrium, every single individual must be right, or whether it would not be sufficient if, in consequence of a compensation of errors in different directions, quantities of the different commodities coming on the market were the same as if every individual had been right. It seems to me as if equilibrium in the strict sense would require the first condition to be satisfied, but I can conceive that a wider concept, requiring only the second condition, might occasionally be useful. A fuller discussion of this problem would have to consider the whole question of the significance which some economists (including Pareto) attach to the law of great numbers in this connection. On the general point see P. N. Rosenstein-Rodan, "The Coordination of the General Theories of Money and Price," ECONOMICA, August, 1936.

[2] Or, since in view of the tautological character of the Pure Logic of Choice, "individual plans" and "subjective data" can be used interchangeably, between the agreement between the subjective data of the different individuals.

doubt that the only justification for this is the supposed existence of a tendency towards equilibrium. It is only with this assertion that economics ceases to be an exercise in pure logic and becomes an empirical science; and it is to economics as an empirical science that we must now turn.

In the light of our analysis of the meaning of a state of equilibrium it should be easy to say what is the real content of the assertion that a tendency towards equilibrium exists. It can hardly mean anything but that under certain conditions the knowledge and intentions of the different members of society are supposed to come more and more into agreement, or, to put the same thing in less general and less exact but more concrete terms, that the expectations of the people and particularly of the entrepreneurs will become more and more correct. In this form the assertion of the existence of a tendency towards equilibrium is clearly an empirical proposition, that is, an assertion about what happens in the real world which ought, at least in principle, to be capable of verification. And it gives our somewhat abstract statement a rather plausible common-sense meaning. The only trouble is that we are still pretty much in the dark about (*a*) the *conditions* under which this tendency is supposed to exist, and (*b*) the nature of the *process* by which individual knowledge is changed.

VII

In the usual presentations of equilibrium analysis it is generally made to appear as if these questions of how the equilibrium comes about were solved. But if we look closer it soon becomes evident that these apparent demonstrations amount to no more than the apparent proof of what is already assumed.[1] The device generally adopted for this purpose is the assumption of a perfect market where every event becomes known instantaneously to every member. It is necessary to remember here that the perfect market which is required to satisfy the assumptions of equilibrium analysis must not be confined to the markets of all the individual commodities; the whole economic system must be assumed

[1] This seems to be implicitly admitted, although hardly consciously recognised, when in recent times it is frequently stressed that equilibrium analysis only describes the conditions of equilibrium without attempting to derive the position of equilibrium from the data. Equilibrium analysis in this sense would, of course, be pure logic and contain no assertions about the real world.

to be one perfect market in which everybody knows everything. The assumption of a perfect market then means nothing less than that all the members of the community, even if they are not supposed to be strictly omniscient, are at least supposed to know automatically all that is relevant for their decisions. It seems that that skeleton in our cupboard, the "economic man", whom we have exorcised with prayer and fasting, has returned through the back door in the form of a quasi-omniscient individual.

The statement that, if people know everything, they are in equilibrium is true simply because that is how we define equilibrium. The assumption of a perfect market in this sense is just another way of saying that equilibrium exists, but does not get us any nearer an explanation of when and how such a state will come about. It is clear that if we want to make the assertion that under certain conditions people will approach that state we must explain by what process they will acquire the necessary knowledge. Of course any assumption about the actual acquisition of knowledge in the course of this process will also be of a hypothetical character. But this does not mean that all such assumptions are equally justified. We have to deal here with assumptions about causation, so that what we assume must not only be regarded as possible (which is certainly not the case if we just regard people as omniscient) but must also be regarded as likely to be true, and it must be possible, at least in principle, to demonstrate that it is true in particular cases.

The essential point here is that it is these apparently subsidiary hypotheses or assumptions that people do learn from experience, and about how they acquire knowledge, which constitute the empirical content of our propositions about what happens in the real world. They usually appear disguised and incomplete as a description of the type of market to which our proposition refers; but this is only one, though perhaps the most important, aspect of the more general problem of how knowledge is acquired and communicated. The important thing of which economists frequently do not seem to be aware is that the nature of these hypotheses is in many respects rather different from the more general assumptions from which the Pure Logic of Choice starts. The main differences seem to me to be two:

Firstly, the assumptions from which the Pure Logic of Choice starts are facts which we know to be common to all human thought. They may be regarded as axioms which define or delimit the field within which we are able to understand or mentally to reconstruct the processes of thought of other people. They are therefore universally applicable to the field in which we are interested—although of course where *in concreto* the limits of this field are is an empirical question. They refer to a type of human action (what we commonly call rational, or even merely conscious, as distinguished from instinctive action) rather than to the particular conditions under which this action is undertaken. But the assumptions or hypotheses, which we have to introduce when we want to explain the social processes, concern the relation of the thought of an individual to the outside world, the question to what extent and how his knowledge corresponds to the external facts. And the hypotheses must necessarily run in terms of assertions about causal connections, about how experience creates knowledge.

Secondly, while in the field of the Pure Logic of Choice our analysis can be made exhaustive, that is, while we can here develop a formal apparatus which covers all conceivable situations, the supplementary hypotheses must of necessity be selective, that is, we must select from the infinite variety of possible situations such ideal types as for some reason we regard as specially relevant to conditions in the real world.[1] Of course we could also develop a separate science, the subject matter of which was *per definitionem* confined to a "perfect market" or some similarly defined object, just as the Logic of Choice applies only to persons who have to allot limited means among a variety of ends. And for the field so defined our propositions would again become *a priori* true. But for such a procedure we should lack the justification which consists in the assumption that the situation in the real world is similar to what we assume it to be.

[1] The distinction drawn here may help to solve the old difference between economists and sociologists about the rôle which "ideal types" play in the reasoning of economic theory. The sociologists used to emphasise that the usual procedure of economic theory involved the assumption of particular ideal types, while the economic theorist pointed out that his reasoning was of such generality that he need not make use of any "ideal types". The truth seems to be that within the field of the Pure Logic of Choice, in which the economist was largely interested, he was right in his assertion, but that as soon as he wanted to use it for the explanation of a social process he had to use "ideal types" of one sort or another.

VIII

I must now turn to the question of what the concrete hypotheses are concerning the conditions under which people are supposed to acquire the relevant knowledge and the process by which they are supposed to acquire it. If it were at all clear what the hypotheses usually employed in this respect were, we should have to scrutinise them in two respects: we should have to investigate whether they were necessary and sufficient to explain a movement towards equilibrium, and we should have to show to what extent they were borne out by reality. But I am afraid I am now getting to a stage where it becomes exceedingly difficult to say what exactly are the assumptions on the basis of which we assert that there will be a tendency towards equilibrium, and to claim that our analysis has an application to the real world. I cannot pretend that I have as yet got much further on this point. Consequently all I can do is to ask a number of questions to which we shall have to find an answer if we want to be clear about the significance of our argument.

The only condition, about the necessity of which for the establishment of an equilibrium economists seem to be fairly agreed, is the "constancy of the data". But after what we have seen about the vagueness of the concept of "datum" we shall suspect, and rightly, that this does not get us much farther. Even if we assume—as we probably must—that here the term is used in its objective sense (which includes, it will be remembered, the preferences of the different individuals) it is by no means clear that this is either required or sufficient in order that people shall actually acquire the necessary knowledge, or that it was meant as a statement of the conditions under which they will do so. It is rather significant that at any rate some authors[1] feel it necessary to add "perfect knowledge" as an additional and separate condition. And indeed we shall see that constancy of the objective data is neither a necessary nor a sufficient condition. That it cannot be a necessary condition follows from the facts, firstly, that nobody would want to interpret it in the absolute sense that nothing must ever happen in the world, and, secondly, that, as we have seen, as soon as we want to include changes which occur periodically or

[1] *Vide* N. Kaldor, "A Classificatory Note on the Determinateness of Equilibrium," *Review of Economic Studies*, Vol. I, No. 2, 1934, p. 123.

perhaps even changes which proceed at a constant rate, the only way in which we can define constancy is with reference to expectations. All that this condition amounts to then is that there must be some discernible regularity in the world which makes it possible to predict events correctly. But while this is clearly not sufficient to prove that people will learn to foresee events correctly, the same is true to a hardly less degree even about constancy of data in an absolute sense. For any one individual, constancy of the data does in no way mean constancy of all the facts independent of himself, since, of course, only the tastes and not the actions of the other people can in this sense be assumed to be constant. And as all those other people will change their decisions as they gain experience about the external facts and other people's action, there is no reason why these processes of successive changes should ever come to an end. These difficulties are well known[1] and I only mention them here to remind you how little we actually know about the conditions under which an equilibrium will ever be reached. But I do not propose to follow this line of approach further, though not because this question of the empirical probability that people will learn (that is, that their subjective data will come to correspond with each other and with the objective facts) is lacking in unsolved and highly interesting problems. The reason is rather that there seems to me to be another and more fruitful way of approach to the central problem.

IX

The questions I have just discussed concerning the conditions under which people are likely to acquire the necessary knowledge, and the process by which they will acquire it, has at least received some attention in past discussions. But there is a further question which seems to me to be at least equally important, but which appears to have received no attention at all, and that is how much knowledge and what sort of knowledge the different individuals must possess in order that we may be able to speak of equilibrium. It is clear that if the concept is to have any empirical significance it cannot presuppose that everybody knows everything. I have already had to use the undefined term "relevant

[1] On all this cf. N. Kaldor, loc. cit., *passim*.

knowledge", that is, the knowledge which is relevant to a particular person. But what is this relevant knowledge? It can hardly mean simply the knowledge which actually influenced his actions, because his decisions might have been different not only if, for instance, the knowledge he possessed had been correct instead of incorrect, but also if he had possessed knowledge about altogether different fields.

Clearly there is here a problem of the *Division of Knowledge* which is quite analogous to, and at least as important as, the problem of the division of labour. But while the latter has been one of the main subjects of investigation ever since the beginning of our science, the former has been as completely neglected, although it seems to me to be the really central problem of economics as a social science.[1] The problem which we pretend to solve is how the spontaneous interaction of a number of people, each possessing only bits of knowledge, brings about a state of affairs in which prices correspond to costs, *etc.*, and which could be brought about by deliberate direction only by somebody who possessed the combined knowledge of all those individuals. And experience shows us that something of this sort does happen, since the empirical observation that prices do tend to correspond to costs was the beginning of our science. But in our analysis, instead of showing what bits of information the different persons must possess in order to bring about that result, we fall in effect back on the assumption that everybody knows everything and so evade any real solution of the problem.

Before, however, we can proceed further, to consider this division of knowledge among different persons, it is necessary to become more specific about the sort of knowledge which is relevant in this connection. It has become customary among economists to stress only the need of knowledge of prices, apparently because—as a consequence of the confusions between objective and subjective data—the complete knowledge of the objective facts was taken for granted. In recent times even the knowledge of current prices has been taken so much for granted that the only connection in which the question of knowledge has been regarded as problematic has been the anticipation of future

[1] I am not certain, but I hope, that the distinction between the Pure Logic of Choice and economics as a social science is essentially the same distinction as that which Professor A. Ammon has in mind when he stresses again and again that a "*Theorie des Wirtschaftens*" is not yet a "*Theorie der Volkswirtschaft*".

prices. But, as I have already indicated at the beginning, price expectations and even the knowledge of current prices are only a very small section of the problem of knowledge as I see it. The wider aspect of the problem of knowledge with which I am concerned is the knowledge of the basic fact of how the different commodities can be obtained and used,[1] and under what conditions they are actually obtained and used, that is, the general question of why the subjective data to the different persons correspond to the objective facts. Our problem of knowledge here is just the existence of this correspondence which in much of current equilibrium analysis is simply assumed to exist, but which we have to explain if we want to show why the propositions, which are necessarily true about the attitude of a person towards things which he believes to have certain properties, should come to be true of the actions of society with regard to things which either do possess these properties, or which, for some reason we shall have to explain, are commonly believed by the members of society to possess these properties.[2]

But to revert to the special problem I have been discussing, the amount of knowledge different individuals must possess in order that equilibrium may prevail (or the "relevant" knowledge they must possess), we shall get nearer to an answer if we remember how it can become apparent either that equilibrium did not exist or that it is being disturbed. We have seen that the equilibrium connections will be severed if any person changes his plans, either because his

[1] Knowledge in this sense is more than what is usually described as skill, and the division of knowledge of which we here speak more than is meant by the division of labour. To put it shortly, "skill" refers only to the knowledge of which a person makes use in his trade, while the further knowledge about which we must know something in order to be able to say anything about the processes in society, is the knowledge of alternative possibilities of action of which he makes no direct use. It may be added here that knowledge, in the sense in which the term is here used, is identical with foresight only in the sense in which all knowledge is capacity to predict.

[2] That all propositions of economic theory refer to things which are defined in terms of human attitudes towards them, that is, that for instance the "sugar" about which economic theory may occasionally speak, is not defined by its "objective" qualities, but by the fact that people believe that it will serve certain needs of theirs in a certain way, is the source of all sorts of difficulties and confusions, particularly in connection with the problem of "verification". It is, of course, also in this connection that the contrast between the *verstehende* social science and the behaviourist approach becomes so glaring. I am not certain that the behaviourists in the social sciences are quite aware of *how* much of the traditional approach they would have to abandon if they wanted to be consistent, or that they would want to adhere to it consistently if they were aware of this. It would, for instance, imply that propositions of the theory of money would have to refer exclusively to, say, "round discs of metal, bearing a certain stamp," or some similarly defined physical object or group of objects,

tastes change (which does not concern us here) or because new facts become known to him. But there are evidently two different ways in which he may learn of new facts which make him change his plans, which for our purposes are of altogether different significance. He may learn of the new facts as it were by accident, that is in a way which is not a necessary consequence of his attempt to execute his original plan, or it may be inevitable that in the course of his attempt he will find that the facts are different from what he expected. It is obvious that, in order that he may proceed according to plan, his knowledge needs to be correct only on the points on which it will necessarily be confirmed or corrected in the course of the execution of the plan. But he may have no knowledge of things which, if he possessed it, would certainly affect his plan.

The conclusion then which we must draw is that the relevant knowledge which he must possess in order that equilibrium may prevail is the knowledge which he is bound to acquire in view of the position in which he originally is, and the plans which he then makes. It is certainly not all the knowledge which, if he acquired it by accident, would be useful to him, and lead to a change in his plan. And we may therefore very well have a position of equilibrium only because some people have no chance of learning about facts which, if they knew them, would induce them to alter their plans. Or, in other words, it is only relative to the knowledge which a person is bound to acquire in the course of the carrying out of his original plan and its successive alterations that an equilibrium is likely to be reached.

While such a position represents in one sense a position of equilibrium, it is however clear that it is not an equilibrium in the special sense in which equilibrium is regarded as a sort of optimum position. In order that the results of the combination of individual bits of knowledge should be comparable to the results of direction by an omniscient dictator, further conditions must apparently be introduced.[1] And while it seems quite clear that it is possible to define the amount of knowledge which individuals must possess in order that this result should be obtained, I know of no real attempt in this direction. One condition would

[1] These conditions are usually described as absence of "frictions". In a recently published article ("Quantity of Capital and the Rate of Interest," *Journal of Political Economy*, Vol. XLIV/5, 1936, p. 638) Professor F. H. Knight rightly points out that "'error' is the usual meaning of friction in economic discussion".

probably be that each of the alternative uses of any sort of resources is known to the owner of some such resources actually used for another purpose and that in this way all the different uses of these resources are connected, either directly or indirectly.[1] But I mention this condition only as an instance of how it will in most cases be sufficient that in each field there is a certain margin of people who possess among them all the relevant knowledge. To elaborate this further would be an interesting and a very important task, but a task that would far exceed the limits of this paper.

But although what I have said on this point has been largely in the form of a criticism, I do not want to appear unduly despondent about what we have already achieved in this field. Even if we have jumped over an essential link in our argument, I still believe that by what is implicit in its reasoning, economics has come nearer than any other social science to an answer to that central question of all social sciences, how the combination of fragments of knowledge existing in different minds can bring about results which, if they were to be brought about deliberately, would require a knowledge on the part of the directing mind which no single person can possess. To show that in this sense the spontaneous actions of individuals will under conditions which we can define bring about a distribution of resources which can be understood as if it were made according to a single plan, although nobody has planned it, seems to me indeed an answer to the problem which has sometimes been metaphorically described as that of the "social mind". But we must not be surprised that such claims on our part have usually been rejected by sociologists, since we have not based them on the right grounds.

There is only one more point in this connection which

[1] This would be one, but probably not yet a sufficient, condition to ensure that, with a given state of demand, the marginal productivity of the different factors of production in their different uses should be equalised and that in this sense an equilibrium of production should be brought about. That it is not necessary, as one might think, that every possible alternative use of any kind of resources should be known to at least one among the owners of each group of such resources which are used for one particular purpose is due to the fact that the alternatives known to the owners of the resources in a particular use are reflected in the prices of these resources. In this way it may be a sufficient distribution of knowledge of the alternative uses, $m, n, o, \ldots y, z$, of a commodity, if A, who uses the quantity of these resources in his possession for m, knows of n, and B, who uses his for n, knows of m, while C who uses his for o, knows of n, *etc.*, *etc.*, until we get to L, who uses his for z, but only knows of y. I am not clear to what extent in addition to this a particular distribution of the knowledge of the different proportions is required in which different factors can be combined in the production of any one commodity. For complete equilibrium additional assumptions will be required about the knowledge which consumers possess about the serviceability of the commodities for the satisfaction of their wants.

I should like to mention. This is that if the tendency towards equilibrium, which we have reason to believe to exist on empirical grounds, is only towards an equilibrium relative to that knowledge which people will acquire in the course of their economic activity, and if any other change of knowledge must be regarded as a "change in the data" in the usual sense of the term, which falls outside the sphere of equilibrium analysis, this would mean that equilibrium analysis can really tell us nothing about the significance of such changes in knowledge, and would go far to account for the fact that pure analysis seems to have so extraordinarily little to say about institutions, such as the press, the purpose of which is to communicate knowledge. And it might even explain why the preoccupation with pure analysis should so frequently create a peculiar blindness to the rôle played in real life by such institutions as advertising.

X

With these rather desultory remarks on topics which would deserve much more careful examination I must conclude my survey of these problems. There are only one or two further remarks which I want to add.

One is that, in stressing the nature of the empirical propositions of which we must make use if the formal apparatus of equilibrium analysis is to serve for an explanation of the real world, and in emphasising that the propositions about how people will learn, which are relevant in this connection, are of a fundamentally different nature from those of formal analysis, I do not mean to suggest that there opens here and now a wide field for empirical research. I very much doubt whether such investigation would teach us anything new. The important point is rather that we should become clear about what the questions of fact are on which the applicability of our argument to the real world depends, or, to put the same thing in other words, at what point our argument, when it is applied to phenomena of the real world, becomes subject to verification.

The second point is that I do not want of course to suggest that the sort of problems I have been discussing were foreign to the arguments of the economists of the older generations. The only objection that can be made against them is that they have so mixed up the two sorts of propositions, the

a priori and the empirical, of which every realistic economist makes constant use, that it is frequently quite impossible to see what sort of validity they claimed for a particular statement. More recent work has been freer from this fault—but only at the price of leaving more and more obscure what sort of relevance their arguments had to the phenomena of the real world. All I have tried to do has been to find the way back to the common-sense meaning of our analysis, of which, I am afraid, we are apt to lose sight as our analysis becomes more elaborate. You may even feel that most of what I have said has been commonplace. But from time to time it is probably necessary to detach oneself from the technicalities of the argument and to ask quite naïvely what it is all about. If I have only shown that in some respects the answer to this question is not only not obvious, but that occasionally we do not even quite know what it is, I have succeeded in my purpose.