

REORIENTING THE ASSUMPTIONS ISSUE

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ECONOMISTS AND THE ASSUMPTIONS ISSUE

The most important methodological issue in economics has been and persists to be over what is called the 'realism' of theories and their 'assumptions'. Profit maximization, perfect information, transitive preferences, diminishing returns, rational expectations, perfectly competitive markets, givenness of tastes, technology and institutional framework, non-gendered agents – these and many other ideas have been assumed by some economists and questioned by others. The issue has often been whether such assumptions are ('too') unrealistic or ('sufficiently') realistic or whether it matters at all if they are one way rather than the other.

The forms in which this issue appears in the work of practising economists can be approached from a variety of angles. From the perspective of the kind of behaviour in which economists engage themselves we may distinguish two forms: let us call them the silent form and the loud form.

The silent form is silent in that the general principles guiding an economist's attitudes and decisions are not explicitly pronounced and invoked in the practice of research and communication. For instance, an economist who prefers realistic to unrealistic assumptions in theories and models, may, without making any noise about it, simply ignore frameworks that are supposed to give rise to insufficiently realistic theories, and pursue ever more realistic models by introducing modifications within the chosen framework. In this silent form, the issue appears most of the time as the daily bread of an economist when considering which theoretical frameworks and, within such frameworks, which assumptions to adopt and which to reject in model building.

The loud form of the issue is loud in that explicit appeals are being made to general principles of scientific theorizing in defending or criticizing a theory or framework for being realistic or unrealistic. It often takes the shape of open controversy in which arguments are forwarded about the past failures and successes and the desirable future course of economic inquiry. Such an open debate occasionally bursts out as an expression of

deep disagreements between schools of thought or between inquirers with different mentalities. This debate has had many incarnations and it has gone through exciting episodes, including the German *Methodenstreit* in the 1880s between Carl Menger and Gustav Schmoller; the marginalist controversy in the 1940s between Richard L. Lester and Fritz Machlup; the Friedman controversy in the 1950s and 1960s; the capital controversy in the same period between the two Cambridges; and so forth. This is a recurring controversy which is generated around ever new topics.

From another perspective, that of the kinds of stakes at issue, we may also find two forms, call them antagonism and family quarrel. An *antagonism* is something that prevails between mutually incompatible frameworks of analysis, theories and approaches, traditions and schools of thought, while *family quarrels* appear within such intellectual formations. Statements about whether a given theory and its assumptions are or should be realistic occur within both antagonisms and family quarrels. Antagonisms and family quarrels differ from one another in regard to the seriousness of consequences that a challenge to a theory or framework may have. A statement made within an antagonism implies a suggested switch or a refusal to switch to another theory or approach, such as from the standard neoclassical framework to Austrian or institutionalist or post-Keynesian frameworks. On the other hand, a statement within a family quarrel implies a suggested move or a refusal to move to another version within a theory or framework, such as within the neoclassical framework.

There is considerable overlap between the two pairs of forms in which the issue appears. Antagonisms are often loud, whereas family quarrels are often silent. However, the two pairs are not identical. Advocates of radically rival approaches often refuse to engage in open controversy over fundamental principles, even though – and sometimes because – the stakes are high. This is the not-so-unlikely case of silent antagonism. On the other hand, open debate on the ways of theorizing may be sparked even though the stakes related to the basic substance of a theory are low. That is, family quarrels may sometimes be noisy, too.

It should also be noted that instead of two dichotomous forms of the issue, we rather have two continua of forms, the first having antagonism at one extreme and family affair at the other, the second having the loud form at one end and the silent form at the other. The issue may take forms that are more or less loud or silent, and closer to constituting an antagonism or a family quarrel. It is not always easy to locate actual cases on these continua. For example, since the identity of a theory or framework is not always firm and clear, it is sometimes hard to tell whether a revision means a move from one theory or framework to another or whether it constitutes a move within the original theory or framework.

In whatever form, the chronic and recurring issue has been and is being plagued by obscurity regarding the fundamental concepts that have been

used in formulating the issue itself and the rival positions about it. The most importantly obscure and ambiguous concept has been that of realism itself. It has been used in a number of varying and mutually inconsistent meanings in the course of the debate both by economists and by economic methodologists. There is a pressing need to bring clarity to the discussions about the issue.

METHODOLOGISTS AND THE ASSUMPTIONS ISSUE

As suggested above, the assumptions issue is both ubiquitous and central to economics, and plagued by serious unclarities. Given these two facts, one would expect that it has to be one of the major preoccupations of the specialists in economic methodology to analyse the issue and to clarify its elements. Surprisingly, one is disappointed in this expectation. Methodologists of economists have recently paid relatively little attention to this theme. There was a lot of more or less sophisticated discussion by economists (with some help from philosophers) in the 1950s and 1960s, mostly centred around elaborating various positions in regard to Milton Friedman's statement to the effect that the 'realism' of assumptions is irrelevant. One would have expected that the new generation of methodologists of economics entering the field from the mid-1970s onwards would have taken this issue as one of their primary concerns.

One reason for the failure of methodologists to contribute to the assumptions issue may be the dominance of Popperian frameworks in recent methodological study. Both the Popperian and Lakatosian varieties of falsificationism approach theory assessment in terms of the success and failure of testable implications. A closer scrutiny of the nature and role of assumptions – their logic, semantics and pragmatics – gets easily discouraged within this framework (with the partial exception of Lakatos's methodology of scientific research programmes which does have something to say on the pragmatics of assumptions). As a consequence, we are not very much wiser about the ways of the assumptions issue than our predecessors in the 1960s, even though much more effort has been invested in the methodological study of economics in the 1980s than during any other earlier decade (see Mäki 1990).

This is not to deny that a few important contributions have been published after the mid-1970s, such as Boland (1979), Musgrave (1981), Caldwell (1992), Lawson (1992) and Hausman (1992), to mention a few. Progress has not been absent, but relative to the recent investments in economic methodology, the fruit has been scant.

TWO APPROACHES TO THE ISSUE

A popular approach to the issue is to construe it in abstract terms and to look for generalized answers. The issue is taken to be one over whether, descriptively, the assumptions of a given economic theory are realistic or unrealistic, or whether, as a general normative principle, theories in economics should involve realistic assumptions, or whether, given the current situation, theories and assumptions should be 'more' realistic. A radical position along these lines has been formulated to read as follows: "Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions' (Friedman 1953: 14).

This abstract construal of the issue is often accompanied by the idea that the dividing line between people holding rival views concerning the desirability of realistic or 'more' realistic assumptions distinguishes those holding a *realist* position from those who are *non-realists* (instrumentalists, conventionalists, etc.). Accordingly, the idea goes, realists prefer realistic assumptions to unrealistic assumptions, while non-realists are either indifferent or have their preferences the other way around.

I argue that the above approach is not very helpful for understanding the assumptions issue. There is a need for reorientation. The alternative approach is different: the issue should not be construed as one of realistic versus unrealistic assumptions in the abstract but rather as one over which specific assumptions are and should be unrealistic or realistic, and over rival ways in which they are or should be so. It is understood that all theories are unrealistic in a number of ways and that the issue cannot be resolved in the abstract. A more concrete (more 'realistic'!) approach is needed to understand the nature of the issue in each specific case.

With this reorientation, it also becomes possible to understand that the advocacy of more or less realistic assumptions *per se* does not yet make anybody a realist or non-realist about economic theories. Both realists and non-realists may legitimately hold theories which are unrealistic in their assumptions.

This line of thought cannot be followed without an array of refined concepts. We need a few notions for distinguishing between different kinds of assumptions in different roles, and between a number of different ways of being realistic and unrealistic. I will provide a beginner's rudimentary guide to these concepts and to the overall argument (for more detailed formulations and discussions, see Mäki 1989, 1991, 1992a, 1992b, 1992c, 1993a, 1993b, 1993c, 1993d, 1993e).

THE UNAVOIDABILITY OF UNREALISTICNESS

Amongst economists, amusing illusions about physical sciences abound. Witness the following:

In physics the assumed premises are realistic. If there is evidence that they are not realistic, or not close approximations to reality, they will be rejected; and at every step the propositions derived from theory will be tested by experiment and observation: all propositions made are subject to the test of falsification. In general-equilibrium economics, by contrast, the assumptions are the extreme opposite of realistic. They are mad.

(Neild 1984: 42)

It is easy to provide evidence to the contrary. Take Boyle-Charles's law of ideal gases in classical thermodynamics. It states that $PV=RT$, where P is the pressure, V the volume, and T the temperature of a body of gas, while R is a constant. This statement is about ideal gases, which means that it may be formulated to assume that the volume of gas molecules is zero, that the forces of interaction between gas molecules are nil, and that the gas molecules are perfectly elastic. It requires a lot of imagination to say that these assumptions are realistic.

Or consider Galileo's law of falling bodies, an example much used by economists in the assumptions controversy. The law states that $s=\frac{1}{2}gt^2$, where s is the distance travelled by a body, t is time and g is the gravitational constant. Among other things, it is assumed here that air pressure is zero, i.e., that the body falls in a vacuum; that all other gravitational forces, such as that of the moon, are nil; that all magnetic forces are zero; that the radius of the earth is infinite, that is, that the earth is flat. Most of these assumptions are unrealistic all the time; all of them are unrealistic most of the time.

We may conclude that economics is not alone in involving unrealistic assumptions. Nor is neoclassical general equilibrium theory alone within economics. Consider Marx's law of value which states that the market prices of commodities correspond to their labour values. Among other things, it assumes that there is pure competition; that there is no foreign trade; that the merchants' profits are zero; that supply equals demand; that the average organic composition of capital in the sector producing the commodity equals the average organic composition of capital in the whole economy. Again, most of the time, such assumptions cannot avoid being unrealistic.

The important thing to note here is that the heavy reliance on unrealistic assumptions is not taken by scientists themselves as a sufficient reason to judge theories or laws either as unscientific or as failures as scientific

hypotheses. On the contrary, it is a ubiquitous feature of the most celebrated scientific theories that they contain unrealistic elements.

KINDS OF REALISTICNESS AND UNREALISTICNESS

We have so far used the attributes 'is realistic' and 'is unrealistic' as if they were unambiguous. They are not so, not in the least. Let us point out some typical meanings in which these terms are being used by economists and others.

Aboutness

A representation may be said to be realistic if it is about something real; it is unrealistic if it is not about anything real. For instance, it may be argued that the theory of phlogiston – or, more precisely, the concept of phlogiston in the phlogiston theory of burning – is not about anything real, since there is no such thing as phlogiston as a constituent of matter. On the other hand, the concept of oxygen is probably about a real constituent of the world. Galileo's law may be taken to be about real bodies and real gravitational attraction. The maximization assumption may be about households and business firms, provided there are such agents acting purposefully in social reality.

Observability

Some variants of realisticness and unrealisticness are related to the idea of observability. Constructs are sometimes regarded as realistic if they are about observable matters. There are those who insist that '[w]e must deal only with observable variables. To speculate about things you cannot observe is futile' (Neild 1984: 42). In a sense, this is a plea for avoiding unrealistic variables in favour of realistic ones. We know that Paul Samuelson's work on revealed preference was inspired by a principle similar to this. We also know that Newton wrestled with this issue when considering his notion of gravitational force. Yet, the postulation of gravitation, electromagnetic forces, black holes, photons, quarks and other unobservables is regarded by scientists and philosophers of science as one of the key reasons for the success of science. Unrealisticness in this sense is vital for science.

Truth

Truth and falsehood are obvious forms of realisticness and unrealisticness. In some discussions about the assumptions issue in economics, they are the only forms (e.g., Brunner 1969; Boland 1979). It is probably false to assume that the pull of the moon does not have any impact on bodies

falling within the gravitational field of the earth, but it may be true to state that the latter does exert an influence expressed by g . It is not true that economic agents have perfect information, and it may be true or it may be false that they maximize in some sense.

A theory or statement has to be realistic in the sense of being about something real – but not necessarily about something observable – in order to be true or false about that something. Truth and falsehood presuppose aboutness.

Success in empirical tests

A representation may be regarded as realistic if it is testable and well confirmed by evidence in empirical tests. One may say that a theory or statement fails to be confirmed by evidence, hence is unrealistic, either because the appropriate test conditions cannot be established or because the evidence is negative in cases where the test conditions are appropriate. Bearing in mind that appropriateness is a contestable notion, we may expect that the travel of a feather in actual atmosphere does not, while the travel of a cannon-ball does, support Galileo's law; for the feather, a vacuum would have to be created. In both cases, many other forces cannot be removed, and the earth cannot be made flat. In economics, a traditional controversy has been over whether the maximization assumption is testable at all, and if it is, what would be an appropriate empirical test of it.

Truth and confirmation – and falsehood and disconfirmation – are sometimes confused with one another (e.g. Nagel 1963; Brunner 1969). However, evidence may speak against a true statement or for a false statement, or there may be no appropriate evidence at all for or against a true – or false – statement.

Plausibility

Truth is sometimes confused also with plausibility, and falsehood with implausibility. Here we have yet other meanings for our key terms. A representation is realistic in one sense if it is plausible, and unrealistic if it is implausible. Plausibility is a matter of being believed by people. (For discussions of plausibility in the context of economics, see Hirsch and de Marchi 1989; Nooteboom 1986; Mäki 1993a.) Some time ago, the assumption of the infinity of the radius of the earth used to be very plausible in relation to the vast majority of humankind: people did believe that the earth is flat. For some time now, it has been found an extremely implausible assumption.

Partiality

A concept, statement or theory is often regarded as unrealistic if it is partial, if it isolates only selected aspects of objects for representation. Galileo's law is unrealistic in this sense, since, among other things, such as omitting the colour of falling bodies, it focuses on the influence of only one factor on the behaviour of the bodies, to the exclusion of others. The maximization assumption not only omits mentioning the shoe size of economic actors, it also excludes other possible motives from consideration. One-sector models in growth theory and 2×2 models in the theory of international trade are prime examples of partial representations. Marshallian analysis is partial in excluding, for instance, cross-elasticities between markets, while Walrasian analysis is partial in excluding culture and gender, for example. All representations are partial in that they isolate small slices of the world from the rest of it.

Sometimes, partiality is confused with falsehood. While it is true that partial representations violate 'the whole truth', it does not follow that they therefore also violate 'nothing but the truth'. A representation may be true or false about a part of a complex whole (see Mäki 1993c).

Abstractness

Abstractness is a special case of partiality. A representation is abstract if it isolates a general feature or a universal from the particularities of the many objects that share it. The concept of the business firm is such an abstract notion, while the concept of, say, the Nokia Corporation is a concrete one. The concept of a falling body is abstract, while 'Hemmo Huimapää' is the name of a particular parachutist and hence concrete.

Practical usefulness

A representation is often regarded as realistic in one sense if it serves well the pursuit of some practical ends. Realisticness in this sense is relative to the specific practical ends at hand. The formulae of atomic theory are practically useful for attempts to fly to the moon, while they are useless, hence unrealistic – some might say as unrealistic as the formulae of the Arrow–Debreu construct – for manipulating the rate of unemployment in your economy. Galileo's law may be useful for destroying your enemy with a cannon, but it is relatively useless for controlling the travel of a feather to delight your baby.

TYPES AND FUNCTIONS OF ASSUMPTIONS

It is vital for dealing with the assumptions issue to understand that economic theories involve many types of assumptions with a variety of functions. This has been recognized by earlier commentators on the issue (see, for example, Machlup 1955; Rotwein 1959; Melitz 1967; Brunner 1969). Within the set of assumptions that are elements of versions of a theory it is helpful to distinguish between assumptions that are taken to be central to a theory and those that are regarded as less central, or between those that constitute the theory and those that do not. Let us call these two classes 'core assumptions' and 'peripheral assumptions'.

Core assumptions

Galileo's law involves the statement that bodies are attracted by the gravitational field of the earth, measured by parameter g in the formulation of the law. If it were called an assumption, it would be a prime example of a core assumption. It serves to sort out what is believed to be the most central force influencing the fall of bodies. Even more, it denotes a fact that is believed to constitute the essence of falling.

The assumption that agents maximize may be regarded as a core assumption in much of economics. It is central to the most popular economic theories, and many economists believe that the constrained strive for maximum outcomes is the most important motive force influencing agents' behaviour. Many of them think that maximizing constitutes the essence of economic behaviour.

Peripheral assumptions

In Galileo's law, the assumptions of vacuum and the absence of other attractive forces serve as peripheral assumptions. They serve to neutralize factors that are not regarded as central or essential to the phenomenon of falling bodies. In economics, assumptions such as closed economy, instantaneous and costless transferability of resources, perfect divisibility of goods and factors, homogeneous capital, full use of resources, constancy of tastes and technology, and the general *ceteris paribus* clause play a similar role. Typically, many such peripheral assumptions are false.

Alan Musgrave (1981) has suggested a typology of assumptions to deal with the issue of realisticness. He distinguishes between three types of assumptions and calls them 'negligibility assumptions', 'domain assumptions' and 'heuristic assumptions'. It seems that the way he characterizes them implies that they are to be treated as subspecies of peripheral assumptions. A few notes will suffice to clarify the typology (for a detailed critical analysis, see Mäki 1994).

Negligibility

In this case, an assumption is formulated so as to function as a statement about the negligibility of a certain factor. The assumption that the pull of the moon is non-existent is false and as such helps to isolate a non-negligible force, the gravitational field of the earth. Reformulated as a negligibility assumption it may turn into a true statement that the pull of the moon has a negligible effect on falling bodies. Similarly, the assumption that a given economy is closed may be false, but reformulated as a statement that the impact of foreign trade on certain phenomena is negligible, it may be true.

Applicability

Sometimes, some of the assumptions of a theory may be used to specify the domain to which the theory can be applied. They serve as statements about applicability. In some cases, the assumption of a vacuum may play this role: Galileo's law applies only if air pressure is nil. Similarly, an economic hypothesis may be argued to be applicable only to economies that are closed. Empirical applicability is a prerequisite for testability which in turn is presupposed by being well confirmed by empirical evidence.

Early step

Some assumptions are used as elements of an early step in a series of theories or models. Closed economy models may function as early steps preparing the way to open economy models. The assumption of the zero-ness of the pull of the moon may in some cases serve as an early-step assumption, to be replaced in later versions of Galileo's law by an assumption giving an account of the specific impact of the moon on falling bodies.

The idea of early-step assumptions (Musgrave uses the term 'heuristic assumption') is often construed as a promise of increasing realisticness as a theory develops. In some senses of the term, realisticness would indeed increase. The comprehensiveness of a theory would increase and its partiality would decrease as new factors are incorporated into it. This is sometimes accomplished by relaxing assumptions that are utterly false and replacing them by other, later-step assumptions, that are true or closer to the truth. Furthermore, a theory which takes into account a larger set of factors is often more successful in empirical tests and then also in this sense more realistic. In economics, however, it is typical that only a few steps are taken in this direction (see Lind 1992).

THE FUNCTIONS OF UNREALISTICNESS

Now that we have an idea of kinds of unrealisticness and types of assumptions, we can better understand a line of thought that may be used for justifying unrealistic elements in economic theory. Consider Galileo's law first. It is unrealistic in that it is partial. It is unrealistic also in involving assumptions that are mostly false. It denotes gravitation which is unobservable. These unrealisticnesses serve one and the same purpose, that of isolating a central force influencing the behaviour of falling bodies. A number of unrealistic peripheral assumptions are used for neutralizing what are believed to be peripheral factors in order to focus on what is believed to be the most important factor. The core assumption, concerned with the contribution of the earth's gravitation, purports to be as close to the truth as possible.

The situation is similar in economics. Unrealistic peripheral assumptions help isolate what are believed to be the fundamental relations from less relevant ones or the major causes from the minor causes of phenomena studied. As Oliver Hart puts it, '[t]hese models, since they concentrate on one issue, tend to make simplifying and hence often unrealistic assumptions about everything which is not the central focus. . . . Any theory, if it is to get anywhere, must abstract from many (even most) aspects of reality' (Hart 1984: 48). The same idea can be found in Friedman's 1953 essay. Let me cite my favourite two passages:

A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure.

(Friedman 1953: 33)

Based on this principle, Friedman's maxim of theory formation prescribes that we should 'abstract essential features of complex reality' (ibid.: 7).

The core assumptions are supposed to capture, in pure form, the 'essential features' or 'the more fundamental structure', while the peripheral assumptions, such as negligibility and early-step assumptions, are there to help see the essence of the matter undisturbed by eliminating the actual disturbances or complications. Friedman's mistake was to defend the core assumption of profit maximization by appealing to an analogy between it and the vacuum assumption, which is a peripheral assumption. The correct analogy would be between it and the core assumption of the gravitational attraction of the earth (see Mäki 1992b).

While it is often the case that unrealistic peripheral assumptions can be justified as devices for eliminating or neutralizing minor factors so as to bring in brighter light the major factors, this does not always have to be

the case. Sometimes the only justification appears to be an increase in formal tractability, the facilitation of proofs within a pre-given formal framework. It is not always easy to tell whether the ground is ontological, having to do with the presumed structure of reality, or more purely pragmatic, related to the manipulability of formal systems. And, of course, the relevance of various aspects of the social context of theorizing for the outcome of theorizing has to be acknowledged.

ANTAGONISMS AND FAMILY QUARRELS AGAIN

We may say that in an antagonistic controversy, core assumptions are questioned. If it is suggested that gravitation be replaced by angels or that maximization be replaced by routines, we have examples of antagonism. Rival claims about the most essential features of the domain of study or the major causes of phenomena to be explained are confronted.

In family quarrels, peripheral assumptions are challenged. One may suggest that for certain falling items, the absence of a vacuum is not negligible and that a vacuum has to be assumed as a prerequisite for empirical applicability; or that the pull of the moon has to be incorporated into the equation; or that it is time to take the next step in the series of economic models by relaxing the closed economy early-step assumption and by incorporating foreign trade and international capital movements.

As said earlier, it is not always easy to agree on which statements are to be treated as the core assumptions and which as the peripheral assumptions, that is, where the stakes are highest and where they are lowest.

Domain or applicability assumptions do not seem to conform neatly to the above rule. Sometimes, there prevails a sort of peaceful coexistence and division of labour between different theories or models. The total domain is divided between them, and unanimity about domain assumptions obtains. Each theory or model is only applied to its agreed-upon respective domain. In other cases, theories make rival claims about one and the same domain as answers to one and the same question. The stakes may then be high in challenging the respective domain assumptions.

REALISTICNESS AND REALISM

Economists usually talk about the 'realism' of their theories and assumptions. This easily misleads them to think that those who favour more such 'realism' in theories are advocates of realism as a philosophical doctrine, while those who are content with unrealistic assumptions are non-realists. In order to avoid this confusion, I have suggested that 'realism' and 'non-realism' be reserved for denoting a variety of philosophical theses, and that 'realisticness' and 'unrealisticness' be adopted for denoting various

properties of linguistic and other representations such as economic theories and their parts (Mäki 1989).

Once this terminological convention is accepted, it becomes easier to see that the use of radically unrealistic assumptions does not commit one to non-realism. A realist economist is permitted, indeed required, to use unrealistic assumptions in order to isolate what are believed to be the most essential features in a complex situation (for the whole argument, see Mäki 1993e). To count as a minimal realist, an economist is required to believe that economic reality is unconstituted by his or her representations of it and that whatever truth value those representations have is independent of his or her or anybody else's opinions of it.

INTERLUDE: REORIENTING THE ISSUE

The controversy over the assumptions of economic theories has often been construed as one between those who are in favour of realistic or at least more realistic assumptions as against those who are satisfied with unrealistic assumptions. It is one of the implications of the above suggestions that I find this construal of the controversy 'unrealistic' in the sense of being oversimplified. Since all theories contain unrealistic assumptions, the real issue can be construed as one about the *substance* of those theories and assumptions, namely *what they exclude* as supposedly irrelevant or inessential and *what they include* as allegedly relevant or essential, and *what they say about* the included items. The issue is one over rival conceptions of what Friedman termed the 'more fundamental structure' of the economy.

FOUR ILLUSTRATIONS

Let us briefly illustrate the idea with four major issues in recent economics. They are the issues of whether the Keynesian or the monetarist approach managed to focus on the crucial factors, whether to include the role of positive transaction costs in one's theory, whether to put the emphasis on equilibrium states or on processes of change, and whether to begin the construction of economic theory with an analysis of exchange or of production.

Friedman and Keynes

Take first Milton Friedman's account of the opposition between his monetarist approach and that of Keynes. We know that Friedman does not find certain kinds of unrealisticness in theories problematic; on the contrary, in his opinion theories have to be unrealistic to perform their task properly. On this, he finds himself in agreement with Keynes:

Of course, his assumptions were not in literal correspondence with reality. If they had been, he would have been condemned to pedestrian description; his whole theory would have lost its power. . . . I believe that Keynes's theory is the right kind of theory in its simplicity, its concentration on a few key magnitudes, its potential fruitfulness.

(Friedman 1972: 908)

Friedman locates the ultimate disagreement elsewhere, in *what* the rival theories say about the structure of economic reality:

I have been led to reject [Keynes's theory], not on these grounds, but because I believe . . . that it has not isolated what are 'really' the key factors in short-run economic change.

(Ibid.)

Friedman considers that Keynes did try to isolate the 'key factors', but that he ended up with excluding what Friedman would find the most essential factor.

The heart of the General Theory is an extremely simple hypothesis – that a highly unstable marginal efficiency schedule of investment and a liquidity preference function that is highly elastic at low rates of interest and unstable at higher rates of interest are the key to short-run economic movements. That is what gives investment its central role, what makes the consumption function and the multiplier the key concepts, what enables Keynes to develop his theory for 165 pages without having to introduce the quantity of money.

(Ibid.)

As we know, the monetarists insist on isolating the quantity of money as the key element in the short-run behaviour of the economy. Friedman thinks that he and Keynes agree that it is the task of theory to isolate the essential or 'key factors' and to exclude the less important items in social reality from theory. Friedman argues that Keynes's theory failed in performing this task, rather than in being (too) realistic or (too) unrealistic.

Transaction costs and institutions

Consider then the role of transaction costs in economic theory. Traditional neoclassical theories contain the false idealizing assumption that transaction costs are nil, that is, that the economy functions 'frictionlessly'. These theories isolate production costs as the relevant cost category. This helps isolate certain relations from the influence of positive transaction costs. These theories are unrealistic in a double sense at least, in containing a false assumption and in failing to encompass the role of one real feature

of the economy. It may be held that those isolated relations constitute the most fundamental structure of the economy and that therefore the exclusion of transaction costs promotes the pursuit of true accounts of the essential features, transaction costs being among the inessential ones.

On the other hand, others argue that the exclusion of positive transaction costs not only is based on a false assumption about their non-existence, but also serves to eliminate an essential factor from our picture of economic reality, namely institutions or organizational structures. It therefore leads to 'blackboard economics' (Coase 1988: 19) which is 'remote from the real world' (ibid.: 15). The assumption is therefore not innocently false. For many purposes, it would be false also when construed as a negligibility assumption. If it is used to specify the domain of applicability, it appears that the respective theory and its theorems – such as the standard neoclassical allocative theorems – do not apply to actual economies (cf. Coase 1960). The standard neoclassical isolation can be argued to divert the focus of theory away from some of the essential features of the economy. For instance, the depiction of business firms as production functions, based on the idealization of frictionlessness, may be taken to divert the attention from what is essential for major explanatory purposes, captured only by depicting firms as governance structures. Thus, 'the modern corporation is mainly to be understood as the product of a series of organizational innovations that have had the purpose and effect of economizing on transaction costs' (Williamson 1983: 1537; see also e.g. Williamson 1985; North 1990).

Now it is clear, as many critics have pointed out and as acknowledged by its advocates, that any form of transaction cost economics is bound to be unrealistic itself; it has to exclude much and it has to idealize and simplify much. For instance, its standard forms exclude from consideration the role of technology and concrete social relations, items that are identified as the key factors in other theoretical orientations. Yet, it is by using these exclusions and idealizations that transaction cost economists can maintain that they have isolated what they find a fundamental factor in the economy. (On this interpretation of the issue, see Mäki 1992c.)

Equilibrium states and processes of change

Take next the issue about assumptions of knowledge and equilibrium. There is the widely used but presumably false idealizing assumption that the agents have full relevant information, that there is nothing to learn. This falsehood helps isolate equilibrium states to the exclusion of processes of change. As Robert Lucas states,

[e]conomics has tended to focus on situations in which the agent can be expected to 'know' or have learned the consequences of different

actions so that his observed choices reveal stable features of his underlying preferences. We use economic theory to calculate how certain variations in the situation are predicted to affect behavior, but these calculations obviously do not reflect or usefully model the adaptive process by which subjects have themselves arrived at the decision rules they use. Technically, I think of *economics as studying decision rules that are steady states of some adaptive process*, decision rules that are found to work over a range of situations and hence are no longer revised appreciably as more experience accumulates.

(Lucas 1987: 218; emphasis added)

On the other hand, those wishing to focus on phenomena of change are critical of the assumptions picturing the maximizing agent: 'Strict adherence to optimization notions either requires or strongly encourages the disregard of essential features of change . . .' (Nelson and Winter 1982: 31; cf. p. 94). To the above statement by Lucas, Sidney Winter responds by making a case for a diametrically opposite position.

To be willing to limit the aspirations of economic science to the study of the steady states of adaptive processes is presumably to view vast realms of apparent rapid change as either unimportant or illusory; it is to join with the writer of Ecclesiastes in maintaining that 'there is no new thing under the sun'. I, on the other hand, side with Heraclitus in arguing that 'you could not step twice into the same river, for new waters are ever flowing on you'. It is the appearance of stability that is illusory; just look a little closer or wait a little longer.

(Winter 1987: 245–6)

Winter here comes to formulate the issue as one between two rival claims as to the essential truth about the economy. Either change is regarded as illusory, or stability is. Both equilibrium and process theories are based on isolations and therefore involve unrealisticness, yet can be used to pursue allegedly realistic accounts of what are believed to be the essential features in the object of study. (For qualifications, see Mäki 1993b.)

Exchange and production

As a final example, consider the historically significant dividing line between approaches building upon models of exchange and those beginning with pictures of production. One set of theories focuses on preferences and the allocation of given resources through exchange, while the other approach puts stress on the use of labour in the production process. The first kind of theories are expressions of catallactics, while the second manifest the plutological approach (Hicks 1976). As Baranzini and Scazzieri suggest,

'[i]n the case of economics, an initial concentration of attention on certain aspects of exchange or production, respectively, often led to the formulation of "ideal" models of the economy in which what is essential in one model appears to be of secondary importance, or altogether irrelevant, in the other model' (Baranzini and Scazzieri 1986: 5). Thus Ricardo built upon

the assumption that producibility rather than scarcity is the dominant feature of a modern economy. Utility and scarcity are excluded from Ricardo's ideal model of the economy, as well as from his theory of value . . . [whereas] Jevons's 'ideal' model is a pure allocation economy in which both scarcity and utility play a crucial role.

(*ibid.*: 6–7)

We may say that what the authors call the two 'ideal models' of the economy, are based on early-step assumptions that help exclude either production or exchange from consideration. Again, the clash between the two traditions is not one between classes of realistic and unrealistic theories but rather between rival claims of factors that are found as dominant or of primary importance for the functioning of the economy.

COMMENTS ON THE ILLUSTRATIONS

Attempts to classify the above controversies as antagonisms or family quarrels are bound to be challengeable, while it may be easier to measure their loudness at any given period of time. Yet, it would seem possible to say that some of them, such as the production versus exchange and equilibrium versus process debates, are closer to antagonisms than family quarrels. No mere peripheral assumptions are at stake. As for the monetarist controversy, even though the debate between the Keynesians and the monetarists was relatively loud, Friedman made the attempt to construe it as a family quarrel by suggesting that both positions can be formulated within one and the same framework; as we know, the attempt is controversial itself. It is also problematic to decide whether the assumption of zero or positive transaction costs has a peripheral status even though it has major implications concerning whether institutions will be included or excluded; those who suggest that transaction cost economics is just another variant of neoclassical theory imply that it is a peripheral assumption, hence the controversy between the two classes of theory would be just a family quarrel. All such judgements depend on prior, explicit or implicit, and always contestable, distinctions between core and peripheral assumptions.

It is another difficult question whether there are any grounds for denying or granting forms of realism (as distinct from realisticness) in the case of any given theory or approach. Could it be that in order to count as a realist, an economist is not allowed to exclude certain entities (such as

institutions and processes?) from theoretical consideration? To answer such questions, much more concrete analyses are needed than has been customary in methodological debate (for a suggestion, see Mäki 1993b).

CONCLUSION

In all four cases and numerous others similar to them, the issue is not about realisticness versus unrealisticness in the abstract; each of the rival approaches produces theories and models that are inescapably unrealistic. The issue is rather about the functions of unrealisticness in the orientation of theorizing, either driven by ontological considerations as to how to draw a line between what is believed or hypothesized to be essential and what is believed or hypothesized to be inessential in the economy, or driven by pragmatic considerations of formal tractability, without forgetting about the social conditioning of theorizing such as economists' pursuit of intellectual credibility within the economics profession with current fashions taken as given (on this last point, see Mäki 1992d).

The argument should not be mistaken for a legitimation of all kinds of unrealisticness in any parts of the structure of any economic theory. The argument suggests that the basic issue should be reconceptualized and relocated. The issue over assumptions should not be construed as one over realisticness in the abstract but instead as one over the functions of specific kinds of realisticness and unrealisticness and the lack thereof in the context of concrete theories, that is, over what is included and what is excluded by each particular theory and framework. This is also how practising economists seem to construe the issue. This is evidenced by statements to the effect that 'in economics the wrong things are often, nay, usually, abstracted from; and the *ceteris paribus* clause often includes the very variable that should be the main object of research' (Wiles 1984: 308).

The task for economists and economic methodologists then is to develop principles that could be used for assessing and choosing between rival claims to realisticness based on theories that involve unrealistic ingredients. We cannot simply follow rules such as that of choosing the theory that appears more realistic than its rivals in being more encompassing or in containing fewer false assumptions. We need principles for assessing theories on the basis of how close they come to capturing the essential aspects of the economy for given explanatory purposes.

One response to this need is to appeal to the predictive success of theory. This is what Friedman seems to be suggesting when he says that 'this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions' (1953: 15). The problem with this suggestion is, of course, that simple predictive success is not always very reliable in this role, not even in sciences that have in

fact indicated remarkable predictive capabilities. A classical example of this is, in the sixteenth century, the predictive superiority of Ptolemaic geocentric astronomy over its young challenger, Copernican heliocentrism, even though the latter was decisively closer to revealing the fundamental structure of the planetary system. The difference between Copernican theory and economics, however, is that the former has indicated predictive progress, while the occurrence of such progress is controversial in the case of economics (see Rosenberg 1992). It may be that we cannot base the appraisal of economic theories on considerations of predictive power only. Other principles seem to be in operation, and other principles may have to be put into operation to get what we want. Much work remains to be done in the articulation of such principles.

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