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PERFECT FORESIGHT AND ECONOMIC EQUILIBRIUM*

by
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The theory of general economic equilibrium, which has been developed in different forms, constitutes the pride of theoretical economics. The variations, however, concern insignificant matters, so that, by and large, one may speak of a uniform theory, whether it is expressed verbally or in mathematical terms. For this reason, it is all the more curious that, in the whole body of literature, there can be found systematically compiled neither exact nor complete statements about the assumptions underlying the theory of general equilibrium. One looks in vain on this score even in Walras and Pareto, although it would be supposed that they and their followers would have given special attention directly to the deduction of the proposition from the premises. The deficiency mentioned shows clearly the common logical carelessness in expression which characterizes theoretical economics to a much higher degree than up to the present has been made clear. One is accustomed in economics with regard to a series of questions which are today still quite unexplained, to be satisfied far too hastily, and to pass on to special questions, about which so much less may be said the longer the other situation continues. The mathematical economists present an especially noteworthy example. They, indifferent whether it is a question of a general or of some particular equilibrium, have been content to assert that there are present as many equations as there are unknowns, rather than from the start proving in an exact mathematical fashion that there is a solution at all—and a unique solution—for these equations.[1]

The following statements, treated very briefly and, therefore, often perhaps only tentative, are directed at pointing out a problem of equilibrium theory, and thereby, every variety of theoretical economics. It is a problem which is immediately raised if there is once attempted an examination, certainly completely justified, of an often-professed and an often-unchallenged premise of every theory. It is the question of the assumption of (here used synonymously) "*full foresight*" or "*perfect foresight*", which is ostensibly one of the preliminary conditions of equilibrium. Whatever form the exact composition or formulattion of the assumptions of the several writers quoted below may take, I shall show in the following discussion that a true problem of any theory, the solution of which is unqualifiedly wanted, can be exhibited irrespective of their observations, which are perhaps historically interesting but really unimportant because of their incompleteness.

[1] *Cf.* K. Menger and A. Wald: *Ergebnisse eines mathematischen Kolloquiums*, Vienna, 1935. Part 6.

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Selecting several works from the writings of different theoreticians, we obtain the following typical illustration: J. R. Hicks, to whom it is of importance to determine the role of foresight even explicitly, after a generalization of the idea of equilibrium in Walras and Pareto, reaches the assertion: *"The condition pre-requisite for equilibrium is perfect foresight*[2]", from which he then draws the rather momentous conclusion that monetary theory lies outside equilibrium theory because the use of money exists only in close association with imperfect foresight (p. 448). "The tacit assumption of perfect foresight takes actually every monetary objective from the 'numeraire' " (p. 446). The above-mentioned assumption of Walras and Pareto is made only tacitly, since no precise expression about it is found in their writings. Hicks, indeed, accepts free competition as reconciliable with imperfect foresight. But in direct antithesis is the conception of F. H. Knight, who, in his well-known work, *Risk, Uncertainty and Profit*, (Boston, 1921), asserts as follows: "Chief among the simplifications of reality prerequisite to the achievement of perfect competition is, as has been emphasized all along, *the assumption of practical omniscience on the part of every member of the competitive system*" (p. 197). H. L. Moore, who, like Hicks, complains about the fact that there is no exact and complete exposition of the premises of equilibrium, compiles these to the best of his ability, and in the compilation, he does not even mention the significant question of the foresight of the economic subject.[3] Included in this exposition, however, is an interesting citation from the *Cours* of Pareto: "L'echangeur subit les prix du marché sans essayer de les modifier de propos delibéré. Ces prix sont modifiés effectivement par son offre et sa demande, mais *c'est à son insu*" (p. 15), which, again, does not hint at a perfect foresight which might induce the individual to behave differently. H. v. Stackelberg, in his book, *Marktform und Gleichgewicht*, (Vienna, 1934), likewise makes a compilation of the conditions of equilibrium, and in this, too, there is not a word regarding foresight. The fact seems all the more remarkable because the theory of duopoly directly demonstrated and developed by von Stackelberg in this book deals expressly with the assumptions which the individual duopolists make about the conduct of the partner at the time, or what is known about each other's views concerning the market.

It may be assumed as well-known that the whole theory of risk, even as great as the differences are here, is to a large extent uniformly interpreted to mean that full foresight removes any risk, this being just a product of the imperfection of foresight. The same thing is true of the theory of profit, which is perceived as a result of the research concerning risk; the existence of a relationship between the degree of risk in undertaking and the profit is shown. Irving Fisher, in this *Theory of Interest* (New York, 1930), in several chapters expressly accepts perfect foresight as an important element of his theory. Likewise, J. M. Keynes, in his *Treatise on Money*, speaks frequently of "correct forecasting" or "accurate forecasting", which, it is true, need not absolutely coincide with complete foresight, although the accompanying text borders upon this interpretation; the theory of monetary equilibrium is apparently included by Keynes in the general economic equilibrium; at present, however, Keynes has accepted Hicks' view.

On the other hand, again, F. A. Hayek thinks that the equilibrium theory has been erected by abstracting the element of time and hence without inclusion of an element of

[2] J. R. Hicks, "Gleichgewicht und Konjunktur". *Zeitschrift für Nationalökonomie*, Vol. IV, p. 445.

[3] *Synthetic Economics*, New York, 1929.

foresight: "The principal difficulty of the traditional analysis existed certainly in its complete abstraction of the element of time. A concept of equilibrium which, in reality, had application only to an economy timelessly conceived could not be of great value. Fortunately, in this respect, however, it has just recently been greatly changed. It has become clear that in place of a simple negligence of the time-motive, well-defined assumptions must deal with the attitude of the persons concerned as regards the future. *Assumptions* of this kind, which the analysis of equilibrium must make, *are substantially that all persons concerned correctly forsee the relevant events in the future*, and this foresight has to include not only the change in objective data *but also the behaviour of all other persons*. It is not my intention to enter into detail with this newer development of the equilibrium analysis as such. Rather, I hope that these bare allusions will be sufficient in making intelligible certain conclusions which appear to be drawn from them for the analysis of dynamic events. It seems to me, namely, as if certain ideas which we are accustomed to use ordinarily in a somewhat light fashion appear, from this point of view, to have a precise meaning. I am thinking here especially of the assumptions so repeatedly used in business cycle theory, that either a *complete economic system or a certain price, such as, perhaps, interest, is or is not considered in equilibrium*."[4]

Obviously, these representative views are in no way uniform. Nevertheless, the opinion prevails that the theoretical perfection of equilibrium could not be obtained without the assumption of complete foresight by the economic subjects and the entrepreneurs. This idea clings, at least as much as in the special theories just mentioned, even to the works of those authors who make no assertions about the problem being raised here. Whoever avoids this can, of course, claim to have or have had any agreeable view, which, however, would have nothing to do with scientific knowledge.

Finally, it may be noted briefly that in business-cycle theory, an important, yet up to now a nebulous role, is assigned to the "errors over time" (M. Fanno) or the "Expectations of businessmen" (A. C. Pigou) and their variations due to an imperfect management of foresight. If an important place is due them, then, purely theoretical investigations of this sort have a wider interest, if it is at all necessary to stress this, for the problem presented deals with one of the fundamental questions of theoretical economics and sociology. The reference to the theory of equilibrium is not significant in the sense of its being a reference to a specific and different variant of the theory, but, rather, the theorem of equilibrium merely represents, for the sake of simplicity, any kind of economic theory.

II.

Foresight is spoken of by the authors just discussed, and, as far as evident, also by the rest of the authors, simply as "full" or "perfect", without their making a closer examination of what is actually meant thereby. Apparently, they believe that it is a completely obvious matter. Without regard to literary connotations, this question may, in a few words, be answered systematically. In the first place, the formulation of the question is incomplete, since there needs to be asked: the foresight of whom? of what kind of

[4] "Preiserwartungen, Monetäre Störungen und Fehlinvestitionen", *Nationalökonomisk Tidskrift*, 1935, Vol. 73, Part 3. Italics mine. O. M.

matters or events? for what local relationships? for what period of time? These basic questions, from which a meaning of perfect foresight may, for the first time, be obtained, lead to a whole variety of problems.

The answer first to be considered is that full foresight, in reference to the period of time taken as unlimited, must mean a foresight up to the end of the world, or at least beyond the last moment managed by someone in the economy (thus, even by the state itself). But this is multifarious, and it immediately points to the extravagent use of certain undefined premises of theoretical economics. On the other hand, it must be admitted as fundamental that with full foresight, precisely no kind of a limitation can be imposed. Yet, if foresight up to a certain, exactly specified moment—indifferent whether it is the next second or in the more distant future—is defined as that which is called perfect, then, this is expressly to be added. Otherwise, any assertion containing the term can have no meaning. The implication of time is valid for any kind of foresight; thus, even for imperfect. So it is of signal importance to economics because it follows that even for the most elementary theses of the theory of equilibrium, recognition must be taken of what is the period to be considered, what periods of production are given, and over what points of time do the individuals look out for themselves and their descendants. It follows from this that in the theory of equilibrium, the time factor has to be clarified, and the element of time must be included in it in an unobjectionably specified manner. As noted, this was not the case with either Walras or Pareto or with any of the other authors. At this point, then, there is already discovered an obsecurity of these theories—one which, as is later exposed, leads further to a grave dilemma.

The necessary, more precise definition of the geographical relationships may well remain unfinished in this frame, for the difficulty produced here are minor in comparison to those created by the element of time. On the other hand, the question worthy of consideration is: what shall properly be foreseen, then, inasmuch as the complications arise because of the full interdependence of all economic events? The theory of equilibrium must posit that every economic event has to be foreseen just because, as it itself proves, full interdependence exists. An analysis of the case of partial equilibrium may be at variance with the position of departure of a general equilibrium, disregarding the fact that a partial equilibrium presupposes wider economic events in which it is imbedded.

So the question arises, what, in that case, shall really be foreseen? The question is no trivial matter, although the answer is comparatively simple to give: Economic things and events shall be foreseen.

Under the granted assumptions, it may be quite clear what is meant—for example, prices, production, revenue, etc. In consequence of the interdependence of all economic processes and given conditions on one another and this with all other facts, no instance could be given of a sector, however small, of the event, the foresight of which would not mean, at the same time, the foresight of all the rest. Should any residue be neglected, perhaps because it be regarded as too insignificant for practical behavior, then, it would have to be foreseen exactly, in order to be neglected even directly as *unimportant*. Consequently, nothing is changed regarding implicit determination in the case of "full" foresight. As long as this condition is granted, it is a practical matter which has nothing to do with the theoretical problem. It is fundamental to point out here that the foresight of

complex economic magnitudes, such as, for example, prices, the volume of money, costs, profits, etc. postulate, just because of the perfection of this foresight, that the constituents of these complex magnitudes also be foreseen. The most important and final elements of this kind are the *individual acts* out of which the complex magnitudes arise. The individual exercising foresight must thus not only know exactly the influence of his own transactions on prices but also the influence of every other individual, and of his own future behavior on that of the others, especially of those relevant for him personally.

The circle of these relevant individuals is extraordinarily large, since, besides, all indirect effects would have to be considered accurately. Obviously, one will have to erect very narrow limits by a more reasonable definition of foresight and, as with the extension of time, here, too, with the pertinent breadth and depth. But, then again, it is not "perfect foresight". This, if it is not qualified, leads to the conclusion that the individuals would have to have complete insight into theoretical economics, for how else would they be able to foresee action at a distance? The epistemologically and logically interesting deductions which result from this have to be reviewed somewhat closer, especially to apply them to the problem in question.

The impossibly high claims which are attributed to the intellectual efficiency of the economic subject immediately indicate that there are included in this equilibrium system not ordinary men, but rather, at least to one another, exactly equal demi-gods, in case the claim of complete foresight is fulfilled. If this is the case, there is, of course, nothing more to be done. If "full" or "perfect" foresight is to provide the basis of the theory of equilibrium in the strictly specified sense, and in the meaning obviously intended by the economic authors, then, a completely meaningless assumption is being considered. If limitations are introduced in such a way that the perfection of foresight is not reached, then these *limitations are to be stated very precisely*. They would have to be so narrowly drawn that the fundamental aim of producing ostensibly full rationality of the system by means of high, *de facto* unlimited, foresight, would be lost. For the theoretical economist, there is no way out of this dilemma. In this discussion, "full" and "perfect" foresight are not only used synonymously, but both are employed, moreover, in the essentially more exact sense of *limitlessness*. This expression would have to be preferred because with the words "perfect" or "imperfect", there arise superficial valuations which play no role here at all.

I am now going on to examine somewhat more closely the conditions which result if full foresight is posited, and *especially if there results reciprocal inclusion of foresight about the probable behavior of others* resulting from their analysis of complex quantities, such as prices, etc. The fact is that a calculation of the effects of one's own future behavior always rests on the expected future behavior of others, and *vice versa. This can be observed empirically every time.*[1] However, the chain of surmised mutual "reactions" breaks off comparatively soon; often, too, they play no excessive role because of the power of the external data of a physical nature. This may be the case on certain markets, for example, as the stock-exchange. With unlimited foresight, it is something else. On another occasion, I gave an example of this paradox, involving two individuals, and it may be reproduced here simply:

"Sherlock Holmes, pursued by his opponent, Moriarity, leaves London for Dover. The train stops at a station on the way, and he alights there rather than travelling on to

Dover. He has seen Moriarity at the railway station, recognizes that he is very clever and expects that Moriarity will take a faster special train in order to catch him in Dover. Holmes' anticipation turns out to be correct. But what if Moriarity had been still more clever, had estimated Holmes' mental abilities better and had foreseen his actions accordingly? Then, obviously, he would have travelled to the intermediate station. Holmes, again, would have had to calculate that, and he himself would have decided to go on to Dover. Whereupon, Moriarity would again have "reacted" differently. Because of so much thinking they might not have been able to act at all or the intellectually weaker of the two would have surrendered to the other in the Victoria Station, since the whole flight would have become unnecessary. Examples of this kind can be drawn from everywhere. However, chess, strategy, etc. presuppose expert knowledge, which encumbers the example unnecessarily."[5]

One may be easily convinced that here lies an insoluble *paradox*. And the situation is not improved, but, rather, greatly aggravated if we assume that more than two individuals—as, for example, is the case with exchange—are brought together into a position, which would correspond to the one brought forward here. Always, *there is exhibited an endless chain of reciprocally conjectural reactions and counter-reactions. This chain can never be broken by an act of knowledge but always only through an arbitrary act—a resolution*. This resolution, again, would have to be foreseen by the two or more persons concerned. The paradox still remains no matter how one attempts to twist or turn things around. *Unlimited foresight and economic equilibrium are thus irreconcilable with one another*. But can equilibrium really take place with a faulty, heterogeneous foresight, however, it may be disposed? This is the question which arises at once when an answer is sought. One can even say this: has foresight been truly introducted at all into the consideration of equilibrium, or, rather, does not the theorem of equilibrium generally stand in no proven connection with the assumptions about foresight, so that a false assumption is being considered?

The example presented here for the appraisal of the probable influence of one's own future behavior on the future behavior of someone else and his reactions on one's own may seem trivial. It has been presented precisely for that reason in order to point out that there is in the total literature no investigation of such relationships, limited either to two persons or extended to a multiplicity. The resulting events are so extremely complicated that only far-reaching employment of mathematics could help to suggest the reciprocal dependencies. The relationship between human behaviors dependent on one another, even without the assumption of foresight, is almost inconceivably complicated, and it requires cogent examination. This would furnish a purely formal framework, into which, for the first time, the special relationship between quantities and prices, as we endeavor to discover it in the economy, could be embedded, and, thereby, theoretical economics might assume some day a more secure basis. Up to the present time, the only examination of a *strictly* formal nature about social groups, even though it is carried out in another field and is limited to the co-existent individuals independent of one another, is a work by K. Menger, which it is hoped, will become known to economists and to socio-

[5] *Wirtschaftsprognose, Eine Untersuchung ihrer Voraussetzungen und Möglichkeiten*. Vienna, J. Springer. 1928. P. 98.

logists because of its importance in laying the foundation for further work.[6]

Next to the assumption of complete, unlimited foresight, there must be rejected, too—as is, likewise, pointed out in *Wirtschaftsprognose* (Vienna, 1928)—the assumption that there exists no foresight at all. That would mean complete anarchy in the conduct of men, with experience being plainly in contradiction. Such an assumption would make the existence of the economy just as impossible as that of economic theory which, as does all science, has to posit a minimum of uniformity in the world. That there is no kind of foresight would be the equivalent of the assertion that the acts of choice of the individuals could not be arranged at all, which, apparently, contradicts one of the other, really indispensable postulates of equilibrium theory. So it can be maintained that some positive degree of "knowledge" as to future behavior, that is, one with more or less established assumptions about the future, is absolutely necessary for the economy. This is expressed, for example, in the fact that the individual today abstains from practicable purchases, since experience has shown him that the price will not be changed until tomorrow, or the next day, etc. He would not be able to make this kind of assumption but, rather, generally to have none; so any conjecture about the next day's price would be equally probable. This illustrates the great role of the *past* on economic behavior—a role which L. Robbins, in agreement with G. Cassel, completely misunderstood when he expressly asserted: "The past is irrelevant".[6] It is quite impossible to eliminate from the present behavior of consumers and entrepreneurs the past to which even the minute just finished belongs!

Although it can be demonstrated that full foresight leads to contradiction, it is necessary, nevertheless, to dwell somewhat upon this idea, since there are obtained quite a number of by-results which are or could be of importance in theoretical economics. The necessity that any individual view with complete foresight all economic relationships—consequently, that he has to be master of theoretical economics—leads to a fact noteworthy for epistemology. Should complete foresight be an indispensable postulate for the erection of the theory of equilibrium, then, there results that wider paradox that the science has already posited the object that it is first to investigate; that, without this assumption, the object could not exist at all in the meaning specifically considered.

The epistemological situation is most clearly to be demonstrated in connection with the natural sciences. In physics or chemistry, there is in no way posited the pre-existence of physical or chemical theses in connection with the objects to be explained by these sciences—for example, atoms and elements—in such a way that the atoms need to make assumptions about the behavior and conditions of other atoms. Conceivably, there exists a complete separation. With theoretical economics, as with the most developed representatives of the social sciences, the situation is quite different. The phenomenon of the economy cannot exist at all if there are not posited by the individual in connection with the object at least certain simple elements of the science in the form of the individual's insight into relationships. Without this, it could exist in none of its inflections, however the combination of the several individual economies be selected—whether barter or

[5] K. Menger: *Moral, Wille und Weltgestlaltung. Grundlegung zur Logik der Sitten.* Vienna: J. Springer. 1934. Special attention is given in the fourth chapter to newly developed mathematical methods, in permitting their employment and amplification to the different kinds of economic problems. Particularly, uses arise for them in relation to a theoretical basis of economic policy. (This work is analyzed in my paper: "Logistik and Sozialwissenschaft", Zeitschrift für Nationalökonomie, 1936)

[6] *An Essay on the Nature and Significance of Economic Science.* London, 1932. p. 62.

developed monetary-intercourse, whether with or without governmental economic policy and restraint. Herein lies a kind of contradiction which, in my opinion, cannot be avoided for the aforementioned case, wherein *full* knowledge of the science, still not existing, has to be attributed to individuals because of full foresight. The remedy would lie in analogous employment of the so-called Russell *theory of types* in logistics.[7] This would mean that on the basis of the assumed knowledge by the economic subjects of theoretical tenets of Type I, there can be formulated higher propositions of the theory; thus, at least, of Type II. On the basis of information about tenets of Type II, propositions of Type III, at least, may be set up, etc. But, obviously, one has to start from below in order to build up; one cannot begin at once with the highest type. This tells against the type of theory may not be attained at all, but rather, insofar as there is assumed at the start too much that itself still needs explanation as the theory itself is concerned.

It is also conceivable that for the third type (of the degrees of development) of theoretical economics, the economic subjects actually have knowledge of the second type and act accordingly. Then altogether something else results than if they conduct themselves still only according to the first, as the lowest, type. In this connection, however, no further investigation will be made here.

It is, of course, necessary to define more closely the first type as the lowest type. Certainly, it must include the least possible number of elements. Conceivably, with these considerations, it is a question of purely logical relationships, so these arguments may, likewise, proceed quite abstractly. The lowest type can theoretically be ascertained in the following manner: the individuals could, from the first, have any sort of views and opinions about the relationships relevant to their behavior in the narrowest range. If these views are all false, then the starting position will not produce stability—ceteris paribus—and the continuance of want satisfaction plans will not succeed. Therefore, they proceed by adjustment—as with exchange—to improvements. This happens until, exclusively through changes of their opinions (strictly eliminating everything else) there is no longer any improvement in the sense of constant welfare. With this at least, permanence of the situation is reached; however, not excluding that, owing to deeper discernment, a likewise more durable situation would be possible on a more or less higher level with equal supplies of goods. The views necessarily bound up with this most simple situation about the economic connections (of the economic organization under consideration) may be defined as "knowledge of Type I", which is called the lowest type. Here is the point: since neither Walras nor Pareto unequivocally stated that full foresight is one of their assumptions—this is mostly upheld for them, however, by their modern interpreters, as we have seen—it can even be accepted that they introduced the precedent of the gradual formation of equilibrium *without* perfect foresight. The above-described adaptation is, no doubt, fully analogous to the determination of prices by Walras through the "prix crié" and its successive improvements through the differing bids of buyers; it is equally valid for Edgeworth's idea of "recontracting". If the buyers all had perfect foresight, then the first "prix crié" would have to be at the same time the final price. There is to be remembered here, furthermore, the famous Walrasian formulation that the equilibrium takes place "par tâtonnement", a view which seems to be held also by Schumpeter. *Successive adjustments are likewise irreconciliable with perfect foresight.* It is extremely

[7]For a statement, compare, among other, D. Hilbert and W. Ackermann: *Grundzüge der theoretischen Logik*, Berlin, 1928. Chapter IV, paragraph 4.

significant, however, that it is constantly tried in theoretical economics to attain end-results after disturbances always by means of gradual adjustments. Given the technical difficulties of prompt adjustment, they disappear instantly, however, if the moments when they shall be completed are known. This implies that the adjustment of price in the sense of reaching an equilibrium must be achieved instantly. Consequently, either the theory is employed paradoxically or it contains, according to the assumption of authors such as Walras and Pareto, no kind of assertion about perfect foresight.

As one can easily see, it corresponds with reality, and it is empirically observable, that there may be very different kinds of states of economic well-being in many countries, if we allow to vary of all data only that factor of insight into economic relationships. "Backward" countries need not always be those countries in which obsolete production methods are employed and scarcity of capital prevails, but generally, too, they are those in which economic knowledge is very limited. For that reason, there are revealed possibilities for investigations using this kind of research in the great field of economic policy, as well as in the wider sense, too, for the entire complex of questions of "rational behavior", which are apparently closely tied up with the group of problems discussed here; the implications of rational behavior are still not completely revealed.

It seems possible to avoid these difficulties in the following manner: it is enough if every individual belonging to the economy concerned simply knows what the concrete situation will be on a certain future market; he does not seem to be required to have a presentment of how the prices and actions of the exchange are to be traced back to his own trading, his ownership of goods, his views, and the views of all others. This opinion is incorrect, however, for it is also posited by the theory that the individual acts *rationally*. The "rationality" posits, however, in its turn, that the economic subjects themselves perceive the connections and dependencies—that they really see through the relationship to a certain degree.

It is necessary, forthermore, to refer to the following in all exactness: if perfect foresight on the part of some *outside observer* is assumed, then the question is quite different from the one previously discussed. Whatever is to be said about such an observer—in this case, for example, the theoretical economist—finds expression in statements parallel to the familiar postulates of theology and logic, about the omniscience of God concerning the future and about the difficulties of free will connected with it. It is quite clear that one must distinguish between (a) the *purely theoretical* knowledge of relationships and (b) perfect foresight; for the latter is characterized in such a way that a concrete (best-expressed as historical) object in respect to the single process has to be looked at in every detail. This is more than the theoretician can ever accomplish with his theory. Noting the distinction may be sufficient indication of this fact, since we are not touching upon the position of an observer so equipped. He represents no necessary assumption of the theory of equilibrium, so the problem need not be discussed. Complicated though it may be, and difficult though the questions of logic it raises are; it may nevertheless be ignored, because it stands far away from the complicatedness of the idea of individually perfect foresight, since with this, too, there is made the additional assumption that all individuals treated thus have this perfect knowledge—indeed, the same perfect knowledge among one another. This leads to a completely inexplicable paradox, as was demonstrated broadly above. If there must be pointed out, from the simplest set of assumptions, the unavoidability of a paradox (on account of the reflection of one's own conduct on that

of the others and *vice versa*), then, it is obviously superfluous to discuss all or even some of the wider aspects of this paradox. With its demonstration, there departs the idea in the beginning of this discussion that "perfect foresight" could constitute a necessary, or even significant, assumption for the theory of equilibrium, and it can be employed just as little for any special theory.

In order to obviate misunderstanding, it must be shown, however, that the following assumption leads to no contradiction: taking for granted that the theory of equilibrium already *exists* in final, complete form (that is, of such a kind that there no longer remains any problem to be solved), then, we could construct a case in such a way that this completed science would be recognized uniformly by all economic subjects and understood equally well by all.

If we do not make the *additional* assumption that they possess perfect foresight as well, then we are dealing only with a quite unusual, superior class of economic subjects, without expressing any assertion which presents a new or additional problem. In no case has there resulted a change of objects for the theory.

This assumption of the perfect knowledge of a completed theory of equilibrium is, however, in no way identical with the Walrasian conception of full foresight. It has been pointed out that the latter assumed the first, but, as one can soon see, the inversion is not admissible. A group of economic subjects can, consequently, have a perfect knowledge of the science, but they need not have greatly different knowledge of the future than men have today. These individuals are distinguished only by deeper insight into the relationships which arise form the arrangement of the data. But they may err in their assumptions about the data, optimism and pessimism can be expressed, etc. In this economy of equilibrium, mistakes can occur, concealed information may exist, "temporary monopoly of the knowledge of changes in data"[8] be present, and, consequently, risk, profit, etc. If, therefore, in the present theory of equilibrium, it is maintained that risk and profit are absent in a state of equilibrium, then, they must be proven apparently, to have another basis than by allusion to perfect foresight. To this end, it would be necessary to trace still wider relationships between the degree of foresight and the equilibrium.

These statements can be made more precise by the introduction of the distinction between technical "foreseeability" and *effective foresight*. With this distinction, we allude to an element which could be profitably used, for example, in the theory of risk, where, however, it has been neglected. Effective foresight can be less (and only by accident greater) than technical foreseeability. This is the case, for example, when a government makes an inflation and believes at the same time that the monetary equilibrium will not be disturbed. From the technical data obtainable from time to time, differences of effective foresight may exist, according to the degree of actual employment of the economic science. This implies, notwithstanding, that, owing to other disturbances—for example, because the people hoard the additional money contrary to expectation, and the like—the results which should be expected from the accepted empirical connection of things might not occur. There emerges a difference between the production of a chain of reasoning on the one hand and the historical events, dependent also on other factors, on the other.

[8] *Cf. Wirtschaftsprognose*, Vienna, 1928, pp. 8 f., 30, etc.

In order to make clear the distinction between technical foreseeability and effective foresight, and, at the same time, to show that it exists in the whole realm of human behavior (this means for the whole theory of sociology), a simple example may be presented: when a pedestrian acquainted with city traffic crosses a bustling street, he will properly turn aside in order to avoid the passing automobiles so that he will remain unharmed. He is able to do this because the traffic regulations and customs are familiar to him, he has learned to estimate the velocity of the automobile, etc. He knows, too, what the automobiles ought to do and, from previous experience, what other properties they have. He has at his disposal a certain measure of technical foreseeability, which he applies to the concrete situation and, in such a way, he attains a definite effective foresight. But if our pedestrian is a savage suddenly transplanted into the city, he will not know how to act, although after some time, the same *technical* data are likewise placed at his disposal. He will not know at what distance the automobiles can apply the brakes, what the changing of the red and green lights means, etc. But it is possible, nevertheless, that the savage remains unharmed, and the skilled townsman gets run over, because, for example, the brakes of the automobile encountered failed, a fact which was *not* part of his technical foreseeability. Only "perfect foresight", or at least such a foresight as would include the critical element in it, which would, consequently, give knowledge of *individual historical events and occurrences*: only this would have given the knowledge of this individual case, or pure chance would have permitted the pedestrian to escape his danger.

There exists, consequently, in social life a farther going foreseeability—that is, above all, the work of the science—as it is practically employed and need be employed. First of all, the degree of foresight is very unequal, and it is distributed quite differently, among different parts of the population—a circumstance which will later be given due account.

We now turn back to the idea of perfect foresight, in order to clarify a further relationship which up until now has been left in the dark as far as the theory is concerned. Apparently, with perfect foresight (we are assuming that the term has an exact meaning here), there is *identity* between *foresight and the expectation of the future*.[9] If I know quite clearly that in three days, a specified price will be at a specified level, then, it is the same thing that I am truly *expecting* the occurrence of this event. Had I expected another price, I should not have had certain, perfect foresight. In such an economy, too, all factors of sentiment like optimism, etc. would be eliminated. With imperfect foresight, with the possibility of other prices, e.g., inability to eliminate factors of disturbance in my expectation, it is always conceivable that I, on the grounds of temperament, of caprice, of daring, etc. form my expectation differently than technical foreseeability would, perhaps, make it necessary for me. For example, I am inclined at one time and at another I am

[9] The introduction of the element of *expectation* in the theory is, thus, fundamentally more difficult than is realized by those authors who perceive an advancement of the theory, if expectation is spoken of without its being defined.

I am thinking here, for example of G. *Myrdal*: "Der Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse." (Beiträge zur Geldtheorie, Vienna, 1933, p. 362 ff.). There is no analysis made of wherein the anticipations exist, what determines them, etc. Too, there is in no way proven that anticipation and expectation must coincide; however, I do not want to go into these wider difficulties here. Another author, who may be mentioned in this connection, is F. A. Hayek (see above, p. 339), as well on account of his application of the Jevons-Wicksellian "Investment function" as because of his view that full foresight could bring a *clarification* of equilibrium ideas, while in fact it furnishes a paradox!

not inclined to undergo a risk. In other words, where really effective final foresight is lacking, the element of expectation appears, if the individual is forced to set immediately acts of behavior in respect to the future or to forego such transactions. *Expectation depends, thus, only to a limited degree on foresight*; it may not be based on anything at all, so that the individual will end up with dissolution of the business, loss of capital, or with a profit. As one can see at once, the theory of risk must be attacked from this angle.

Here we have behavior analogous to that described by K. Menger,[10] where individuals, conditions precisely defined, value the importance of uncertainty differently than it would be valued according to a calculation of probability. But, since, for the construction of an orderly theory of risk, one would have to go far back fundamentally, and particularly exact account of the construction of the theory of probabilities, would have to be taken, we may leave the matter with these observations for the present.[11]

It is, perhaps, not uninteresting to point to some of the economic implications which are included in "perfect foresight". It will immediately be recognized that this assumption could never lie at the basis of the theory of equilibrium, and they who attribute this to such authors as Walras and Pareto, who are included as representatives of equilibrium theory, are in error. In the first place, strange to say, it happens that even *material* assertions can be made about such an economy on the basis of the assumption of perfect foresight. They are fundamentally of the negative type. For example, no lotteries or gambling will exist, for who would play if it were well-established where the profit went? Telephone, telegraph, newspapers, bills, posters, etc. would, likewise, be superfluous, obviously; but, also, the very important industries, based on them, with all their affiliated industries, would be absent. Only packages and letters implying documentary evidence would need to be delivered by post, for to whom would letters be written? The tale need not be carried further, for it is obvious how little considered are the "fundamental assumptions" so frequently employed in theoretical economics, where really a matter of nonsense is at issue.

When one speaks of equilibrium, he must, of course, state precisely what is meant by the term. The assertions made here refer to the customary conceptions, in which it is the problem to describe a synchronized, circular process. It is admitted that the economy takes part in strictly periodic fluctuations, that people die and are born; it may even be the economy itself grows, etc. It is clear that for an idea of equilibrium of the type described, perfect foresight would lead to the paradox mentioned. These paradoxes would show in fact that there could be no continuation of a uniform economy. If, however, it is meant that the theory of equilibrium describes only an absolutely static situation, then, one can, of course, establish perfect foresight, for nothing can be changed *ex definitione*, since everything is given as static and unchangeable. If even a single variation should result, then, one could no longer speak of equilibrium nor even of a tendency toward equilibrium, but, rather, the paradox described would result. It is clear that a theory of equilibrium which "explains" only a *static situation, which is given as unalterable* and which, because of this basic assumption, is completely unable to say anything about the economy when a variation occurs, is utterly unimportant

[10] K. Menger: "Das Unsicherheitsmoment in der Wertlehre", *Zeitschrift für Nationalökonomie*, Vol. V. 1934.

[11] It may be well to point out at this time that these variations of expectation and their conditionality are at the basis of *discontinuities* which create repeatedly difficulties in economic theory. Here would be the place for the physical factors in the course of the cycle, stressed, among others, by Pareto, Fanno and Pigou.

from a scientific point of view. It would hardly deserve the names of theory and science.

It would be incorrect to say that constancy of data implies perfect foresight; for it is plain that some completely fixed data might be given and the individual need know nothing of this circumstance. On the other hand, the reverse relationship is valid, since the events which are to be foreseen, could be posited as "given". Only, it is nonsensical to assume that all future events could be given, to which in addition comes the reciprocal estimate of the behavior of individuals.

The following case which takes up a concrete example of economic theory is more important. Consider competition and monopoly theory. In competition (as defined by Pareto), every profit is absent. In monopoly, it is determined as the maximum of a certain function in accordance with limiting conditions. The theory of duopoly, which has recently been the subject of searching studies, clearly indicates that certain assumptions have to be made about whether the duopolist considered regards his partner as one with so-called "quantity" dependency or "price" dependency or *vice versa*. The theory has to be taken as known here. Chamberlin has clearly pointed out that the duopolists reach a maximum profit at the time when both sell at a monopoly price—a situation which corresponds to a tacit agreement.[12] On the other hand, irrespective of the number of suppliers, which means even in the case of duopoly, competitive price will result if each neglects the behavior of the other. So if the two duopolists have sufficient foresight, they will immediately turn to the monopoly price and will make no changes in the equilibrium thus established. If we now assume that we are dealing with, not two, but with a series of 3, 4, 5 . . . . n suppliers, whereby an indefinitely great number of this series satisfies the condition of price competition of the type described by Pareto, then, it is not to be seen why the generally important process of a more or less quick approximation of the monopoly price should not result merely on account of the increase in the number of competitors. If the individuals have perfect foresight—as is still alleged a fundamental assumption for the whole of theory—then, they would retain the monopoly price, for the monopoly profit, divided by the number of sellers, must alwasy secure for every individual a still greater profit of the individual, it would then be greater than zero. Since the business man prefers an ever-so-small profit to no profit at all, the situation is at this point unequivocal. In this connection, if, in a competitive economy, the individuals who do not look ahead perfectly are replaced by others having full foresight, then, apparently, prices must be raised. The fundamental hypothesis of equilibrium theory—that by free competition the cost-price would be paid—is automatically removed. Certainly, a paradoxical conclusion.

To continue, there does not exist a possibility of limiting the process of monopolizing described by Chamberlin directly to a single market, as, for example, that of consumers' goods. Consequently, also the factors of production would only be obtainable at monopoly prices. In this event, the monopoly profit of the last sphere would disappear completely. But if universal monopoly exists, the question arises whether an indeterminate price system does not thereby exist.This indeterminancy, in turn, would have to be foreseen by the individuals, etc. It can, of course, be true that the separate means of production are "private" (in the terminology of Hicks)[13]. Then, this process need not lead to indeterminacy. Likewise, it is necessary that we be informed which kind of foresight is to be assumed with the individual so

[12] E. Chamberlin: *The Theory of Monopolistic Competition*. Cambridge, Massachusetts. 1933.

[13] J. R. Hicks: "Annual Survey of Economic Theory: The Theory of Monopoly", in *Econometrica*, Vol. 3, 1935, p. 4.

that a compact theory may be developed. It has already been indicated that some *positive* degree of foresight must be underlaid. Further, one must remain strictly within the limits of the assumptions, for it cannot be told whether the departure from the Chamberlinian monopoly occurs with three or with ten or only with a thousand competitors; whether that happens to the same degree under all circumstances; and whether it is always equally probable. Finally, it would seem as if the *bare knowledge* of duopoly theory would be sufficient to initiate the process of forming a monopoly just described, which stands in contradiction with the earlier judgment of a difference between perfect foresight and a complete knowledge of theoretical economics on the part of the economic subjects. But no contradiction appears: perfect foresight (with knowledge of duopoly and of oligopoly theory) includes in itself that any individual knows that he masters the duopoly theory and that he is determined to employ it effectively. Insofar this goes beyond the theory, because this knowledge is not a constituent part of it, any more than is the knowledge of exactly what direction the present partner will take and how he estimates his own reactions on it. Consequently, there is no question of a contradiction. The reason why the monopoly process described will not occur *practically* is obvious and need not be stated.

III.

From the whole exposition, it follows that the assumption of perfect foresight is to be cut out from economic theory. It looks as if this would be a completely negative statement, yet this would be unjustified inasmuch as there can be shown simultaneous routes along which the research has to proceed, although these routes are very difficult. In the first place, it follows that all deductions connected with "perfect foresight" are false, or, as long as it is a question of empirical observations, these have no foundation and must be discussed in other connections. Profit is particularly to be considered in this relation, as well as the alleged—repeatedly stressed by J. R. Hicks and accepted by many others—*cleavage of the theory into a general theory of equilibrium and into a special theory of monetary equilibrium*, which may be distinguished from the first, as was mentioned in the beginning of this discussion. It is, then, doubtlessly inadmissible to make this distinction, if it shall stand only on this basis; it might make sense from another point of view. The theory of risk, likewise, requires a new structure; this is evident already from the fact that one of the most interesting and most influential forms of the theory of risk, namely that of F. H. Knight, as is evident from the citation on page 170, stands on wavering grounds. And this applies in even greater degree to the other theories in the field. At the same time, there are promised not only interesting but really very important developments directly by the construction of the theory of risk in which the considerations discussed culminate. This is particularly true because just recently definitely new knowledge of the theory of probabilities has appeared.

It may now be shown in which direction the broader investigation is to proceed: in such a way that it is to be regarded as proven that there are always positive expectations about the future and that these expectations are bound up with a certain degree of foresight. This foresight, again, assumes a certain minimum amount of insight into the economic relationships, since it is clear that there must result variations of expectations—because of *variations* of one or more of these components—which in turn must show in the price structure and, consequently, in the productive structure. We need

only a moment to posit that in an economy, all entrepreneurs have a more far-reaching "foresight" than all consumers (if the distinction is supposed). If we now vary this data, just as we are accustomed to do, for example, with production data, in such a way, for instance, that we just invert the two classes of foresight, then, evidently, a different price system has to result. We cannot give reasons well in advance which are the conditions of stability that have to be erected, apparently. For if the scheme of equilibrium is to be retained, there are not many—but rather very few, probably—kinds of division of different degrees of foresight and expectation on the part of the economic subjects, which are compatible with stable equilibrium.

The problem could be put in the following manner: exactly which division of different degrees of expectation and foresight corresponds to the conditions of equilibrium described by the Walrasian equations? It is a long road until a satisfactory answer may be obtained for this question. Who knows whether the idea of equilibrium can be retained at all? The variations of expectations which one can make quite unequivocally indicate that this factor turns up in the same manner as all the other factors—for example, the changes in supply of the means of production—all of which if varied influence the final result.
result.

Finally, it may be indicated that a great number of empirical studies may have to be made, studies of a very detailed character, in order to obtain some kind of a picture about the range of the element of expectation—the importance of which can scarcely be the same for all markets. It would, for example, be quite conceivable to submit as the adequate data concrete transactions, going on to prove what this result would be, compared with the actual, had different coefficients of expectation been set up. Empirical studies of this kind are certainly possible, but they will require a new technique, since the usual statistical methods fail here because one is dealing at the onset too much with individual cases. On the basis of these empirical studies and by means of the materials of experience, as expressed, for example, in the theory of value, concrete theorems may be handled in such a way that there are discovered expectations and foresight factors, which have been included but generally unexpressed. These verifications partly would coincide with the development which the theory receives through the introduction of the factor of time. Time and expectations are certainly bound up together in intimate fashion. With these investigations, one will have to pay attention to all those questions concerning variations, so that the factor of foresight bound up with every variation does not take part in fluctuations which are to be attributed to other elements. A fruitful example of the introduction of the element of expectation is illustrated by the special theorem of duopoly just discussed.

The results would seem to indicate that there is contained in the theory of equilibrium a variable element, on which the theory is dependent, although its role is as yet unsettled. Consequently, in economic theory falsification exists, the extent of which is still unknown. One might assume this falsification as being not inconsiderable—to say nothing at all concerning the other elements of this sort which, up to the present time, have been disregarded.