

## GENERAL EQUILIBRIUM THEORY - AN EMPIRICAL THEORY ?

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I) Two basic reasons seem to make dealing with the General Equilibrium Theory (GET) attractive for the philosopher of science. First, it is an axiomatic and mathematically demanding theory, a matter that facilitates the business of logical analysis. And second, it seems possible to withdraw from endless hermeneutical debates which often press upon other economic theories and soon lead into the impenetrable thicket of disagreements in economic policy.

GET demonstrates very well that the formal strength of a theory is not necessarily followed by a consensus among scientists concerning its methodological status. Taking a closer look at the GET, it becomes obvious that it is highly disputed in almost every aspect - even among those who support the GET. Some consider it to be a quantitative and empirical theory, but it is admitted that the reference to our economic life is only "in principle" and therefore quite complicated. Others regard it as a "pure theory", but in general this means hardly more than that dealing with it should be taken seriously and valued as "scientific".

In the following I shall first try to reconstruct what economists regard as the aims and methods of the GET and how they hope to reconcile these claims with the actual state of the theory. Subsequently, I shall take a closer look at the methodological problems of the GET - especially at the problem of the utility function - and I want to show how difficult it is to bring into line the current reconstructions made by philosophers of science with the general methodological understanding of the mainstream economists. My last point will be a proposal how one

has to interpret the GET from the viewpoint of philosophy of science in order to get into touch with this general understanding of the economists.

II) What is the usual expectation concerning the achievements of the GET? As the mainstream economists understand it, it should serve as an aid for the explanation of economically relevant situations in decentralized economic systems with private ownership, and it should allow predictions of such situations. Usually one considers as economically relevant all constellations of prices and quantities of goods at the supply or demand side.

To explain an economically relevant situation is to see it as a consequence of individual action. This action is explained by the plans (consumption or production plans) on which it is based. Such an economic plan coordinates possible economic actions and aims under the aspect of optimization; in particular it determines the profit-maximizing supply of a producer or a demand which serves to the optimal satisfaction of the needs of a consumer. The different ways of satisfying needs are beyond the scope of economics; only one point is of interest: at any moment we are exposed to a lot of different needs, so that in a given situation, we are forced to decide which needs will be satisfied and in which order. This decision is expressed by a preference ordering. It is, so to speak, the self-composed report of a person about his state of needs within a given situation. Production and the satisfaction of needs are not seen in a direct relationship. The supply of goods in its qualitative and quantitative composition is only considered as a means to realize an optimal profit. Finally, one speaks of an equilibrium, if the plans for action arrived at independently by the producers and consumers can be realized under certain conditions.

I think there are many reasons to assume that economists use an intentionalist schema of explanation at least within the framework of neo-classical economics. In its simplest form, this schema runs like this:

A intends to bring about p.

A considers that he cannot bring about p unless he does q.

Therefore: A sets himself to do q.

G.H. von Wright has investigated the logical and methodological status of this schema in his famous "Explanation and Understanding" (15) and

I shall later return to it. From such an intentionalist point of view it is quite natural, I think, that an equilibrium situation is of special interest. We call an action well thought-through only if the underlying plan of this action has at least not been unrealizable in like situations, so that in any case equilibrium plans of the past should belong to the set of feasible plans of a consumer.

The history of modern microeconomics is governed by the basic idea that only a quantitative theory is a good theory and so various attempts have been made to show how the state of needs could have a measurable influence on the demand for goods. With some assumptions about the structure of the consumption set and the preference ordering, economists assumed at last to have found quite harmless conditions, which allow the construction of a continuous utility function. By that one has not achieved quantification in a narrow sense; however, it is much more than a purely comparative theory.

But it is particularly the utility theory which makes it problematical to consider the GET as an empirical theory. Unlike in physics, we use all the concepts of economic theory in our everyday life. Nor have these concepts sifted down from the theory into our ordinary language in the process of the scientification of our everyday life. They were part of our ordinary language before all theorizing started, and we use words like "utility" or "preference" daily as a matter of course. The question is now to what degree the theoretical economist is bound to the colloquial meaning of these concepts when he wants to offer with his theories a deeper insight into this very everyday life. The elder theoreticians and most of today's authors of undergraduate textbooks have hardly any difficulties with this problem. It seems quite obvious to them that the semantical core of the economic concepts is made up of their colloquial meaning. So, according to that opinion, a necessary condition for an economic theory to have empirical content is that its concepts, as far as they refer to social phenomena, were first comprehended in their colloquial meaning. Subsequently the theoretician only has to make this meaning more precise. In this sense one would expect e.g. from the concept of the production function, that every process, which is called "production" in economic life, could be represented by this theoretical concept.

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cepts "preference" and "set of commodity bundles", both taken in their colloquial meaning, so that the proposition "person A prefers a commodity bundle  $x$  to a commodity bundle  $y$ " has its colloquial meaning, too. In a first step, the phrase "set of commodity bundles" is made precise by saying that the set of commodity bundles for a consumer  $i$ ,  $i \in \{1, \dots, n\}$  is represented by a non-empty subset of  $R^n$ , the so-called consumption set  $X_i$ , which is closed, convex and bounded from below. The consumption set stands for the objectively existing possibilities of consumption for a consumer. In a second step it is assumed, that every consumer  $i$  has a reflexive, complete and transitive preference ordering  $\prec$  on his consumption set  $X_i$ .

At the transition from the informal speech about needs, preferences and commodities to the formal language of the theory, it is often said, that from now on only a so-called "rational" consumer is the focus of interest. But if the GET should not lose its character as an empirical theory already at the beginning, one has to refer to a conception of rationality, which is specific to a certain culture, especially to our society. And there are some serious doubts, whether the postulates of transitivity or completeness of the preference relation have something to do with the usual meaning of a rational consumption planning. Especially the postulated transitivity has often been criticized by showing that people in choice situations sometimes construct intransitive preference orderings. In our context this argument is only of little interest and with R.J. Aumann (1) I think that a criticism of the postulate of completeness is more fundamental.

As General Equilibrium Theorists say hardly anything about the relation between needs, utility and the consumer's decision, let us assume that consumption plans refer only to goods which are relevant for satisfying someone's needs. Proceeding from this understanding, what could "completeness of the preference ordering" mean? This postulate means that at any moment of decision a consumer is exposed to all those needs, for the satisfaction of which goods are produced. Or, in other words: at any given moment of decision, all feasible commodities are relevant for the consumer. But why should the needs of a single person always cover the whole feasible supply?

Neither do we always have the same needs nor is the process of their differentiation determined by the supply of consumption goods. In saying that a consumer considers only those goods which are relevant for his needs, it should not be ruled out that the basis for the consumer's

decision is formed by a subset of the consumption set. It is often said that those goods which are irrelevant to a consumer can be put at the bottom of his preference ordering. But this would mean that the consumer prefers goods which he doesn't like at all, to those goods, which only are of no interest to him. And with this strange use of the word "preference" one should not say that the GET gives a precise version of something which we deal with only in a vague manner in our everyday life.

III) Of course not all of the economists interpret the utility theory in such a direct and "realistic" manner. There are many, who - influenced by the ideas of the logical empiricism - consider "preference" or "utility" as theoretical concepts, which do not get their meaning by a recourse to the ordinary language, but by connecting them with empirically collected data whereby generally only quantities of goods, prices or income are accepted as those data.

In pursuing that line consequently, one has to determine the demand for commodities dependent on prices and income, as it is done in empirical demand analysis. Here utility functions are sometimes used, too, but wherever this concept appears, it only has to fulfill the heuristic task of specifying the functional relations between market demand, prices and consumer income. The so-called Stone-Geary utility function, for example, makes it possible to develop models of consumer demand, which are linear in prices and income and therefore easily manageable (linear expenditure systems).

I do not want to deal with the well-known objections against this empiristic concept. In our context it is only of interest, that everybody who tries to interpret theoretical concepts in an empiristic manner, has to simultaneously renounce the intentionalist schema of explanation. To make it clear by a comparison: The mainstream economist explains a certain aggregate market demand for goods as a consequence of actions which are adequate means to the ends of the consumers. In the case of an empiristically oriented economist, the aggregate market demand for a commodity is explained by a function which - in the last instance - contains time series data of observed commodity quantities, observed prices and other observed facts which are assumed to influence demand. This we can oversimplify by saying, that the empirically oriented economist stops with his explanation at a point which is exactly the star-

ting point for the explanation of an intentionalistically oriented one.

It seems surprising that despite the obvious difficulties which prevent an evident interpretation of utility theory, hardly any attempts have been made to modify the theory. I think the reason is that economists suspected a breakdown of the essential parts of microeconomics, if the postulates of the utility theory were weakened. Only in the last few years these fears could be dispelled. Along the way a landmark was set by A. Mas-Colell and D. Gale (7), (8), (9). They developed a model which is more general than the usual ones in its assumptions about the production set and the income function, but which, more importantly for the present context, contains the following interesting assumptions: Let  $X_i \subseteq R^n$  be the consumption set of consumer,  $i, i \in \{1, \dots, n\}$ . The consumption sets are closed, convex and bounded from below. Corresponding to consumer  $i$  is a binary and irreflexive preference relation  $P_i$  on the consumption set and for every element  $x_i \in X_i$  the set of elements which are preferred to that element is non-empty and convex. In other words, Mas-Colell and Gale dispense with the postulate of completeness and transitivity of the preference ordering and they only demand non-saturation and the convexity of the "preferred-than"-sets. These weak assumptions are sufficient in order to prove the existence of a market equilibrium.

This result is of great importance and means more than just another variation of the same old tune, because with this model the claim of constructing the GET as a measuring theory (in the weakest sense) is finally abandoned. The remaining assumptions are certainly not trivial, but the important thing for our argument is that the picture of the homo oeconomicus, who is calculating everything, is at least retouched.

It seems to me, that the proposal of Mas-Colell and Gale comes near to a precise version of our common idea of a rational preference decision. But does this mean a rehabilitation of the above mentioned direct and "realistic" view as we can find it in the textbooks, and have we found with this proposal at least a starting point for a consistent interpretation of the GET as an empirical theory? Unfortunately, the answer to this question is negative and we can substantiate this claim with the aid of the conceptual apparatus of the so-called structuralistic view (12), (13), (14).

IV) Within this approach, a fully developed (especially axiomatized)

empirical theory is represented by a quadruple  $K = \langle M_p, M_{pp}, M, C \rangle$ , the so-called core of the theory, which contains the formal structure of the theory in a classificatory manner, and by a set  $I$ , the set of the intended applications of the theory.  $M$  is the set of models of the theory. The elements of  $M$  are endowed with the full conceptual apparatus of the theory and also obey the fundamental laws of the theory. If we cancel the fundamental laws of the theory, we get the set of the potential models,  $M_p$ .  $M_p$  still contains all the concepts of the theory and if we cancel the theoretical concepts, too, we get  $M_{pp}$ , the set of partial potential models. Defining "empirical" as "non-theoretical", we can say that  $M_{pp}$  contains all the empirically describable objects which are "candidates" for being explained by the theory. The constraints  $C$  rule out certain combinations of models or possible models. Or, in other words, the constraints are conditions which have to be satisfied in every application of the theory on a certain level. Finally, the set  $I$  contains both, the applications of the theory which are successful and also those which are merely intended and function as targets for the theoretical efforts. I do not want to go into details, they can be found in the literature. I only want to mention that within this approach a production function, for example, is part of  $M_{pp}$ , while the utility function as a theoretical function (according to Sneed's criterion for theoreticity) is part of  $M_p$ . Whether one has to regard the concept of preference à la Mas-Colell and Gale, for which a colloquial interpretation (that is, an interpretation beyond the scope of the theory) is given, as a theoretical concept or not, still seems an open question to me.

With the distinctions of the structuralistic approach at hand, we can subsume essential parts of the traditional criticism of the GET under only two basic ideas: (1) We can understand one line of the criticism as the claim that the set  $I$  is an empty set for the GET; i.e. there are neither successful applications nor is there a set of merely intended ones, which would be accepted by the equilibrium theoreticians. (2) The other important line can be comprehended as the claim that the constraints of the GET are so strong that no substantial applications could ever be given.

Concerning the question of the intended applications of the GET, two points have to be maintained. First, we must call back to mind that applying the theory-core  $K$  successfully means that we can state an empirically given fact (e.g. a description of the economic situation of the BRD at a certain date), which can be explained by the theory. Bal-

zer (2) has pointed out that in general the core of a theory will not suffice to get claims which are not empirically empty, but that one has to first specialize the core. It is a plain but unfortunately true statement that there are no specializations of the core of the GET, with which reasonable applications could be connected.

Our second point is, that the structuralist conception allows to speak of an empirical theory even when there are no successful applications but when situations can be described which are merely intended for an application of the theory. In that case, one does not yet hold a theory. The theory is, so to speak, just in a state of development leading to a full-fledged empirical theory. But even under this liberal view, we cannot regard the GET as a (quasi) empirical theory. Most economists think that there are connections between the formal parts of the GET and economic reality, but they are unable to name particular situations which could constitute the objects of the theory. And when sometimes such situations are mentioned, then they are only arbitrary restrictions to partial phenomena (e.g. the stock-market).

Coming to the problem of the constraints of the GET, we first have to realize that there are no explicitly given ones, so that it is a matter of interpretation, which assumptions should be taken as constraints. Under the premise that the GET is thought of as a theory of economically acting persons, we can interpret those tacit assumptions as constraints which, on the one hand, determine the static character of the theory, and, on the other hand, concern the information problem of the acting subjects. We can understand then one part of the above mentioned criticism of the GET as the claim, that the GET - though it is thought of as a general analysis of economic action - does not and indeed cannot include the crucial properties of this action just because of these constraints. Four observations have to suffice to substantiate this critical claim.

(1) Let us start with a look at the producer's part in the GET. Here an economically relevant action means to first make feasible production plans and then to single out of this set the profit-maximizing ones. These plans do not have to be connected with a certain date. Because commodities are distinguished by location and date of their availability, it is possible that the theory contains production plans for a longer period. The static property of the theory comes into play in another way: within the Arrow-Debreu world nothing may disappear or be added.

This is the first and most fundamental constraint. It implies, for example, that the static profit maximization criterion assumed to characterize the producer does not take into account expenditure on capital goods or investment in plant or equipment at all (by the way, the static utility maximization criterion assumed to characterize the consumer does not take into account savings and the accumulation of earning assets at the expense of deferring consumption). As in the GET production is not understood as a recurring process but as a single event, not even replacement costs from depreciation can be considered. And what is called "profit maximization" in the GET framework may be understood at best as the formulation of a short-termed liquidity aim. As such it is a subordinate aim in the hierarchy of aims which derives its significance only by combination with other aims of the producer.

(2) The next constraint is best illustrated on the consumer's side. In the GET, an interpersonal comparison of utility is not required and with that no standardised measure of utility. It is thought to be sufficient, that every consumer builds up a preference ordering of his own. We all know from everyday life that in some cases our preferences are fixed, maybe even for our whole life, yet in others our preferences are constantly changing. If one would take this experience into account, one ought to reduce the demand for permanence to a minimum and only postulate that the preferences should remain constant for a very short period. But then, a utility function facilitates neither an interpersonal nor an intertemporal comparison of utility, and so even in situations which coincide in all decision-relevant parameters, we cannot say that the demand of a consumer would be the same. So, to give a pragmatial reason for the theoretical efforts, one has to renounce the everyday experience and postulate that in all applications of the theory constantly changing preferences are ruled out.

(3) At this point now we have to mention the role of prices and money within the GET. The prices of commodities are not explained by the theory. "With each commodity, say the  $h^{\text{th}}$  one, is associated a real number, its price  $P_h$ . This price can be interpreted as the amount paid now by (resp. to) an agent for every unit of the  $h^{\text{th}}$  commodity which will be made available to (resp. by) him" (3, p.32). That's what Mr. Debreu has to say.

This proceeding is justified by saying that all agents are price takers, which means that nobody has the possibility of influencing the

formation of the market-price by his decisions. Nevertheless, the fundamental problem remains: to show that a market clearing price-vector may exist does not mean to show that a mysterious price-setting mechanism is also able to produce such a price-vector. It is possible that the rules, according to which prices are scheduled, may prevent reaching an equilibrium price. According to the static GET this cannot be excluded. It is obvious that in such a world there is no room for the manifold utilizations of money we are acquainted with. In the GET money can only be considered in its trivial function as numeraire.

If you talk to a neoclassical economist about such objections, he will probably reply that one has to see the static GET only in connection with the tâtonnement-process as the core of the dynamic theory of price. The tâtonnement-process is constructed as  $p = z(p)$ , where  $p$  is an element of the normalized price simplex  $P = \left\{ p \in \mathbb{R}_+^n \mid p \gg 0, \|p\| = 1 \right\}$ , and  $z(p)$  is an aggregate excess demand function of price  $p$ , satisfying the following conditions: a)  $z(p)$  is continuous and twice differentiable, b) homogeneous of degree zero and c) follows Walras' Law. But even this "dynamic" tâtonnement-process is static in many essential points: the number of market-participants must be kept constant, of course nobody is allowed to produce or to consume, and trading is allowed only if equilibrium prices are achieved. The so-called non-tâtonnement-models indeed allow a recontracting and so renounce the last assumption, but the work of F.M. Fisher demonstrates in a striking manner how extremely difficult it is to make some progress in this field (4), (5).

(4) The last of the tacit assumptions which can be understood as a constraint concerns the information which economic agents have at their disposal: in every application of the theory the consumers and producers should be informed equally and completely about all decision-relevant facts. This so-called "axiom of complete information" (which usually is not explicitly given) is dealt with comprehensively in the literature, and there is extensive agreement that this axiom mainly prevents the development of a reasonable concept of money and of an adequate understanding of the firm as it actually exists in the framework of the GET.

If one considers all these difficulties, the idea is near at hand to give up the claim that the GET is an empirical theory at all. And indeed, there are many economists, who consequently regard the GET as inappropriate for the formation of the fundamental concept of empirical microeconomics. In their understanding, a microeconomic theory is a meaning-

on the ability of reflection of the interviewed person. If you have a lasting and close relationship with a person, you are able to judge the adequacy of his statements by talking and acting with each other. Under such conditions you are able "to get to know somebody", as we are used to say colloquially. But this is not the normal situation for which explanations of actions are demanded. In the "normal" situation you do not have a direct way to judge subjective statements, and what is even worse, often you know only about the action or the consequences of this action without having any knowledge at all about the subjective statements of the agent.

In such a case, we can only establish a reasonable contact with the agent if we impute that his doing is not pure behavior but an argumentatively prepared action. Only under this premise it makes sense to talk to somebody about his doing or to enter into negotiations about a future performance or omission. Even the question, what he is intending in particular with his doing rests on the imputation that he is intending something at all. For example, the question "why do you blink your eye?" makes sense only if one assumes that this blinking is not just a common reflex movement.

So, applying the intentionalist schema does not require a distinction between action and behavior which is independent of the schema. Whether we have to establish contact with others simply as a person or in the role of an empirical social scientist: in each case it is reasonable to impute that the others have reasons for their doing, i.e. that their observable doing is founded on an argumentative preparation. We have to comprehend the doing as behavior only if this imputation proves a failure. So within this view, behavior is not a central category of the social sciences, but only a remnant (for details see 10, 11).

The imputation that doing is argumentatively prepared raises only few problems when there are commonly accepted reasons for acting in certain situations in certain ways, that is, if this doing is determined by social norms and rules. By the way, I understand a norm to be an imperative which urges all members of a group to bring about the situation  $S_1$  in a certain situation  $S_0$ ; a rule urges all members of a group to execute a certain action A in a certain situation S. These norms and rules determine the connection between aims and means in a social context, and so they represent social rationality. It is important to maintain that we do not get these norms and rules as statistically assured regulari-

ties of behavior, that we do not obtain them by observing what persons are actually doing in certain situations.

There are essentially two ways in which we gain knowledge of socially relevant norms and rules. First, as members of society and some of its subgroups we learn some of these norms and rules through the practice of life. Education, school, vocational training make us familiar with the relevant regularities of our spheres of life. We learn how to judge their relevance by the success and failure of our own practice. The knowledge of norms and rules of spheres which do not belong to our own spheres of life is gained by communication. This becomes evident for example in the training of managers, where the communication of such norms and rules via textbooks and case-studies plays an important role. Especially the case-studies are not a mere didactical aid. More importantly, in a paradigmatical manner they transfer how a "good" manager has to act (or should not act) in a certain situation. In addition to all that, the knowledge of social norms and rules is a precondition for being able to identify social actions (just think of actions like "buying", "selling", "leasing", "issuing bills", etc.) and to determine deviant behavior.

The knowledge of single norms and rules allows an identification and interpretation of actions "from the distance", and by collecting them systematically, we can get a general view of the institutional framework that is given for the expression of individuality as an entrepreneur or a consumer for example. Those norms and rules which are obligatory for the economic actions of all entrepreneurs, all consumers, workers and the government form the economic system ("Wirtschaftsordnung") of a society. But the knowledge of the norms and rules of the economic system is not only a precondition for understanding economic actions of individuals, this knowledge is also necessary for judgements within the context of economic policy. Because when we are talking about efficiency or economic justice in a political context, we normally do not think of the actions of single persons, but rather of the norms of the economic system.

I think it is already obvious how I want to interpret the GET: The GET should be an analysis of the functioning of the economic system. It should not have the de facto economic actions for subject, but the de facto norms and rules, according to which economic actions have to be performed. To make this idea a little bit clearer, let me give an ex-

ample. As I have mentioned, it seems to me that the proposal of Mas-Colell and Gale comes close to a precise version of our common idea of a rational preference decision. With that I do not mean that we are always aware of these postulates in our everyday life. Of course, we often act from habit or commit errors. But even in these cases we have an idea of rationality which allows us to comprehend our actions as successful or erroneous in retrospect. So we should not understand the proposal as a claim about de facto actions, but as a claim about a de facto accepted pattern of rationality, which serves as a common base for judgments on actions.

In analyzing an economic system, one of the first questions will be whether the norms are consistent, i.e. whether it is possible to define compatible aims within this institutional framework of action. And this would be the new formulation of the question of economic equilibrium: does our economic system allow the formulation of mutually compatible aims of action, and are there institutions which enforce the compatibility of initially incompatible aims?

This question is meaningful, regardless of whether the economic subjects are acting according to the norms. Because if the answer would be no, this should, for example, have political consequences. And if somebody really cannot do without talking about possible worlds, he could say that the model of an economic system represents the limits of possible economic actions. The central purpose of such an "institutionalist" GET would not be to make possible the prediction of economic actions, but to lay the foundation for judging the economic system. But in an indirect manner, the institutionalist view is a prerequisite for an analysis of de facto economic actions: only against an institutional background can a deviant action be identified and comprehended as reasonable in so far as it represents an answer to a certain failure of the institutional order.

By the way, I think that the suggested step from an analysis of individual economic action to an analysis of the institutions which govern this action is not very revolutionary. If we look at economics as a whole, we shall find that an institutionalist element has been around since the days of Walras up to the modern literature about industrial structures.

Of course, I do not want to claim that the GET in its present state is

a fully developed theory of economic institutions; the defects are too obvious. But on the other hand, under this view the above mentioned constraints will lose their significance, and I suppose that one will be able to disclaim constraints (2) and (4). For this reason, I think the GET could be understood as a germ-cell of a general theory of economic institutions which in its present state serves at the least as a pattern for theory construction within the framework of neoclassical economics.

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