

# Model Platonism: Neoclassical economic thought in critical light

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**Abstract:** This article is a translation of the original German text of Hans Albert's 1963 article titled 'Modell-Platonismus. Der neoklassische Stil des ökonomischen Denkens in kritischer Beleuchtung', in F. Karrenberg and H. Albert (eds.), *Sozialwissenschaft und Gesellschaftsgestaltung – Festschrift für Gerhard Weisser*, Berlin: Duncker und Humblot, 45–76. The enduring relevance of Hans Albert's critique together with an introduction to the author as one of Germany's most distinguished philosophers of science can be found in Arnold and Maier-Rigaud (2012) also in this volume.

## 1. Introduction

Those who view theoretical economics as an empirical science – and the majority of modern economists will be included here – will generally come to the conclusion that, in the final analysis, all of the phenomena that it investigates can be traced back to human behavior, to the opinions, decisions, and actions of individuals. The theory speaks of economic subjects, who appear in their capacities as consumers, employees, holders of assets, businessmen, etc. and behave in accord with certain perspectives, which are important to investigate. The issue is thus the action of certain social role players, and economics cuts out a segment of the role structure of society that it views as economically relevant and able to be analysed under reasonably unified theoretical points of view.<sup>1</sup>

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1 T. W. Hutchison long ago drew our attention to the fact that all economic problems are in principle concerned with questions of the behavior of people in different established positions, i.e. of social

Because market phenomena have been the focus of economic thought since the emergence of modern economics, it is primarily market-related roles and role segments of the social structure that are taken into consideration. The conceptual tools of economics have been market oriented from the outset.<sup>2</sup> Anyone who analyses the logical grammar of economic terminology thus continually runs up against social relationships of a particular nature, namely commercial relationships between individuals and social structures, as they are constituted in market behavior, as basic objects. In light of the way it formulates problems, it is not entirely unwarranted to interpret economics in part as sociology,<sup>3</sup> and indeed primarily as a sociology of commercial relationships.

Even though it is relatively easy to determine the sociological character of economic problems, it is still not possible to say that theoretical economics has reaped the benefits of research from general sociology or from the closely related discipline of social psychology. Neither is this to be expected as long as theory formation is dominated by neoclassical thought, which channels economic analysis so as to abstract as much as possible from social facts. The field of market relationships, which it thereby aims to get a handle on, then appears to be a relatively autonomous area of social life, which can be dealt with in isolation from these other areas; and strangely, beyond that, the factors that otherwise tend to be drawn on to explain social behavior appear to play no considerable role. To theoretically apprehend market behavior and the commercial relationships of people and social groups, it does not seem to be necessary to dig deeper into motivational or institutional problems. Neither the social milieu of the individuals and groups active in the market, the internal structure of these groups, the cooperation among them, their will-formation, the motivational structures, nor the attitudes or value-orientations of the individuals appear to be relevant for the behavior of economic units, unless they are expressed with sufficient precision and generality in accord with a behavioral maxim: the neoclassical assumption of maximization or other simple reaction functions.

Theoretical economics of the neoclassical variety thus appears quite oddly to combine sociological problems with relatively sociology-free solutions. It appears to be immune to the intrusion of sociological and social-psychological knowledge. How can sense be made of this? Need the results of other

role behavior; see his methodological paper, 'Theoretische Ökonomie als Sprachsystem', *Zeitschrift für Nationalökonomie*, VIII, 1937, 88. [Hutchison, 1937]

2 In this respect, there was an essential difference from the old European economics, oriented on matters at home, worked out by Otto Brunner; see also his book, *Adeliges Landleben und europäischer Geist*, Salzburg, 1949, 240ff. and passim), and 'Die alteuropäische Ökonomik' *Zeitschrift für Nationalökonomie*, XIII, 1952, 114ff. [Brunner, 1949, 1952]

3 Especially Gerhard Weisser has pointed to the sociological character of theoretical political economics. For example, see also my papers, 'Marktsoziologie und Entscheidungslogik', *Zeitschrift für die gesamte Staatswissenschaft*, 114, 1958, and 'Nationalökonomie als Soziologie' *Kyklos*, XIII, 1960. [Albert, 1958a, 1960]

social sciences be presumed to be irrelevant for the economy? Or can this peculiarity of economic thinking perhaps be brought into connection with certain idiosyncrasies of the methodological approach that is often affiliated with it? It seems to me that there is much to be said for the latter of these two possibilities. To show this, a few general preliminary comments of a methodological nature are first needed.

## 2. Reference to reality, informational content and truth

In everyday situations, if, in answer to an inquiry about the weather forecast, one is told that the weather will remain the same as long as it does not change, then one does not normally go away with the impression of having been particularly well informed, although it cannot be denied that the answer refers to an interesting aspect of reality, and, beyond that, it is undoubtedly true. An answer so extremely and obviously lacking in content will indeed normally only be offered as a joke, so it seems moot to take this kind of case into consideration. Nevertheless, it is perhaps worthwhile to relate a few general considerations to a case of this sort, for it particularly clearly displays a matter of fact that can play a certain role in less obvious and less extreme cases. It shows that it is possible to speak about reality, even to make true statements about it, without saying anything about it; in this case, that means, without providing any information.

In statements or sets of statements, it thus may be useful to differentiate very generally between *reference to reality*, *informational content*, and *truth*. For one, we can ask whether and in which way the respective statement or set of statements – for example, a theory – is related to reality;<sup>4</sup> for another, we can ask whether and to what degree it provides information about it,<sup>5</sup> and if applicable, which information it contains; finally, we can ask whether this statement or set of statements is ‘true’, that is, in our case, whether it corresponds with the constitution of reality, or more precisely still, with the aspect of reality under consideration. We are not normally interested merely in the truth of a statement, nor merely in its relation to reality; we are fundamentally interested in what it says, that is, in the information that it contains. As our trivial example shows, it is extremely easy to make assertions that are related to an interesting aspect of reality, and that are true, if we are willing to forego information. We need only make statements that are true for reasons of logic – analytic statements, as they are often called.<sup>6</sup> Given that they preclude no conceivable situation, they have

4 Here, for example, it may be that there is a prescriptive (for example, an evaluative or normative) relationship to reality, not a descriptive one. However, in the context treated here, this is of no particular interest.

5 Analogously to the informational content of cognitive statements, one could say that normative statements refer to a class of behavior that is excluded by the respective normative (prescriptive) statement.

6 For this, in this case – when concerned with the area of economics – substitutional cases of logical laws constructed in economic language come into question, but so do statements that are able to be traced

full latitude. They are formulated such that they will conform to anything that can happen in the world. The same applies to those statements that, while not being analytically true, are stipulated so as to possess a certain unlimited 'alibi'.<sup>7</sup>

This already provides us with the decisive difference from informative statements that provide information about reality by virtue of precluding certain possible matters of fact (situations, events, processes, etc.), and that consequently must be viewed as refuted if these in fact do occur. Information can only be obtained by limiting logical possibilities; and this in principle entails the risk that the respective statement may be exposed as false. It is even possible to say that the risk of failure increases with the informational content, so that precisely those statements that are in some respects most interesting, the nomological statements of the theoretical hard sciences, are most subject to this risk.<sup>8</sup> The certainty of statements is best obtained at the cost of informational content, for only an absolutely empty and thus uninformative statement can achieve the maximal logical probability.

The nomological hypotheses (laws) characteristic of general theories often have the character of general hypothetical statements (conditional propositions, if-then statements) or they can be brought into this form with no difficulty. It is possible in this case to differentiate between the content of both components of the hypothesis (its if- and its then-clause) and the content of the hypothesis itself, and to determine the following relationship: with an increase in the content of the if-clause of such a statement, the content of the statement itself decreases, while it rises with the increase in the content of the then-clause, both under the presupposition that the content of the respective other component does not change.<sup>9</sup> This relationship can become important in judging

back to these with the help of definitions. Beyond that, definitions themselves can be viewed as analytically potent stipulations. The general assault of American neopragmatism on the analytic-synthetic distinction (Quine, Goodman, White) has proven to be questionable. On the problematic of empty formulas, see, especially, the work of Ernst Topitsch, particularly his books *Vom Ursprung und Ende der Metaphysik*, Vienna, 1958, and *Sozialphilosophie zwischen Ideologie und Wissenschaft*, Neuwied, 1961, as well as his article 'Über Leerformeln. Zur Pragmatik des Sprachgebrauches in Philosophie und politischer Theorie', in *Probleme der Wissenschaftstheorie. Festschrift für Viktor Kraft*, edited by E. Topitsch, Vienna, 1960. [Topitsch, 1958, 1960, 1961]

<sup>7</sup> This expression is from G. C. Archibald, a defender of the conception of modern methodological theory in economics. T. W. Hutchison, who has contributed to this area for more than twenty years, called attention to the significance of *ceteris paribus* clauses as a form of alibi in the above-mentioned article (see, e.g., 85f.). In relation to our example, one could also use the alibi clause '...under given conditions...'. [Hutchison, 1937]

<sup>8</sup> Karl Popper dealt in detail with these contexts, which I can only allude to here, in his book, *The Logic of Scientific Discovery*, London, 1959 (first published in German as *Logik der Forschung*, Vienna, 1935). Compare also his paper, 'Some Comments on Truth and the Growth of Knowledge', in *Logic, Methodology and Philosophy of Science*, edited by E. Nagel, P. Suppes and A. Tarski, Stanford, 1962. [Popper, 1959, 1962]

<sup>9</sup> For these contexts, see especially Karl Popper, *The Logic of Scientific Discovery*, 121ff. (36. 'Levels of Universality and Degrees of Precision'). This can perhaps clarify that additional qualifications in the

theoretical statements, for example, as will also be shown, for the question of the interpretation of *ceteris paribus* clauses.

As the informational content of a statement increases, its general testability and verifiability also increase, for with the decrease in the scope of the statement, the possibility to test it increases; the possible strength of the testing increases so that the respective statement (set of statements) can be subject to a higher risk of failure. This also results in an increase in the possibility of verifying it on the basis of facts.<sup>10</sup> The degree of verification of a statement or a set of statements can be made dependent on the strength and the outcomes of the testing to which it was subject. We are in fact not strictly able to determine the truth of informative statements, especially when these are nomological hypotheses that claim validity for arbitrary spatiotemporal spheres, but we can at least attempt to subject them to a high risk of refutation (*Scheiterns*) by the facts so that we have some grounds for accepting them for the time being, as long as the results of tests related to them are positive.<sup>11</sup>

If theoretical statements are empirically re-examined, there is a tendency to apply the respective theory in a certain way to concrete situations, and that means, among other things, that one draws relevant logical consequences from them for the respective conditions of the application. Here it is important to know that, in the course of the logical deduction, there can be no increase in the informative content of the statements under question.<sup>12</sup> Through the process of deduction – that is, through logically permissible transformations – it is never

then-clause only specify the relationships predicated there – e.g., a form of movement or a behavioral pattern – while additional qualifications in the if-clause only tighten the conditions under which the relationships predicated in the then-clause are valid, and thus the area of validity; and they usually also limit the special temporal area of validity of the hypothesis. These things may have some significance for the problematic of the ‘presuppositions’ of the political economy, which can often be interpreted as components of the general predication of the if-clause. As we will see, one such presupposition, for example, is the *ceteris paribus* clause. By the way, here it is pointed out that the word ‘assumption’ tends to be applied in diverse ways in political economics. [Popper, 1959]

10 For more on this, see the work of Karl Popper, above all the above mentioned book. In certain respects, one can compare a scientific theory with a car that is subjected to the greatest possible strain in tests. If it can successfully get through the tests, then it has proven itself; and in fact, the greater these strains were, the more it has proven itself. A car that can withstand greater strains consequently performs better. So too, a theory that can withstand more rigorous tests performs better; it has more explanatory power than other theories. That applies, for example, to theories that prove themselves not only in one social area but in diverse ones. This yields certain arguments against the tendency to prefer theories in the social sciences that are tied to particular areas, such as the claim for the autonomy of economic thinking, above all, in neoclassicism. Compare this with my critique in the above mentioned article, ‘Political Economy as Sociology’, l.c. 5. [Albert, 1959a; Popper, 1959]

11 We thus must differentiate between the *relation to reality*, the *informational content*, the *degree of proof*, and the *truth*, whereby it is to be noted that for informative statements, i.e., hypotheses, the truth remains problematic. The methodology of the empirical sciences is thus primarily concerned with the problem of provability.

12 See also, for example, Rudolf Carnap, *Einführung in die symbolische Logik*, Vienna, 1954, 20, or another book of modern logic. [Carnap, 1954]

possible to pull more information out of a set of statements than they contain. Consequently, one can be certain that only true conclusions are derived from true premises, presuming of course that there are no breaches in the logic. The logical deduction guarantees to a certain extent the transfer of positive truth value, of the truth, to the derived statements. Besides that, it can be shown relatively simply that the falsity of a deduced statement yields the falsity of at least one of the premises. It is thus possible here to speak of the re-transference of negative truth value.<sup>13</sup> Practically this means, for example, that it is possible to refute a theory if one is able to refute its consequences. This correlation is continually used in the empirical testing of theories.

### 3. Model Platonism: the application of conventionalist strategies in economic thinking

#### 3.1. Immunization against experience as a neoclassical tendency

The connections sketched out above are part of the general logic of the sciences and can thus be applied to the social sciences. Above all, with their help, it appears to be possible to illuminate a methodological peculiarity of neoclassical thought in economics, which probably stands in a certain relation to the isolation from sociological and social-psychological knowledge that has been cultivated in this discipline for some time: the *model Platonism* of pure economics, which comes to expression in attempts to *immunize* economic statements and sets of statements (models) from experience through the application of *conventionalist strategies*.<sup>14</sup> That need by no means be the intention of the respective theoretician, although there is an array of well-known economists who explicitly hold an *a priori* methodological view or who for one reason or another at least view the empirical examination of economic theory as superfluous.<sup>15</sup> The neoclassical style of thought – with its emphasis on thought experiments, reflection on the

13 On the significance of the question of the transferability or the re-transferability for the problem of rationality, see also William W. Bartley, 'Rationality versus the Theory of Rationality', in *The Critical Approach to Science and Philosophy. Essays in Honor of Karl Popper*, edited by Mario Bunge, Glencoe, 1964. [Bartley, 1964]

14 I have touched upon this subject in a discussion entitled 'Der logische Charakter der theoretischen Nationalökonomie' *Jahrbücher für Nationalökonomie und Statistik*, 171, 1959, 1ff. Since then I have been able to determine that the tendency to 'model Platonism' has decreased in German-speaking countries. Another impression may largely rest upon the fact that there is a tendency, on the basis of a statement's relation to reality, to draw the conclusion that it has informative content. On the problem of models, see also Hans Anger, 'Theoriebildung und Modelldenken', in *der Kleingruppenforschung, Kölner Zeitschrift für Soziologie und Sozialpsychologie*, 14, 1962, 4ff. [Albert, 1959b; Anger, 1962]

15 Besides Ludwig v. Mises, Frank H. Knight, for example, is included here, as is clear from his controversy with T. W. Hutchison; see also T. W. Hutchison and Frank H. Knight, in *The Journal of Political Economy*, XLIX, 1941, 732. Lionel Robbins' views also appear to be close to the conception of Mises. That is clear from his interesting book, *An Essay on the Nature and Significance of Economic Science*, 2nd edn, London, 1952, passim. In my view, a passage of this book that is also characteristic of the methodological conception of other thinkers is: 'It is a characteristic of scientific generalisations

basis of illustrative examples and logically possible extreme cases, its use of model construction as the basis of plausible assumptions, as well as its tendency to decrease the level of abstraction, and similar procedures – appears to have had such a strong influence on economic methodology that even theoreticians who strongly value experience can only free themselves from this methodology with difficulty. Hereby, through conventionalist procedures, theories that certainly entail interesting ideas are often rendered insensitive to the facts and thus rendered useless. In other social sciences, too, model Platonism also may often stand in the way of useful theory formation.<sup>16</sup> It appears, however, to be fairly certain that because of their methodological tradition, this is a danger to which representatives of the economic sciences succumb especially easily, particularly as long as they remain unconscious of it.

If one tries to gain an impartial overview of the development of economic thought, then, under certain circumstances, one may be impressed by the great number of errors and misunderstandings to which even the most famous and penetrating thinkers succumb. But this stand of things is by no means exceptional or maybe even regrettable. A horrible error can even be more significant for the growth of knowledge than a trivial truth. Science progresses through the gradual elimination of errors from a large offering of rivalling ideas, the truth of which no one can know from the outset. The question of which of the many theoretical schemes will finally prove to be especially productive and will be maintained after empirical investigation cannot be decided *a priori*. Yet to be useful at all, it is necessary that they are initially formulated so as to be subject to the risk of being revealed as errors. Thus one cannot attempt to preserve them from failure at every price. A theory is scientifically relevant first of all because of its possible explanatory power, its performance, which is coupled with its informational content.

### 3.2. *Remarks on the law of demand*

In the area of microeconomics there is an array of examples that can demonstrate the possibilities of model Platonism. For the sphere of consumption goods, the

that they *refer to reality*. Whether they are cast in hypothetical or categorical form, they are *distinguished from the propositions of pure logic and mathematics* by the fact that in some sens[e] *their reference* is to what exists, or that which may exist, rather than to purely formal relations.’ (my emphasis, *Ibid.*: 104). V. F. Robbins appears to view the relation to reality as the essential distinguishing characteristic. This would then open the possibility of viewing the substitution cases of logical laws as economics statements. This interpretation of these lines is in fact not incontestable, but it is in any case in harmony with the basic tendency of his book. Elsewhere I have already attempted to show that strong tendencies of this sort can be found in Walter Eucken. In German-speaking countries, his methodological conception has many adherents. [Hutchison, 1941; Robbins, 1952]

16 For more on this, see the remarks from René Königs in ‘Grundlagenprobleme der soziologischen Forschungsmethoden (Modelle, Theorien, Kategorien)’, in *Sozialwissenschaft und Gesellschaftsgestaltung. Festschrift für Gerhard Weisser*, edited by F. Karrenberg and H. Albert, Berlin, 1963. [Königs, 1963]

*law of demand* is an essential component of the theory of consumer market behavior. With this law, a specific procedural pattern of price-dependent demand is not postulated, that is, a certain demand function, but only the general form that such a function ought to have. The quantity of the good demanded by the consumers is namely characterized as a monotone-decreasing function of its price.<sup>17</sup>

The law appears *prima facie* to predicate a relatively simple and easily testable relationship and thus to have a fair amount of content. However, upon closer examination, this impression fades. As is well known, the law is usually tagged with a clause that entails numerous interpretation problems: the *ceteris paribus* clause. In the strict sense this must thus at least be formulated as follows to be acceptable to the majority of theoreticians: *ceteris paribus* – that is, all things being equal – the demanded quantity of a consumer good is a monotone-decreasing function of its price.<sup>18</sup> The *ceteris paribus* clause is not a relatively insignificant addition, which might be ignored. Rather, it can be viewed as an integral element of the law of demand itself.<sup>19</sup> However, that would entail that theoreticians who interpret the clause differently *de facto* have different laws of demand in mind, maybe even laws that are incompatible with each other. A law tagged with this clause can be viewed as a general hypothetical proposition,<sup>20</sup> to a certain extent as an always-and-everywhere if-then clause. The *ceteris paribus* clause would accordingly belong to the if-clause of the respective law of demand; the proposition about the general form of the demand function, by contrast, would belong to the then-clause. The entire law would thus have something of the following schematic structure: If such and such conditions remain the same, then the demand function has such and such a general character.

17 For a clear and precise presentation of the main tenets of this problematic, as it is largely accepted today, see also J. R. Hicks, *A Revision of Demand Theory*, Oxford, 1956; on methodological questions in connection with this, see Tapas Majumdar, *The Measurement of Utility*, London, New York, 1958; for a critique of the utility theory-based substructure, see also E. J. Mishan, 'Theories of Consumer's Behaviour: A Cynical View', *Economica*, XXVIII, 109, 1961, 1ff. [Hicks, 1956; Majumdar, 1958; Mishan, 1961]

18 This formulation is not yet acceptable to the majority of people because it does not take the Giffen paradox into consideration. This paradox, which we can abstract from here, might entail further problems.

19 T. W. Hutchison already referred to this in the above-mentioned article. See also his book on methodology, *The Significance and Basis Postulates of Economic Theory*, London, 1938; reprint New York, 1960, 40ff. [Hutchison, 1938]

20 In logic such statements are called formal implications, but this language use may be a little unfortunate, just as is terming molecular bonding rates as material implications, because in both cases, this too strongly suggests that these are matters of a deductive relationship, a logical implication. Anyway, there are economic theoreticians who expect hypothetical propositions of a theory to have the character of logical implications; see also, e.g., Walter Eucken, *Die Grundlagen der Nationalökonomie*, 5th edn, Godesberg, 1947, 411, footnote 54, as well as my critique in the article, 'Der moderne Methodenstreit und die Grenzen des Methodenpluralismus', *Jahrbuch für Sozialwissenschaft*, 13, 1962, 161f. Hutchison's above-mentioned book contains a critical analysis of this conception. For an analysis of the character of nomological propositions, see especially Appendix \*X, 420ff. of Popper's *The Logic of Scientific Discovery*. [Eucken, 1947; Popper, 1959; Albert, 1962a]



Here it seems advisable to return to our general observation about the informative content of hypothetical statements. We determined that the content of the then-clause changes in the same direction as the content of the general proposition, and the content of the if-clause changes inversely to it. Bringing this to bear on our law of demand, the consequence is that, under the presumption of the same then-clause, the difference in the informational content in a certain respect depends on a different interpretation of the *ceteris paribus* clause. If the factors that are to be left constant remain undetermined, as not so rarely happens, then the law of demand under question is fully immunized to facts, because every case which initially appears contrary must, in the final analysis, be shown to be compatible with this law. The clause here produces something of an absolute alibi, since, for every apparently deviating behavior, some altered factors can be made responsible.<sup>21</sup> This makes the statement untestable, and its informational content decreases to zero. This is a classical case of the use of a conventionalist strategy.

One might think that it is in any case possible to avert this situation by specifying the factors that are relevant for the clause. However, this is not the case. In an appropriate interpretation of the clause, the law of demand that comes about will become, for example, an analytic proposition, which is in fact true for logical reasons, but which is thus precisely for this reason not informative. This of course applies to any interpretation that makes the then-clause of the law of demand under question a logical consequence of its if-clause so that, in this case, an actual *logical implication* results. One might think that examples of this sort of interpretation must be constructed *ad hoc*; but that is not necessary. One example of this sort occurred not long ago.<sup>22</sup> Here, through an explicit interpretation of the *ceteris paribus* clause, the law of demand is made into a tautology.

Various widespread formulations of the law of demand contain an interpretation of the clause that does not result in a tautology, but that has another weakness. The list of the factors to be held constant includes, among

21 T. W. Hutchison determined in his book (p. 41) that the purpose of the clause is usually to limit the falsifiability of the law – i.e. its informative content. ‘Falsifiability’ is used here in the sense that Popper applies the term. [Hutchison, 1937]

22 See also Sidney Weintraub, ‘The Foundations of the Demand Curve’, *American Economic Review*, XXXII, 1942, 538ff., where Marshall’s demand curve is interpreted such that, in the *ceteris paribus* clause, the entire spending for all goods and the prices and amounts of all other goods turn up. If one presumes the constancy of these factors, not only does the common form of the demand function logically follow from this, but even a logically much stronger then-proposition, which ascribes the demand curve with a right-angled hyperbola. In the interpretation of the clause proposed by Weintraub, the informational content of the if-clause is even greater than that of the then-clause of the full proposition. In any case, this special interpretation leads to a tautology, as Milton Friedman saw; see his article ‘The Marshallian Demand Curve’, in *Essays in Positive Economics*, Chicago, 1953, 47ff. [Weintraub, 1942; Friedman, 1953b]

other things, the structure of the needs of the purchasing group in question.<sup>23</sup> This leads to a difficulty connected with the identification of needs. As long as there is no independent test for the constancy of the structures of needs, any law that is formulated in this way has an absolute 'alibi'. Any apparent counter case can be traced back to a change in the needs, and thus be discounted. Thus, in this form, the law is also immunized against empirical facts. To counter this situation, it is in fact necessary to dig deeper into the problem of needs and preferences; in many cases, however, this is held to be unacceptable, because it would entail crossing the boundaries into social psychology.<sup>24</sup>

In connection with the law of demand, other questions would of course also be considered, which are connected, for example, with the static character of this law, with the possibility of interpreting it subjectively or objectively, and with its alternative analytic structure. Here the point has only been to illustrate this peculiarity of neoclassical thought.

### 3.3. *The problematic of the quantity theory of money*

In the theory of money, quantity theory belongs to the traditional teaching corpus. Recently, it has been displaced by other theories, which tend to be presented as having greater explanatory power. In its classical form, the quantity theory of money declares, for example, that a change in the volume of money is always connected with a change in the level of commodity price in the same direction. Sometimes even a strict proportionality between these two changes has been presumed. Under the common presupposition that the expressions 'price level' and 'amount of money' are defined independently of one another, 'naïve' quantity theory, that is, the claim of the proportionality of both movements, is a rather informative proposition. To test it, a sufficiently precise definition of both factors is required, and, in connection with this, useful indicators for it must be identified. Unfortunately, this theory has not proven to be successful, consequently it has been necessary to resort to a less demanding form of it. In the course of the development of economic thought, this form, too, has been abandoned in favor of what is known as the quantity or exchange equation, which maintains that the product of the amount of money and speed of money flow is identical to the product of the trade volume and the price level. However, as it is normally interpreted, this equation is analytic; thus the transition from

<sup>23</sup> This also applies, for example, to the interpretation suggested by Friedman; see the above-mentioned article. [Friedman, 1953b]

<sup>24</sup> The development of voting theory has led to the de-psychologization of utility theory. Many theoreticians viewed this as a positive development. With this, the theory of demand freed itself from the problem of needs and the satisfaction of needs, which was originally thought to be interesting, but which earlier also tended to be treated more speculatively and introspectively. In place of these problems, questions have arisen that are less suited to the context of an empirical theory of the motivation of behavior than to a formal theory of rational decision-making. For economics, as an empirical science, thus as a partial sociology, the value of this development appears to me to be rather doubtful.

the old quantity theory to the equation of exchange results in a tautology, and consequently a decrease in the informational content to zero, something which has by no means been noticed by all theoreticians.<sup>25</sup> Because the equation is formulated in language of application in one area of economics, it undoubtedly has a reference to reality, something which may well have misled various theoreticians into assuming that it also has informative content. In this way, one might arrive at the view that this is simultaneously a necessary proposition (because of its analyticity) and a content-laden one (because of its relation to reality).<sup>26</sup>

In itself this view is of course very easy to understand, for the difference, for example, between the two following statements appears at a superficial glance to be negligible: ‘*Under the assumption that the trade volume and the speed of the money flow remain constant – that is, *ceteris paribus* – the changes in the amount of money lead to changes in the same direction in the price level*’; and ‘*Because the trade volume and the speed of the money flow remain constant, changes in the amount of money lead to changes in the price level in the same direction.*’ Nonetheless, at issue here is the difference between an analytic proposition and a hypothesis.<sup>27</sup>

25 The transition from quantity theory to the equation of exchange, by the way, was able to occur without further ado with the help of an *ad hoc* introduced special *ceteris paribus* clause; namely, the *ceteris paribus* clause had to be interpreted such that the speed of circulation and the trade volume or the quotient of both values were the factors that were to be held constant. The more rigorous form of quantity theory about the connection between the amounts of money and the price level logically followed from the constancy of these factors. The then-clause of the full proposition would thus then be a logical implication of the *ceteris paribus* clause interpreted in this way, and the proposition itself, a version of the equation of exchange. Now, for the most part, the proponents of neoclassical thinking are clear about the character of the equation of exchange, but that often does not prevent them from raising the pursuit of ‘necessary’ propositions of thought to ideal methodological praxis, and indeed often *expressis verbis*: see also the comments on this from Erich Schneider in *Einführung in die Wirtschaftstheorie*, vol. IV/I, Tübingen, 1962; he expressly refers to Walter Eucken in this. Footnote 54 of Eucken’s *Grundlagen der Nationalökonomie* (5th edn, 1947, 411) is typical. [Eucken, 1947; Schneider, 1962]

26 See also Hutchison’s criticism in the above-mentioned article by him (p. 89); on this problem, also see the interesting article by Jürg Niehans, ‘Die Wandlungen ökonomischer Gesetze’, *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 87, 1951, 300ff. Niehans speaks of a division in quantity theory into two logically distinct elements: on the one hand, a conceptual system, which is transcribed in the manner of bookkeeping; on the other hand, a hypothesis, which merely formulates a special case and thus is only to be upheld from case to case. In my view, this interesting observation shows very clearly how easily conventional and historical strategies can be linked to one another. In their methodological conceptions, many proponents of ‘pure’ theory are not so far removed from the historicists, who believe that it is necessary to reject the possibility of general theories *a priori*. It is not uncommon to find political economists in both camps, who are proponents of the view that general theories must be analytic or in any case immune to facts, but that theories with content must be historically relative. [Hutchison, 1937; Niehans, 1951]

27 Admittedly, it is necessary to emphasize that the first of these two propositions by no means need be analytic. Rather, that depends on the logical grammar of the propositions used in it. The common use of these propositions today makes them analytic. In order to make that formally clear, it is of course necessary to introduce the fitting definitions for the respective terms. However, it is by no means precluded

In the more recent discussion, the quantity theory of money has tended to be replaced by a conception emerging from Keynesian theory. From a methodological perspective, it is extremely interesting to note the way that this substitution tends to be justified. Given the results of modern philosophy of science, one might expect that matters of fact would play a certain role in this argumentation, unless of course it were possible to demonstrate either a contradiction within quantity theory or a lack of informational content, something which, offhand, is only possible for the degenerate form of this theory, namely the equation of exchange. The content of a nomological hypothesis entails precisely that certain possible matters of fact can be excluded. Thus, for example, in its informative versions, quantity theory precludes quantity booms in which the amount of money in fact increases, but does so at constant or sinking price levels, because the social product correspondingly increases at the same time. In opposition to quantity theory, one could, for example, maintain that there have in fact been such quantity booms in economic history. However, argumentation of this sort is by no means an everyday affair. Rather, one sometimes attempts, with the help of one consideration or another, to prove the preferability of the Keynesian instrument, without thereby committing oneself to a special hypothesis that can be formulated with the help of this instrument.<sup>28</sup> Here, one can of course rely on the fact that every informative theory, consequently every version of quantity theory under consideration, must allow the construction of logically possible cases with which it is incompatible. The fact that such cases can be formulated with the tools of Keynesian theory is of course not a sufficient basis for refuting quantity theory. The analysis of such cases can only create clarity about the informational content of this theory, not about its (positive or negative) ‘validity’, consequently, for example, its degree of reliability, but not its relative reliability in comparison with other theories.

Let us presume that propositions are formulated with the help of the tools of Keynesianism, which can be shown to have much greater logical scope than a certain version of quantity theory. Such a finding would initially only imply the greater informational content of the latter theory, so it would scantily be possible to draw negative consequences about its comparative usefulness. If the propositions of the rival theory are also formulated in such a manner that

that changing the rules for applying these terms in a fitting way or modifying the terms might abolish their analytic character.

<sup>28</sup> Here, for example, I am thinking of Erich Schneider’s attempt to clearly delineate between the content and the validity of quantity theory; see also his *Einführung in die Wirtschaftstheorie*, vol. III, 4th edn, Tübingen, 1957, 208–216. Schneider is explicit about the difference between the informative versions of quantity theory and the degenerate version – the equation of exchange – but he nevertheless attempts to refute the former, above all by indicating that, with the help of a Keynesian tools, he can analyze conceivable cases and in so doing show natural cases that lie outside the purview of quantity theory. For a critique of this ‘taxonomic’ article, see, for example, Milton Friedman, *Essays in Positive Economics*, Chicago, 1953, 227ff. In my view, this style of argumentation is typical for proponents of neoclassical thought. [Friedman, 1953; Schneider, 1957]

they have complete leeway, then this theory will immediately be eliminated as a possible alternative, even if one wants to take the view that it contains the factors relevant for the range of phenomena to be explained.<sup>29</sup> In any case, a comparison of theoretical conceptions can only show a difference in their informational content and thus in their *testability* and their *reliability*. To judge them, it would also be necessary to empirically test them, since without doing that, nothing can be said about their comparative reliability.

It is thus hardly possible to judge quantity theory without going into the historical facts such as those mentioned above, facts, which, however, only appear to lead to the refutation of the theory. Now, of course, one can draw completely different conclusions from such a refutation from those commonly drawn. One can forgo both the tautology and the switch to another sort of tool. Recent research shows namely that it is not possible to preclude a reformulation of quantity theory that does not immunize the theory to experience and that is nevertheless compatible with the relevant facts already known.<sup>30</sup> That, however, need not make such a theory acceptable.

### 3.4. *Models in the area of growth theory*

Other examples of model Platonic constructions are found in the area of growth theory, which, according to the widespread interpretation, is mainly concerned with formulating the conditions for what, in certain respects, is ‘undisturbed’ economic growth.<sup>31</sup> Thus, often in constructing growth models, equations are postulated, on the basis of some consideration or another that is supposed to explain the conditions of progressive economic equilibrium. Then these equations, under the application of further equations – for example, of

29 On the basis of a non-informative theory, one cannot *de facto* make out which factors are relevant for the explanation. Judging the causal relevance of a conceptual apparatus thus requires empirically testing the theories formulated with its help. Considerations of plausibility are no substitute for this. Also, references to the fact, for example, that economic processes are dependent upon the decisions of economic subjects, but that, for example, quantity theory contains no relationship to these factors are irrelevant for judging the theory, despite their plausibility; for they are largely uninformative and just as compatible with quantity theory as with other explanatory attempts in this area. There is hardly anything as odd in economic argumentation as the illusion that a reference to the significance of decisions, resolutions, etc. of the economic subjects conveys with no further ado something like relevant information about the course of economic processes.

30 See also, e.g., Milton Friedman ‘Geldangebot, Preis- und Produktionsänderungen’, *Ordo-Jahrbuch*, XI, 1959, 193ff. Friedman’s methodological conception is largely oriented on the results of the modern logic of the sciences; for more, see the introductory article of the earlier-mentioned book by him. However, his treatment of the ‘presuppositions’ that play such a great role in formulating economic theory is problematic. Regarding this, I have the impression that Friedman’s view is capable of securing him from robust testing from precisely the microeconomic theories that he prefers himself, a fact which is out of sync with his general methodological view. For more, see, e.g., T. W. Hutchison’s critique in the above-mentioned book by him (1960), l.c. XIIff., as well at the critique of Eugene Rotwein in ‘On the Methodology of Positive Economics’, *The Quarterly Journal of Economics*, LXXIII, 1959, 554. [Friedman, 1959; Rotwein, 1959; Hutchison, 1959]

31 See also, e.g., Erich Schneider, *Einführung in die Wirtschaftstheorie*, III, l.c. 223. [Scheider, 1957]

definitions for the marginal capital coefficients and for the investment rate – are subjected to certain transformations, from which further equations result. From the interpretation of the results of this operation, one can then gather that it is perhaps necessary to manipulate certain factors such as the investment rate or the capital coefficient in a certain way in order to achieve a progressive equilibrium.<sup>32</sup> Because the initial equations for the transformations are not definitional equations, but explications of the ‘conditions’ for progressive equilibrium, one might get the impression that one is dealing here with the deduction of practically relevant consequences from content-laden theoretical propositions. That, however, is by no means clear. Under ‘conditions’ one can, on the one hand, namely understand *empirical* conditions for the occurrence of certain phenomena, which are able to be characterized independently of the respective conditions, but, on the other hand, also the ‘*logical* conditions’ for some phenomenon or another, which, in this context, are to be understood as including prerequisites, which allow the occurrence of the respective phenomenon to be logically derived without the aid of nomological hypotheses, or without which it cannot be derived. Such ‘conditions’ are in certain respects a part of the statements characterizing the occurrences in question. They ‘define’ the occurrences. It is debatable whether it is efficient in both cases to speak of ‘conditions’ without any further qualification. In any case, the analysis of conditions in the second sense has nothing to do with the formulation of theoretical hypotheses to explain any facts.

In the economic discussion, one can see again and again that the expression ‘condition’ is by no means used in the mentioned empirical sense, but that it is often used in a way that comes closer to the second meaning. In growth theory that even seems to be the normal case.<sup>33</sup> The initial equations, which explain the named ‘conditions’, are not generally treated as hypotheses or as

32 See also, for example, Klaus Rose’s critical analysis, ‘Der Erkenntniswert der Wachstumsmodelle’, *Jahrbücher für Nationalökonomie und Statistik*, 168, 1957, 321ff., which I essentially agree with, albeit not in every respect; see also my comment, ‘Wachstumsmodelle und Realität. Bemerkungen zu Roses Kritik der Wachstumstheorie’ in vol. 169 of the same journal, as well as Rose’s reply. [Rose, 1957; Albert, 1958b]

33 Gottfried Bombach, ‘Zur Theorie des wirtschaftlichen Wachstums’, *Weltwirtschaftliches Archiv*, 70, 1953, I. One does indeed find the tendency to link hypotheses with this that identify the equation of exchange with trend movements, but this leads to certain difficulties, which appear to render this type of theory formation hopeless; see Klaus Rose, l.c. 325f. Rose thus thinks that the only type of interpretation of the respective equations that is possible renders it devoid of content. Consistent with this, he denies the theory any of the explanatory power that other theoreticians such as Erich Schneider attribute to it, although it may be difficult to supply the evidence for this. Rose, however, does not go so far in his skepticism to doubt the practical application of theory. Because there is a close connection between informative content and practical application, I am not able to assent to his view on this matter; see also the discussion in the *Jahrbücher* mentioned in comment 32. For more on the criticism of growth theory, compare above all Christian Watrin, ‘Modelle und Hypothesen in der Wachstumstheorie’, in *Wirtschaftspolitische Chronik*, vol. I, 1960, Institut für Wirtschaftspolitik at the Universität Köln. [Bombach, 1953; Rose, 1957; Watrin, 1960]

components of hypothesis, but as mere assumptions,<sup>34</sup> whose consequences are to be examined. Of course, in the context of growth analysis, the claim can be made without further ado that the concerned equations must be fulfilled if the respective social system is to be considered on the ‘equilibrium path’ of economic growth. However, as long as this equilibrium path in principle cannot be determined independently from the concerned equations, as appears to be the case currently, this statement is immune to all testing against the facts. In fact, one can even usually presume that this equilibrium path ought to be defined with the help of the concerned equations, something that is difficult to recognize since one does not tend to view such equations as fragments of more complex statements. In general, a certain danger in economic thinking seems to me to consist in severing from their contexts the components of possible hypotheses that can easily be formalized and treating them as pure assumptions, whose theoretical role is then difficult to determine. If achieving the equilibrium path is made logically dependent upon fulfilling the concerned conditions, then the statement in question becomes a tautology. To avoid this, by introducing suitable hypotheses, one can of course relate the model to certain independently identifiable conditions.<sup>35</sup> As long as that does not happen, it is in any case immune to the facts.<sup>36</sup>

34 I would like to point out that the expression ‘presupposition’ was used differently from, for example, in the case of *ceteris paribus* clauses, but also in a way that it often appears in economic theory. It is concerned here with statements that are only viewed with respect to the logical consequences that follow from them, without a great interest in their validity or even their testability. At least in the context under discussion, there is often a tendency to avoid linking them in any way with assertions that transcend real relations. As part of the method of model Platonism, statements that could be interpreted and treated as hypotheses or as components of hypotheses are degraded to the status of mere assumptions so that the problematic of testing and confirming them, and consequently their significance for empirical science, can be sidestepped. It is of course possible to formalize systems of such statements and conduct all sorts of transformations with them. The criticism is thus often directed towards the formalization of the sets of such statements under consideration rather than towards the methodological practice connected with it. In the criticism of the use of mathematical forms of expression and of deduction in economics, which continually occurs, the concern is sometimes with methodologically justified misgivings about how this is ‘falsely arrived at’: see also, e.g., James Duesenberry’s discussion: ‘The Methodological Basis of Economic Theory’, *The Review of Economics and Statistics*, XXXV, 1954, pp. 361ff. One should not only note that the use of mathematical language may be accompanied by certain advantages regarding precision, etc. and that it thus is not very sensible to polemicize against it. It can also be observed that there are theoreticians who confuse theory formation and formalization. It is indeed more modern, but not more sensible. [Duesenberry, 1954]

35 For example, it is not impossible to treat the assumption of increasing equilibrium, which Rose proposes –  $\frac{\Delta Y}{Y} = \frac{\Delta P}{P}$  which can be transformed with the help of a suitable definition into  $\frac{\Delta Y}{Y} = \frac{i}{c}$  (whereby  $i$  is the investment quote and  $c$  the marginal capital co-efficient) – as a fragment of a general hypothetical proposition of the form  $(x)Qx \Rightarrow Rx$ , and in fact, in this case, as the then-clause of such a proposition. The then-clause could then include the empirical conditions necessary for the basic equation so that the whole proposition becomes a hypothesis. In this case, however, a logical relationship between  $Qx$  and  $Rx$  cannot exist.

36 Erich Schneider concludes his section on growth theory (p. 240) with the characteristic remark that the growth model that he discusses does not state anything ‘about the powers... that really determine

### 3.5. *Procedures of model Platonism*

It would be possible to multiply the examples of such procedures with no difficulty. Just think about the theories on market forms, market relations, and market behavior that generally tend to be constructed so as to leave open all possibilities, but that nevertheless are expressed in a form that leaves the impression that they are content-laden assertions.<sup>37</sup> When any behavioral maxims whatsoever appear, they are not formulated and treated as hypotheses, but as assumptions about the possible behavior of economic subjects, whose logical implications are to be examined. Here all sorts of questions come to the fore that have little to do with the informational content, the explanatory power, the prognostic value, or the degree of validity, namely questions of the context of the deduction, the ability to formalize it, and its plausibility.<sup>38</sup> This also applies, for example, to marginal productivity theory, which long was considered a useful instrument for explaining the functional income distribution. Here often it was not treated as a theory with empirical content, but was developed from the presumptions of profit maximization under certain further presumed conditions, without the question of its content and its validity even arising.<sup>39</sup>

Welfare economics, whose theoretical conceptions and procedures – and in fact both in the Pigovian and in the Paretian versions – display typical traits of neoclassical thought, has created a large area for pure formal controversies and propositional constructs whose empirical relevance is difficult for the impartial observer to recognize.<sup>40</sup> However, some theoreticians view the reference to the

growth, which can be observed in the course of time'. It merely 'exposes the conditions of unimpeded growth' and shows 'how income and investment must develop over time if these conditions are to be fulfilled'. Obviously it is left to the reader to decide whether the concern here is merely pure logical connections or a content-laden theory. [Schneider, 1957]

37 Without empirical content, virtually all classification systems are included, for example, also the system that Robert Triffin develops in his book, *Monopolistic Competition and General Equilibrium Theory*, Cambridge, 1940. While Triffin by no means attempts to give the impression that he is formulating content-laden propositions, hypotheses with the help of which it is possible to explain real phenomena, this is not so easy to recognize among other theoreticians. [Triffin, 1940]

38 The representatives of the Chicago school who advance Friedman's methodological conception have usually argued in this manner in the discussion with proponents of the theory of monopolistic competition, who are not in agreement with their own methodology; see also G. C. Archibald, 'Chamberlin versus Chicago', *The Review of Economic Studies*, XXIX, 78, 1961, pp. 1ff. [Archibald, 1961]

39 See also the interesting analysis of Erich Preiser in his article 'Erkenntniswert und Grenzen der Grenzproduktivitätstheorie', *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 89, 1953, which expresses the character of this theory very well. On the question of empirical testing, see also G. C. Archibald, 'Testing Marginal Productivity Theory', *The Review of Economic Studies*, XXVII, 1959/60, pp. 210ff. [Preiser, 1953; Archibald, 1959/60]

40 For more on this, see, for example, the critical analysis of E. J. Mishan, 'A Survey of Welfare Economics, 1939–1959', *The Economic Journal*, LXX, 1960, no. 278. This also nicely shows how central theories have been immunized by the use of conventionalist strategies so that, to those with the appropriate intuitions, their usefulness becomes a secret; see also my contribution, 'Social Science and Moral Philosophy', in *The Critical Approach to Science and Philosophy. Essays in Honor of Karl*



normative character of the teaching as providing a sufficient reason for avoiding statements with empirical content.

It is difficult to completely describe the different sorts of conventionalist strategies that may be used to immunize statements and models to the facts. As already said, here it is by no means necessary that these always be strict tautologies. As I have outlined above, it is possible to use unspecified or correspondingly interpreted *ceteris paribus* clauses for this, which can, however, in borderline cases lead to a tautology, and which in any case provide the respective statements with an unlimited alibi. Besides that, by more or less explicit statements, it is possible to limit the application of the respective models to the areas in which the presuppositions are met, for example, to the areas of the respectively defined rational behavior. Clearly, it is possible to interpret the ‘presuppositions’ of a theoretical system – that is, in this case, the theoretical statements from which all other statements of the system are deduced – not as hypotheses, but simply as limitations to the area of application of the system in question. Since a relationship to reality is usually ensured by the language used in economic statements, in this case the impression is generated that a content-laden statement about reality is being made, although the system is fully immunized and thus without content. In my view that is often a source of self-deception in pure economic thought. The only assertions that remain in these procedures are related to the logical connections, and they are thus often of a meta-economic (that is meta-linguistic) nature. A further possibility for immunizing theories consists in simply leaving open the area of application of the constructed model so that it is impossible to refute it with counter examples.<sup>41</sup> This of course is usually done without a complete knowledge of the fatal consequences of such methodological strategies for the usefulness of the theoretical conception in question, but with the view that this is a characteristic of especially highly developed economic procedures: the thinking in models, which, however, among

*Popper*, edited by Mario Bunge, Glencoe, 1964. G. C. Archibald rightly pointed out that it is not difficult to neutralize this structure; see his article, ‘Welfare Economics, Ethics, and Essentialism’, *Economica*, XXVI, 104, 1959. Besides that, he drew attention to the fact that propositions of welfare economics are typically not formulated so as to be testable, but that it is possible to reformulate them so that they are. However, such a procedure probably does not suit the intentions of most representatives of this discipline. [Archibald, 1959; Mishan, 1960; Albert, 1964]

<sup>41</sup> For example, Andreas G. Papandreou, in his book, *Economics as a Science*, Chicago-Philadelphia-New York, 1958, seems to aim at this (see especially ch. 6, ‘Models vs. Theories’) where he notes that, in economic theory formation, the social space to which the model applies is often not specified. I do not have the impression that K. Klappholz and J. Agassi’s criticism in ‘Methodological Prescriptions in Economics’, *Economica*, XXVI, 101, 1959, p. 69f., does full justice to Papandreou’s book any more than their criticism does justice to the books of any of the other authors they treat. One need only look over economic textbooks to see that the social area of application of many models is clearly not identifiable – or at most with the help of assumptions that the authors would probably reject – because they do not want to advance hypotheses. [Papandreou, 1958; Klappholz and Agassi, 1959]

those theoreticians who cultivate neoclassical thought, in essence amounts to a new form of Platonism.<sup>42</sup>

#### **4. Between model Platonism and conceptual realism: fronts in the German method controversy**

Neoclassical thought, which is dominant in most areas of economics, combines the tendency toward model Platonism with the tendency to insulate theoretical economics as an autonomous discipline from the basic social sciences, thus above all, from sociology and social psychology. Economics appears to be an object area that can clearly be interpreted and explained without recourse to the results of sociological and social-psychological research.

This economic orthodoxy has always stood in contrast to orientations in economic thinking that have attempted to treat economics as a human science, and that, in contrast to neoclassicism and similar approaches, have declared misgivings regarding content and method, such as Marxism, the historical schools, the social law school, and institutionalism. It can hardly be denied that these currents, however one may judge their overall success, have contributed essential elements to the development of economic thought, which, in light of the one-sided orientation of pure economics, were capable of providing a certain counterbalance. If one attempts to acquire a view of the diverse method controversies that have appeared at the frictional interface between the heterodox views and neoclassical economics, it is now difficult to accept the views of one or the other side as a whole. Those, for example, who would like to argue that, in the first German method controversy, Carl Menger advocated the concerns of theoretical thought against pure historicism can hardly avert the objection that the type of theory he advocated and, above all, his methodological approach to theoretical thinking appear extremely questionable from the viewpoint of contemporary philosophy of science. A strong tendency towards apriorism is characteristic of the Austrian school, which then, via Ludwig von Mises and other theoreticians, also had a considerable influence on Anglo-Saxon thought, completely independently of the fact that similar tendencies were long at work here. Some thoughts of Schmoller and his students – especially, for example, Eulenburg – on issues of method are obviously much closer to the views developed in contemporary philosophy of science than are the views of the Austrian school.<sup>43</sup> However, Schmoller himself unmistakably employs a certain inductivism, which had an adverse effect on the methodological practices of the

<sup>42</sup> In my contribution to the discussion mentioned above, 'Der logische Charakter der theoretischen Nationalökonomie', I go into greater detail about other characteristics of model Platonism, which I want to forgo here. [Albert, 1959b]

<sup>43</sup> For this reference, above all to Eulenburg's work, I would like to thank Reginald Hansen. For a criticism of the Austrian direction, see especially the book mentioned above by Terence Wilmot Hutchison. [Hutchison, 1938]

younger historical school, negatively influencing theory formation. In any case, in light of the results of contemporary philosophy of science, it is not necessary to choose between Mengerian apriorism and Schmollerian inductivism.

The contemporary fronts in the German method controversy have developed under the influence of social-scientific-oriented theoreticians following Max Weber, whose methodological views can largely be traced back to the effects of phenomenological hermeneutic philosophical orientations. Beginning at the turn of the 20th century, these currents in philosophical thinking, especially in German-speaking countries, first had a growing influence on the thinking in the social sciences, which – as sciences of socio-historical reality, orders of life, and interpretations of the world – were contrasted with the natural sciences, and were thought to have a special methodological status.<sup>44</sup> The economists of this orientation tend to be critical of the model thinking of pure theoreticians, even if they by no means necessarily reject their results and methods. They tend rather to accuse them of absolutizing their ‘partial knowledge’ and of being unwilling to recognize its fragmentary character in view of the ‘cumulative knowledge’ aimed at by society as a whole. The economists oriented on the social sciences and humanities thus combine their anti-naturalistic methodology with the tendency to emphasize holistic social contexts and to argue sociologically. However, upon analyzing the studies that they refer to as ‘theoretical’, one often discovers a purely conceptual discussion, which is supposed to result in clear insights into essential relationships.<sup>45</sup> Here, too, defining statements and other analytic propositions are viewed as informative and as essential components of a theoretical conception since they are related to reality. Insofar as the scientific research in this area is to have theoretical relevance, in general it is not oriented towards hypotheses, as in the natural sciences, but towards concepts. A wealth of concepts is confused with a wealth of information. Among other things,

44 In this context, I cannot go into details about methodology in the social sciences and humanities. For this, see also my article, ‘Probleme der Wissenschaftslehre in der Sozialforschung’, in *Handbuch der empirischen Sozialforschung*, edited by René König, vol. I, Stuttgart, 1962, and the literature provided there, especially, Karl R. Popper, *The Poverty of Historicism*, London, 1957, and Quentin Gibson, *The Logic of Social Enquiry*, London, 1960; in addition, the newer work by Ernest Nagel, *The Structure of Science*, London, 1961. For a criticism of the national economy oriented on the human sciences, also see also my article, ‘Der moderne Methodenstreit und die Grenzen des Methodenpluralismus’ *Jahrbuch für Sozialwissenschaft*, 13, 1962. [Popper, 1957; Gibson, 1960; Nagel, 1961; Albert, 1962a,b]

45 As especially Popper noted, after being eclipsed by natural scientific thinking, conceptual realism or essentialism has found asylum in certain philosophical movements and in the social sciences. It is especially to be found where phenomenological–hermeneutic currents of philosophy have wielded influence: among the social scientists who are oriented on the humanities. However, it must be emphasized that there are a number of exceptions, such as, for example, Max Weber. Only since the 1920s has the ‘material apriorism’ of the phenomenological components of this current had a greater influence on strengthening essentialist tendencies. For a criticism of conceptual realism, see Gerhard Weisser, ‘Wirtschaft’, in the *Handbuch der Soziologie*, edited by Werner Ziegenfuß, Stuttgart, 1956, pp. 978ff. There is, of course, no clear connection between the use of the word ‘essence’ and essentialism as a methodological conception. [Weisser, 1956]

because the proponents of modeling in pure economics rely on mathematical expressions and forms of reasoning, they are treated as advocates of the natural scientific method and criticized for reputedly not having adequately considered the connection of these methods to objectivity.

We are thus confronted with a formation of fronts in the German method controversy that can be traced back to an array of misunderstandings, without which it would hardly be understandable. One tendency is 'pure' economic modeling that largely ignores sociology and that leans toward the model Platonism outlined above; another tendency is a humanistic form of economics, open to sociology, but which cultivates essentialist thinking and thus tends towards conceptual realism. The pure economists have a very understandable aversion to the imprecise, vague, and opaque verbal speculations of the essentialists, who have adopted the dominant questionable philosophical style in Germany since Hegel, while the essentialists are very skeptical of the modelers' copious use of mathematical expressions and their disinterest in social relationships.

As much as these two styles of thinking differ in details, for example, regarding their views of the theoretical autonomy of economic thought and their inclusion of social factors, the use of mathematical or verbal expressions, the significance of models, etc., they share a common distinctive element of importance, which understandably is thus hardly emphasized in their method controversy: namely, the methodological apriorism, which largely dominates their theoretical methods. Problems of the information content, the empirical testability, and the confirmation of theories hardly play a role in this controversy in German-speaking countries. In light of these circumstances, it may be difficult for some economists who are not directly involved in this controversy to be convinced of the need to decide in favor of one or the other of these tendencies. From the perspective of the modern philosophy of science, for example, the confirmation of the concerned theories could be viewed as an essential argument, but both model Platonism and conceptual realism tend to immunize the theoretical conceptions under question to facts, consequently they by no means have the opportunity to be confirmed. Why should a proponent of pure economics be prepared to consider the verbal speculations about the essence of the economy and of society in his model constructions if the relevance of these thoughts for his work cannot be shown? He will readily be able to content himself with his 'partial knowledge' if the relevance of the problem of the abstraction from social facts cannot be clearly shown on the basis of his models. The same of course applies to the proponents of a form of social economics that aims at essentialist thinking and 'complete knowledge'. Platonic constructions will hardly be able to convince him that a form of pure economics that abstracts from non-economic factors can be a successful undertaking. The *a prioristic* basic tendency characteristic of both approaches results in a solidification of the positions that can only be redressed by referring back to it as the fundamental weakness of both

positions, and bringing the methodologically decisive question into play from there.

It is important above all to dispel the impression that these two tendencies are the only alternatives available. The formation of fronts in the German method controversy appears to feed into the grave misunderstanding that one has to choose between a model Platonism with mathematical tendencies, which ignores sociology, and a sociologically oriented conceptual realism, which tends towards verbal speculation. It is thus important first of all to recognize that considering sociological knowledge by no means requires one to assume the methodological conception of the essentialist thinkers. Modern sociology has long disassociated itself from studies of the essence of society, the state, or social structures, which was typical of earlier work; and, in connection with social psychology, by applying natural scientific methods, it has procured an array of useful results, also of a theoretical nature, which may at least be of some relevance in explaining economic phenomena. On the other hand, the application of mathematical expressions and mathematical deductions need not be connected with the attempt to immunize the models in question to the facts with the help of relevant strategies and simultaneously to insulate economics from sociological insights. There is no set of problems in the empirical sciences, not even in the social science disciplines, for which it makes sense to immunize theory formation *a priori* to possible objections that emerge on the basis of relationships to the facts. However, neither is there an economic problem, except of a purely formal character, in relation to which one could say *a priori* that it could be solved, without the incorporation of social factors that have not yet been taken into consideration. In the final analysis, this means that it is impossible, on the basis of the way it formulates problems, to decide in advance on the theoretical autonomy of economic thinking.<sup>46</sup>

## 5. Overcoming neoclassicism by sociologizing economic thought

However, perhaps it is worthwhile to dwell a bit longer on the strange fact that precisely the economic thinkers closest to the neoclassical tradition, who tend towards model Platonism, are at the same time the most tempted to insulate economic thought from knowledge of the social sciences and to defend the theoretical autonomy of economics. It is possible that this connection is not coincidental. Economics is essentially concerned with theoretically penetrating and explaining aspects of a particular area of social life. The way economic facts are presented has often led to the impression that it is concerned with an

<sup>46</sup> For more on this, see my above-mentioned article, 'Nationalökonomie als Soziologie'. A claim like this can of course always be made if one does not shy away from certain 'costs', e.g., if one is prepared to engage in tautology and historicism, i.e., to sacrifice informative content. However, one would do well to consider the consequences of such a procedure. [Albert, 1960]

'astronomy of the movement of goods',<sup>47</sup> but in the end hardly anyone can doubt that this movement of goods is steered by human behavior and that it is necessary to refer back to human behavior in order to explain it. However, what is seen less often is the sociological character of microeconomic terminology, which focuses on ascertaining the exchange relationships between persons and social groups, thus between certain, i.e., social, relationships constituted by reciprocal behavior. The central idea of economic thought is in a fundamental sense a sociological one: namely, that the production and distribution of goods regulates itself quasi-automatically by a system of commercial relationships between the people and groups of a society, flanked by a judicial sanctioning mechanism, in a way that is relevant to satisfying the needs of the individuals concerned. It is thus concerned with the analysis of certain effects of processes in a subdomain of society that is organized by the market. In this, an attempt is made to trace all relevant processes back to the decisions of economic subjects that follow certain maxims. In neoclassical thought these maxims are usually formulated as maximization assumptions, whereby the object maximized can differ. However, in order to make the money flow and the flow of goods comprehensible, other reaction functions, which can be postulated *ad hoc*, can readily be substituted for maximization assumptions.

Based on the research results of other social sciences, above all sociology and social psychology, we know today that the action and decisions of those performing social roles are dependent on facts that by no means have a 'pure economic' character, that is, factors that cannot be captured with the tools of neoclassical thought and its derivatives. These are above all factors of a dispositional nature, such as motive structures, attitudes, value orientations, etc., as well as the respective social context of the behavior concerned, also aside from the field of commercial relationships, especially insofar as it is defined by the existing institutions.<sup>48</sup> There is a good deal of research showing the importance of particular standards, social norms, and their institutionalization

47 I have tried to work out the peculiarities of the economic perspective that are characteristic for neoclassical thought in my article, 'Die Problematik der ökonomischen Perspektive', *Zeitschrift für die gesamte Staatswissenschaft*, 117, 1961, pp. 438ff. This perspective leads to a considerable abstraction from the social and to an attempt to isolate the monetary flows and the flows of goods insofar as this is possible. However, this can never be completely isolated. At least implicitly, the actions of the members of the social entities under question always play a pivotal role. Even the conceptual apparatus of microeconomics is in the final analysis directed towards this, as the logical analysis shows. See also, e.g., Jürgen v. Kempster, 'Handlung, Maxime und Situation. Zur logischen Analyse der mathematischen Wirtschaftstheorie', *Studium Generale*, 7, 1954, pp. 60ff.; reprinted in *Theorie und Realität*, edited by H. Albert, Tübingen, 1964. [Von Kempster, 1954; Albert, 1961]

48 For the basic orientation with reference to economic behavior, see also, e.g., Talcott Parsons, 'The Motivation of Economic Activities', in Talcott Parsons, *Essays in Sociological Theory*, rev. edn, Glencoe, 1954, pp. 51ff. [Parsons, 1954]

and internalization, as well as of the reference groups and other factors for the action of members of society.<sup>49</sup>

It is extremely unlikely that all of these results are without significance for the action of people in the economic domain of society.<sup>50</sup> The significance of the factors concerned does not appear to be limited to special areas of society. The models of the pure theoreticians tend not to incorporate such factors. Rather, here reaction functions tend to be postulated that are obviously supposed to be completely or at least predominantly independent from the dispositional characteristics of the individuals concerned and from all non-commercial components of the social environment.<sup>51</sup>

The economy, the system of market relationships between members of society, is viewed as a relatively closed network of forces, as a system, which indeed receives a certain external impetus, but functions independently of factors such as those mentioned above, which cannot be ascertained with economic tools. Here, the money flows and flows of goods in a commercialized industrial society run through nearly all social structures that could be found, and all members of society participate in the commercial processes in one way or another through the individual structural roles that steer these flows. If one gains clarity about these relationships, then one can begin to understand why the models constructed with the help of simple behavioral assumptions by neoclassical oriented theoreticians

49 For the problematic of the level of demand, see above all the summarizing work of Kurt Lewin, Tamara Dembo, Leon Festinger, and Pauline Snedden Sears, 'Level of Aspiration', in *Personality and the Behavior Disorders*, edited by J. McV. Hunt, New York, 1944. Later research tends to make reference to this. On the problematic of reference groups, see also Robert K. Merton (with Alice S. Rossi), 'Contributions to the Theory of Reference Group Behavior', as well as 'Continuities in the Theory of Reference Groups and Social Structure' by the same authors, in Merton, *Social Theory and Social Structure*, rev. edn, Glencoe, Illinois, 1957. For the question of motivation, see also, e.g., *Motives in Fantasy, Action, and Society*, edited by John W. Atkinson, Princeton-New York-London-Toronto, 1958. [Lewin *et al.*, 1944; Atkinson, 1958; Merton and Rossi, 1958a,b]

50 By contrast, there is already a series of studies that apply these to economic problems; see also, e.g., the book by David C. McClelland, which is based on a Max Weber-like formulation of the problems, *The Achieving Society*, Princeton-New York-London-Toronto, 1961, in which connections between the motive structure and economic development are studied; further, George Katona's book, which is available in German, *Das Verhalten der Verbraucher und Unternehmer*, edited by Erik Boettcher, Tübingen, 1960; James S. Duesenberry, *Income, Saving and the Theory of Consumer Behavior*, Cambridge/Mass., 1949; C. Addison Hickman and Manford H. Kuhn, *Individuals, Groups, and Economic Behavior*, New York, 1956; many worthwhile references are in the book from Robert A. Dahl, Mason Haire and Paul F. Lazarsfeld, *Social Science Research on Business. Product and Potential*, New York, 1959; for an application of the conception of the level of the demand, see also Richard S. Weckstein, 'Welfare Criteria and Changing Tastes', *American Economic Review*, LII, 1962, pp. 133ff. [Duesenberry, 1949; Hickman and Kuhn, 1956; Dahl *et al.*, 1959; Katona, 1960; McClelland, 1961; Weckstein, 1962]

51 Gerhard Weisser has frequently pointed to the necessity to consider such extra-economic factors; see also his book, *Form und Wesen der Einzelwirtschaften. Theorie und Politik ihrer Stile*, vol. 1, 2nd edn, Göttingen, 1949; also his above-mentioned article, 'Wirtschaft'; see also the work of Gunnar Myrdals, above all the eighth chapter of his book, *Das politische Element in der nationalökonomischen Doktrinbildung*, Berlin, 1932, 2nd edn, Hannover, 1962. [Weisser, 1949, 1956; Myrdals, 1962]

must be immunized against experience in one way or another if their failure is to be avoided. It is not by chance that the attempts of some proponents of pure economics to achieve autonomous theory formation tend to be translated methodologically into model Platonism: *the immunization from the influence of non-economic factors leads to the immunization from experience in general*. It appears that the diagnosis of the fundamental methodological weakness of the neoclassical way of thinking must lead to an aversion to sociology. By contrast, regardless of all methodological differences, all heterodox currents in economics characteristically share one element: the accentuation of the significance of social factors for economic relationships and the consciousness of the fact that the social domain analyzed by pure economics is embedded in a more comprehensive social complex that cannot be abstracted away from with no further ado if useful explanations are being sought. The methodological weakness of these currents should not prevent one from seeing what is, in my view, the decisive point, which generally tends to be buried amidst an array of irrelevant arguments about subordinate problems or pseudo-problems, such as those about the applicability of mathematical expressions, the usage of certain types of terms, the question of the preferability of generalizing or pointedly emphasizing abstraction, etc.

If the results of the research conducted in the social sciences thus far are correct, the behavior of those who assume economic roles, which one generally attempts to explain with the help of simple maximization assumptions or *ad hoc* constructed reaction functions, is *de facto* dependent on the motivational and institutional structures that underlie this behavior, and which, for their part, also have a quasi-invariable character.<sup>52</sup> They are built upon and change in reference to learning and generally all kinds of social processes – in which social factors play an important role. There can hardly be any doubt that the ‘style’ of social life and economic activity are subject to more or less slow historical change.<sup>53</sup> This, however, is by no means a sufficient reason for limiting theoretical analysis in the social sciences to the formation and application of ‘style-related’ quasi-theories, as is sometimes suggested; but it does perhaps provide an occasion for posing the question of the extent to which, for example, existing theoretical conceptions

52 I deal in more detail with the problem of quasi-invariances in my article, ‘Theorie und Prognose in den Sozialwissenschaften’, *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 93, 1957, pp. 60 ff., and in the above-mentioned article in *Kyklos*, 1960. [Albert, 1957, 1960]

53 In this, it is possible to agree with the representatives of the ‘humanistic approach’ to economics without further ado, if only because they are obviously operating with a concept of style which aims at apprehending certain quasi-invariances of social life. For an interesting analysis of such facts, see Gerhard Mackenroth, *Bevölkerungslehre. Theorie, Soziologie und Statistik der Bevölkerung*, Berlin-Göttingen-Heidelberg, 1953. Mackenroth here attempts, among other things, to work out certain institutionally influenced economic and population approaches and to ferret out the conditions for their transformation. See also *Trade and Market in the Early Empires: Economies in History and Theory*, edited by Karl Polanyi, Conrad M. Arensberg, Harry W. Pearson, in which it is very impressively shown that some views of the general applicability of economic categories cannot be maintained. [Mackenroth, 1953; Polanyi, 1957]



today in fact grasp quasi-invariances, dependent on our cultural milieu, or limited to a more limited specific socio-temporal domain.<sup>54</sup> The fact that this question has some justification specifically with regard to economic theories appears to me to result very clearly from findings of Max Weber's research, as well as from the other historical, sociological and social-psychological studies in the wake of his research.

Like all theoretical tools, the conceptual apparatus that tends to be used in economic analyses is selective in a very specific manner; it embodies a perspective characteristic of economic thinking, which is related to the problem and influences the way it is formulated.<sup>55</sup> The extent to which the factors emphasized by the economic tools are in fact relevant for explaining economic facts can only be determined in reference to how the theories constructed with their help in fact stand up. To confirm theories, it is more important to test them in new areas of application than to continually retest them in the same area.<sup>56</sup> For social scientific theories, and thus also for economic ones, it is extremely important to vary the socio-cultural area in which testing is carried out. It follows from this that the general relevance of the economic conceptual apparatus can best be tested by applying economic theories to socio-cultural areas that they were not designed for, for example, to developing countries. In investigations in areas like these, one continually finds that it is necessary to refer back to factors that are not considered in the conceptual apparatus of theoretical economics,<sup>57</sup> but

54 In his book, *Die Grundlagen der Nationalökonomie* (pp. 35ff., p. 276f.), Walter Eucken speaks of nature as having an 'invariant general form' and of economics as having a *prima facie* 'variant general form', but he notes that in the theoretical analysis in this area in the last analysis it is also possible to break through to an invariant general form. Herewith he indicates that he does not want to recognize resignation as the historical solution to the problem of theory formation. However, if one examines his 'overcoming of the great antinomies' of theory and history, especially the very problematic concept of theory he utilizes in them, and his interpretation of the significance of what are known as 'pure forms' and the methods of 'poignant emphasized abstraction', then one may have some doubt about the validity of the path he sets out upon, if not the general usefulness of his objectives. [Eucken, 1947]

55 See also my above-mentioned article, 'Die Problematik der ökonomischen Perspektive'. Paul K. Feyerabend has drawn attention to the fact that alternative theories of explanation of an area set out from strongly divergent points of view (in my terminology, perspectives) precisely in cases that are of interest for the development of the sciences, and thus also diverge from one another in their conceptual formation; see also his treatise 'Explanation, Reduction, and Empiricism', in *Minnesota Studies in the Philosophy of Science*, III, *Scientific Explanation, Space, and Time*, edited by Herbert Feigl and Grover Maxwell, Minneapolis, 1962. He deals there very instructively with the question of theory formation and the possibility of the reduction of theoretical systems to such a high level. [Albert, 1961; Feyerabend, 1962]

56 The more diverse the test milieus are, the more stringent the testing can be. See also Karl Popper, *The Logic of Scientific Discovery*, ch. X, especially pp. 269ff. By the way, varying the area of application makes it possible to eliminate quasi-invariances. [Popper, 1959]

57 See also, e.g., the book from McClelland mentioned in footnote 50, *The Achieving Society*; also Robert N. Bellah, *Tokugawa Religion. The Values of Pre-Industrial Japan*, Glencoe, 1957; Everett E. Hagen, 'How Economic Growth Begins. A General Theory Applied to Japan', *The Public Opinion Quarterly*, XXII, 1958, as well as the extremely interesting book by the same author, *On the Theory of*

that are accessible to sociological and social-psychological research. It appears to me that we have no reason to ignore these issues in economic theory formation. They can give us occasion to suppose that we have thus far very often dealt with quasi-invariances in economics that can only be relativized by referring back to deeper-lying factors. Model Platonism of the neoclassical variety is, however, a useful means to temporarily sidestep this consequence; I believe that I have shown the price this entails above.

In my view, it should not have only been possible for us to have learned from Max Weber that the dominant style of economic activity in many parts of the world is a social product, that it has a traceable historical development, and that it may be subject to great alterations in the future. It should also have been possible for us to have drawn the obvious consequence of his studies for theory formation; namely that it will only lead to general insights if it penetrates behind the quasi-invariances of the economic style that began with the industrial economy. Changing economic studies in accord with these insights would require a decisive shift in theoretical perspective, a shift that would perhaps resemble traditionally heterodox currents of economic thought more than the pure economics of the neoclassical variety.

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