Intermezzo

We have seen that Böhm-Bawerk aimed to make scientific progress after Classical Economics. He tried to do so by theoretical unification, that is by conceptual and explanatory unification. He also tried to choose the ‘right’ level of complexity of the explanans in order to leave nothing unexplained of what should be accounted for.

In what follows I want to show that Böhm-Bawerk’s endeavour to theoretically progress raises questions concerning (1) the empirical content of economic theories, (2) the prior convictions economists can be assumed to have in this endeavour, (3) the feasibility of explanatory progress in contrast to predictive progress, (4) the role of abstraction in economic method and (5) the existence of economists’ essentialist convictions.

These points have been discussed in the previous two chapters as well. But they have significance beyond the case of Böhm-Bawerk and even beyond economics. They are not restricted to the idiosyncratic case of these chapters, which talk just of a particular economist’s way of developing his research program in the history of Austrian Economics. In this intermezzo I shall glance over points (1) to (5) in a perspective that is broader in two ways: broader than that of an interest merely in Böhm-Bawerk and Wicksell and broader than economics as a social science. First, I believe that the five issues bear on the development of conceptual apparatus of economics even today. But secondly, if this is true, it is evident that the role of economics as a study of (an aspect of) the social is being contested; certainly when its important role as producer of policy recommendations is concerned.

This intermezzo serves as the path toward the chapters III to V, which discuss these matters more in depth. All three chapter to come are relevant for these broader perspectives.

1 Empirical progress?

Many jokes about economics mock its alleged poor predictive record and its use of strong assumptions such that it applies only to very special cases, which will never occur in reality. Both allegations attack the irrelevance of economics as a source of fruitful (economic) policy recommendations. At the same time, many research institutions emit studies with predictions on the basis of economic indicators. Generally, economists produce predictions about economic variables with much more precision than sociologists do with regard to other social variables. This is one of the causes of economics being classified as ‘the queen of the social sciences’ (be it that this appraisal is sometimes uttered with intended irony). If economics as a discipline does so badly in prediction, it is a mystery why it does so well in the market of advice and counselling.
Many economic predictions go with a wide margin of error, which increases with the length of the period the prediction applies to. This makes economics an easy target for the joke that ‘God created economists in order to make weather forecasters look fine’. Indeed, many similarities between economics and meteorology strike the eye, including this curious mixture of public mockery and acceptance of its predictions. But economic prediction is not only in demand by the public for practical reasons, it is also a scarce good for economists themselves. Predictions are needed to test theoretical hypotheses.

However, Neil de Marchi has noted that economic theory alone yields few unambiguous predictions. Economics has developed purely theoretically since the eighteenth century as ‘political economy’ and until deep in the twentieth century economists did not deduce testable hypotheses at all. De Marchi therefore observes that ‘Popper’s critical rationalism actually involves a change of style in terms of the way economists engage in debate’ [over the comparative appraisal of two competing theories]. His history of the atmosphere at the London School of Economics in the nineteen sixties depicts how after the publication of the translation of Popper’s (1934) _Logik der Forschung_, in 1959, the LSE economists wanted to derive testable hypotheses from their theories, for instance in order to compare their success relative to Chicago economic theories. It turned out very difficult, DeMarchi concludes. Problems concerning the derivation of empirical tests had moved into the focus of attention in London already over the fifties, after Milton Friedman’s (1953) _The Methodology of Positive Economics_ had stressed testability as well. Furthermore, for qualitative predictions – predictions as to the direction rather than the size of the change of a variable – one needed measurement of quantities. As causal influences work in opposite directions, the specification of the direction of the net outcome that theorists are interested in involves quantification.

The want for testability and for quantification came with the insistence on these practices by the LSE professors Archibald and Lipsey, respectively. DeMarchi contends that Popperian philosophy ‘has no compelling answers’ to either need. The Duhem-Quine problem of underdetermination in case of falsification and the prior need to specify a model that fits the problem context stood in the way of testing. The specification of error terms stood in the way of quantification.

The tools both for testing and for quantification come from econometrics. According to Bert Hamminga, econometricians maintain a remarkable relationship with theoretical economists. Econometric modelling, the argument goes, aims to tract and identify variables in data sets. It requires very strict specifications while, in contrast, economic modelling seems to engage in a loosening of the specifying labour. Hamminga pretends to prove the autonomy of each discipline relative to the other (called ‘the mutual independency thesis’; see below for a discussion). The discipline that develops the fundamental theories and the discipline that enables

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2 Although Lipsey has a more positive memory of this episode (personal correspondence).
3 Ibidem, p.140. DeMarchi borrows this insight from Harry Johnson.
testing are said to stand isolated. What the one does is not recorded by the other. He argues that, at least in the case of international trade theory:

neither some kind of realism [realisticness] of some kind of assumption nor
the conformity of predictions to reality, play a fundamental role in theory de-
velopment.¹

If this is true, doubt is cast on the predictive record of at least the neoclassical economics of international trade. Hammninga researched trade theory from the factor price equalisation theorem⁵ to the Stolper-Samuelson theorem⁶.

2 Progress by appeal to prior plausibility?

‘Every Ohlin-Samuelson programme participant seems to “have” a System of Elementary Plausibility Convictions’ (SEPC), Hamminga says.⁷ These SEPC differ, both among economists and among points in time of theory development. Clearly, SEPC are a priori. They direct research in a context of discovery. Very little is said about measurable economic processes although the SEPC always have some relation to the ‘real world’. Hamminga phrases a first, tentative approach to interpret the notion of ‘the plausibility of a theorem’ as

the probability of the theorem being true in the “real world”, where the “real
world” is just one of the worlds that can be expressed by means of the econom-
ists’ language (production functions, utility functions, factor endowments).
If a theorem holds in many worlds that can be expressed in the economists’
language the probability of the theorem being true in our real world is high,
even without considering at all what our real world is exactly like.⁸

Purely mathematical methods can be employed so as to increase the plausi-
bility of an economic theorem, because a proposition of economic theory is a mere
metatheoretical provability theorem. Kuipers concludes:

[Hamminga’s] diagnosis of the mathematical nature of economics may be an
important underlying motive for the striking ambivalence of economists
about the question of whether economics is an empirical social science or not. Moreover, the diagnosis illustrates that the cognitive aims of the social sci-
ences in general and of economics in particular appear to be less evident than
philosophers of science use to assume on the basis of an analogy to the natural
sciences.⁹

In his study, Hamminga describes four strategies of theory development in in-
ternational trade theory, none of which has anything to do with testing competing hypotheses. Therefore his example shows that progress in theoretical economics sticks to delivering fundamental explanatory hypotheses in a way which is quite

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² In consequence of free trade the same factor of production in different countries will be rewarded
   equally.
³ International trade lowers the reward for the relatively scarce (more expensive) factor in a country.
   Hence, a tariff benefits the scarce factor.
⁹ Kuipers (2001), p.34.
disobedient to Popperian methodology. All four strategies build on alterations of Propositions of Economic Theory (PET). A PET describes an implication with a field and a number of conditions in the antecedent and a so-called Interesting Theorem (IT) in the consequent. In Hamminga’s somewhat informal way, a PET has this form:

PET: Field, conditions, → IT

This formulation makes it clear that the PET is metatheoretical. I shall now give a brief review of each ingredient of the conditional. Before that, let me just give an example. A PET can be the meta-theorem that the Stolper-Samuelson IT (that the scarcest factor will be remunerated less after trade opens) can be proved on the domain of two countries, two goods and two factors, given the special conditions that production functions are homogenous to the degree one and that the factors are immobile between the two countries.

In the consequent of the PET we find the IT. This is the intended implication of the conditions. For example, the factor-price equalisation theorem is an IT and the Stolper-Samuelson theorem is another IT. Economists try to formulate such theorems as surprising or at least new claims under conditions that are ‘plausible’. A theorem is interesting if it influences discussions among economists or among the public when economic policy measures are at stake.

Let me start with the antecedent of the PET. The field is the domain of the IT. It is specified as c countries, f productive factors, and g goods: F_{c-f-g}. One strategy of theory development by the alteration of the PET is field extension. If the IT can be proved for a ‘F_{2-2-2}’, and next also for ‘F_{2-2-3}’ (or for F_{c-f-g}, 2 < c, f, g ≤ z; z being any reasonable number in terms of the number of countries, goods and factors as we generally encounter these in the actual world), it has become more interesting, because more plausible. Intuitively, this increased plausibility is related to the fact that countries in fact do not trade bilaterally only, certainly not in only two goods, and that productive factors are many (taking land, human capital, and risk taking into account). The quest for ‘field plausibilism’, then, is one strategy to progress.

The conditions in the antecedent are (1) the set of fundamental (neoclassical) convictions and concepts by which the programme is governed and (2) any number of special conditions. The convictions are called Fundamentals of Economic Analysis (FEA), together with some Explanatory Ideal (EI). The special conditions are for example strictures on slopes of functions, or on the available endowments.

The FEA are, it seems to me, much like symbolic generalisations and metaphysical paradigms of the Kuhnian disciplinary matrix. For instance, we have encountered the FEA of what I shall loosely call ‘the classical programme’ in the previous two chapters. The classical programme is what Böhm-Bawerk attacked in his critique of the Kostengesetz, that prices (and costs) reflect values and these in turn reflect the (labour) costs of production. The ‘explanatory ideal’ (EI), in turn, is

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10 The arrow ‘⇒’ can be read as a material implication here. Hamminga does not discuss the problem that a false antecedent leads to a true implication trivially. See below.
a term from Toulmin’s (1972) Human Understanding. Hamminga characterizes it in a way that makes it fit into what Kuhn called values (on how to solve puzzles) and exemplars.\textsuperscript{11} Indeed, although Hamminga does not explicitly equate his circum-
scriptions to these distinct elements from the disciplinary matrix, he compares entire paradigms (in the sense of ‘group commitments’ as a whole) with the combination of FEA and EI.\textsuperscript{12} The classical EI was the canon answering the question why countries engage in trade instead of being autarkic. Ricardo had said that this was due to differing labour productivities (and therefore differing end prices) for different end goods in the respective countries. The neoclassical EI is opposed to this: assuming identical production functions, but differing factor endowments. One country would produce more labour intensively than the other.

The three remaining strategies for theory development are all ways of looking for ‘conditions plausibilism’. Hamminga distinguishes weakening of conditions, replacing conditions by other conditions with deeper concepts, and constructing alternative special conditions such that the former cannot be deduced from the latter or vice versa. These strategies differ but I shall discuss weakening only.

The conditional PET is strengthened if the antecedent is weakened. The decreasing strictness on the number of countries, for example, comes with more plausibility, as many more countries than two actually do engage in trade.\textsuperscript{13} Another strategy is weakening of the special conditions. These are substituted by a new set of special conditions that can be derived from but are not equivalent to the old set. One result of the Stolper-Samuelson investigations was the theorem that the scarce factor in a country would benefit from an import tariff, under the rather special conditions that one country is small and the other big (in terms of domestic income) and hence that the terms of trade are constant.\textsuperscript{14} It could be shown that these conditions rested on particular underlying conditions concerning factor endowments, utility functions and production functions. Hamminga compares this form of progress with the description of the earth as the centre of gravity for the

\textsuperscript{11} Hamminga equates the FEA with a ‘world view’. I do not see why he excludes the EI from the reference of this concept. Kuhnian exemplars are standard ways to solve puzzles. As the paradigmatic ‘world view’ includes exemplars, it should include the entire FEA-EI couple.

\textsuperscript{12} Hamminga (1983), pp.123-126. He notes that at a more specific level than that of an entire world view, a description of economic research deviates from Kuhn’s very typical natural science examples. In a normal science period, physicists for example do not look for so-called ‘Interesting Theorems’ or for (the conditions that are true in) hypothetical worlds in which these theorems are true. In revolutionary science, new paradigms give a blow to old ones due to the prediction of new phenomena, but the trade theory programme did not look for (new or old) real phenomena at all. Hamminga lists more differences.

\textsuperscript{13} Hamminga finds many allusions to the notion of ‘plausible’ in what he calls ‘a fountain of expressions’ as economists employ these. Take for instance the likelihood that there are more goods than factors of production; or the conditions having or lacking economic meaning. In the case of ‘lacking meaning’ they use terms like pathological, artificial, and implausible.

\textsuperscript{14} A small country is one which cannot influence world market prices or the ‘terms of trade’. In fact, this is one of the special conditions.
moon orbit first under the special conditions that the moon does not also attract the earth, and in the next step assuming that it does.\textsuperscript{15}

I shall restate Hamminga’s account with the help of the following figure. Note, meanwhile, that IT’s receive their plausibility from the conditions from which they are derived. Weaker conditions are more plausible than stronger conditions. The condition that countries do not specialize but diversify in production is more plausible because it is weaker: its extension is wider, more possible worlds satisfy it. At the same time, if conditions are weakened, the PET becomes stronger (and the IT is more interesting\textsuperscript{16}).

PET-C is the conditional $C\rightarrow IT$ and PET-C* is the stronger $C^*\rightarrow IT$. In the figure, the box represents all logically possible worlds. The worlds where conditions $C$ and $C^*$ are the case form subsets of these logical possibilities. $C = C_1 \ldots C_j$, i.e. the FEA-, EI-, and special conditions that lead to the IT, are itself true of worlds in the subset denoted $C$ in the figure. The weaker the conditions are, the stronger the conditional (the PET). $C^* (= C^*_1 \ldots C^*_k)$ is weaker than C, so $C^*$ has a greater extension than C, it excludes less. The following holds:

\textsuperscript{15} See Hamminga (1983), p.52. New conditions with regard to demand and import elasticities were introduced by Lloyd Metzler in 1940. They were weaker, because they allowed that an increase or decrease in trade altered the rate of exchange in both of two countries instead of only in one of them. The original Stolper-Samuelson result turned out to be a special case of the Metzler result.

\textsuperscript{16} Note that the IT need not be more interesting due to this. Some IT’s are not economically meaningful, even though the PET may be very strong.
\[ C \subseteq C^* \rightarrow [[\text{PET-C}]] \supseteq [[\text{PET-C}^*]] \]

(The symbol \([[ \ldots ]]\) here means ‘the extension of …’, i.e. \([[\text{PET}]]\) is the domain on which the PET can be proved.) Increasing plausibility can be interpreted in this representation as a growing number of worlds satisfying both the conditions and the IT. Of course, each relevant PET is satisfied trivially anywhere else in the box. But the economists’ feeling of likelihood of the interesting theorem is generated by the proof that, besides C-worlds there are more hypothetical worlds such as w’ and w'', for which the interesting theorem is true. In other words: PET-C* can be proved to hold in worlds w’ and w'' too. The ‘trick’, then, is to increase plausibility of IT’s by showing that there is an increased number of worlds in which these must be true. In this sense, plausibility also has ontic implications.

So, if Hamminga is right, economists try to weaken conditions so as to raise the plausibility of the theorems they find interesting. I have translated this into the claim that economists aim for a greater strength of a Proposition of Economic Theory. Although intuitive knowledge plays a role in this development strategy, observable or measurable economic phenomena do not.

However, it is clear that the discipline also, and often, tries to measure real phenomena. Without this no sensible policy advice is possible. Empirical work is centred in econometric work. In order to measure economic phenomena, well specified variables must be sought; and the more specific the claims of researchers are, the stronger they are.

Weakening of conditions is precisely the opposite of strengthening the definitions needed to identify the data (already available or to be collected). As Hamminga says, in econometrics ‘minimum requirements for identifiability [of a variable] are far stronger than the special conditions used in the Ohlin-Samuelson programme’.\(^{17}\) For instance, production functions must be specified. But the trade theory of Hamminga’s case says nothing about how to specify ‘identifiable mathematical models’ for structural conditions under which production takes place. There are also anchoring problems for econometricians. That is, it is not clear from the theory how to select variables from data sets and interpret them correctly as endogenous or exogenous. To quote Hamminga’s example, how should we know whether the 1947 US economy approximates a two factor economy near equilibrium? So he asserts:

One of the great misconceptions of theoretical economists on the nature of their own enterprise is that the notion of “Heckscher-Ohlin theory” unambiguously denotes some element in the structure of economic expositions.\(^{18}\)

Thus, Hamminga arrives at his blatantly dramatic ‘mutual independency thesis’, which says that ‘results of econometric research cannot in the least affect the dynamics of the Ohlin-Samuelson programme’.\(^{19}\) And although his conclusions are derived from a case study of international trade, his pretension clearly is that it

\(^{17}\) Hamminga (1983), p.98.

\(^{18}\) Ibidem, p.99.

\(^{19}\) Ibidem, p.100.
stretches beyond that and applies to all theoretical economics. Econometric practice cannot help economics in theory evaluation. So while de Marchi concluded earlier that deriving testable hypotheses from economic theories is difficult, Hamminga believes that econometrics is not tailored to the practices of theoretical economists. It seems to me that de Marchi and Hamminga refer to the very same problem; that is, Lipsey and Archibald, who had wanted to do testing at the LSE, bumped into the problems the origin of which is explained by Hamminga’s research. Thomas Mayer apparently agrees. He contends in his (1993) that (what he calls) formalist theory and empirical science theory ‘invoke widely different criteria in evaluating theories’. Mayer proposed to ‘honour them both’, i.e. to allow each sub-discipline to have a life independently from each other.

However, although evaluation problems related to the divide between the need for theory and the requirement of tailoring an adequate basis for producing evidence to it certainly exist, they are not only due to the different specification orientations of economics and econometrics. Another problem is that the context of justification and the context of discovery are severely entangled. Recent research finds that, as regards economics, the usual assumptions that theories, models, and data are clearly separated and that empirical assessment comes after model building are both false Boumans (1997). Criteria for the quality of mathematical models are a priori. They stipulate requirements that models help find solutions to theoretical problems, explain phenomena, hint to policies, or simply provide a mathematical conception of relevant phenomena without further derived uses. Economic models transform ingredients such as empirical facts and theoretical ideas, metaphors, stylized facts, mathematical concepts, and policy views into a coherent mathematical form. Some properties of convenience are introduced into the model as a special case of mathematical moulding. Furthermore, in order to develop the best model, it has to be calibrated. The parameters must fit the data and all the other ingredients, and make sure that the model is ‘true for all the ingredients’.

But there is no unique manual for the construction of the model, and for each new investigation there seems to be another recipe. Thus, Boumans compares economic model building to baking a cake without recipe. The economist cooks and tastes at the same time, adjusting the cooking process according to his liking: ‘a new recipe is a manual for a successful integration of a new set of ingredients’. The emphasis is to stress that other procedures could have rendered perhaps not an identical but at least an equally satisfactory result. It is a trial and error process.

It is safe to conclude that simple deductive-nomological schemata do little justice to the sort of scientific development patterns we should be able to find in economics. The strategy Böhm-Bawerk followed in order to do better than the Classical Economists boils down – as I have shown – to trying to make explanatory

\[\text{\cite{Mayer}}\]

\[\text{\cite{Boumans}}\]

\[\text{\cite{Ibidem}, p.27. It seems to be more appropriate to speak of ‘valid’ instead of ‘true’ here.}\]

\[\text{\cite{Ibidem}, p.2. See also Boumans (2005). He inserted the italic ‘is’ in order to stress that it concerns one out of a set of several possible manuals.}\]
progress. The proof for the comparative advantage of Austrian subjective value theory pops up occasionally as having been provided by the observation of everyday phenomena, and some other times in the form of higher conceptual and theoretical unification. The same is true for the interest- and distribution theory.

3 Explanatory empirical progress?

I have positioned Böhm-Bawerk’s interest and distribution theory as explanatory, not predictive, but it does not follow that there is no empirics – that is, no basis for producing evidence – to it. He subsumed many different phenomena under a description by one set of hypotheses and with the use of one set of concepts. The key hypotheses are LoMA and the law of diminishing marginal utility (for the value theory), the productivity of roundabout production methods (for the capital theory), and the three causes of surplus value of present over future goods (for the interest theory).

The mechanistic approach offers a minimum level of complexity of the re-description of economic phenomena that enables the economist to explain both how markets tend toward equilibrium and how disequilibria recur. In other words, the description of the mechanism explains stable patterns and instability. Classical ‘political economy’ (but also much of 20th century neoclassical economics) cannot do this. As Hamminga phrases it, the Fundamentals of Economic Analysis of neoclassical economics is ‘a functional framework which maps any data set on an equilibrium set’. This means that markets are supposed to be in equilibrium, rather than to be in perpetual flux. Neoclassical explanations perceive all market phenomena as either ‘already’ or ‘not yet’ equilibrium phenomena. The resulting focus on stability is apposite for explanations that come by lawlike regularities. After all, social (quasi-)laws apply to relatively stable environments, free of external shocks for some reasonable period of time. A body of phenomena must strike the observer as constant to make lawlike parlance intelligible. (This is not to say that the same cannot be claimed, to a certain extent, for the mechanistic approach.) The point now is that the regularity account of economic phenomena forces the theoretician to insert ceteris paribus clauses. Tony Lawson is vigorously fighting the dominant constant-conjunction view of economics, as I understand it, because it does no justice to the necessary ontology for an explanation of socio-economic reality; it lacks metaphysical content. I am not sure whether this

25 Note that also explanations in terms of mechanisms must assume something to remain relatively stable, otherwise there is no structural relationship between the composite parts of the mechanism. Stathis Psillos has eloquently shown this as he noted that an analysis of causation in terms of counterfactuals enters analyses of mechanisms. The mechanistic approach says that two events relate causally if there is a mechanism that connects them. This makes sense only if there is some constant conjunction at stake, and if we can think of what the mechanism would do if initial conditions were different. See Psillos (2004). See also chapter V as I quote James Woodward on a related issue.
3. Are true claims about unobservables possible beyond mere reference of terms?

4. Is there one best conceptualisation, or vocabulary, of W?

An epistemological relativist would say ‘no’ to question 1. An epistemological realist says ‘yes’ and has the choice to be agnostic or not about the reference of nonobservational terms. If not, he can still admit or deny. To say ‘no’ to question 2 is to be a particular type of realist (i.e. an observational realist; a constructive empiricist would be agnostic). Clearly, constructive empiricists and observational realists have to form an opinion as to the status of the objects made visible by telescopes, electromicroscopes and many other instruments whose working relies on the truth of well developed observation theories. But stronger positions are available if you answer question 3 positively too and thereby choose to be a theory realist.

Arrived at this position, if you believe that, in principle, an infinite set of vocabularies might adequately help to describe the object of research – the social or the natural – you answer question 4 negatively. It is the answer of a constructive realist and this is the position Kuipers defends. But an essentialist prefers one conceptualisation over the other and goes further than this by a quest for the best set of concepts. The previous two chapters have claimed that Böhm-Bawerk believed that this is a worthwhile operation and that he succeeded in effecting it. In addition, chapter II assessed Wicksell’s judgement as rather benign to this extremely strong claim. In WKR, Wicksell praised Böhm-Bawerk for his concept of time as a productive force and sharply contrasted it with competing conceptions, such as the one developed by Jevons.

By the term ‘essentialism’ I refer to the belief that question 4 must be answered positively: there is one best way to conceptualise the world in order to research it. This fits the view that essentialist explanations refer to kinds, as these are supposed to be fixed in reality. One may fight over the question whether a conical form that appears in reality is best described as a triangle (seen sideways) or as a circle (seen from below), but a better way to describe it would be as a cone (depending on one’s description objectives, that is). Perhaps the best way is still different, in terms of more than three dimensions.

If Wicksell is right in his judgement that ‘time-as-productive-factor’ is the best explanatory concept, progress has been made possible by successfully abstracting particular aspects of social phenomena from manifold appearances. These aspects may comprise any combination of objects, properties, and relations. In any case, they are aspects that identify social mechanisms shaping market outcomes: market mechanisms. These are rooted in the stable social structure of modern entrepreneurial capitalism. The social structures, in turn, are the ‘social kinds’ Böhm-Bawerk tried to dig up with his conceptual apparatus.

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28 In more recent work Kuipers also proposes a refinement by which it is possible to distinguish entity realism (terms for objects in reality refer) and structural realism (although such terms do not refer, theoretical laws have truth value). Theory realism defends the combination of these two.
opposition is required. Stories quoting regular patterns in economic reality might as well make explanatory sense even if that reality is in flux. Moreover, such patterns may actually exist in a changing world, at least for a (policy) relevant period of time. The point is not whether clauses theoretically exclude changes in initial conditions, but whether the theorems in which these clauses figure are false or not. The following chapter tries to prove that the use of such clauses need not involve falsity in any fundamental sense.

This also sets idealization apart from abstraction as strategies of analysis. Glanced over superficially, the product of either epistemic procedure sometimes resembles the other, but they can be conceived of as each other’s inverse. In addition, this conclusion has consequences for the policy relevance of economics.

4 Does abstraction involve essentialism?

Explanatory progress, it seems, comes with the abstraction of concepts that help identify particular patterns or constitutive elements from a set of economic phenomena. The story which tells how these elements cohere and what the patterns are like will be better as more phenomena can be subsumed under one single rede-scription. Explanatory progress and explanatory unification come in a brace.

But abstraction involves priors. What to abstract and what to ignore is to be decided intuitively, as with Hamminga’s SEPC, or on the basis of more explicit methodological rules that are prior, at least analytically if not in time, to theorising. Hans Radder has an interesting account of how concepts both abstract from observed reality and structure the observations. I shall discuss his work in chapter V, which investigates to what extent the essentialist inclinations, which apparently buttress conceptual developments in economics, can be philosophically upheld.

Essentialism can for now loosely be characterised as a meta-judgement saying that scientists refer to natural kinds in order to explain. In giving the notion somewhat more precision I follow Kuipers’ hierarchy of epistemological positions. He lists five questions, the answer to which helps identify the particular epistemology that some philosopher adheres to. The sequence of questions and answers resembles a forensic inquiry: do you confess or deny? It is a hierarchy, for any question presupposes that a previous question has been answered.

The first is a basic question in the sense that it ontologically grounds the subsequent epistemological positions. To say no to this question is to be an ontological idealist, or worse, a solipsist and it would leave very little room for a choice between epistemologies. That is why it is counted (by Kuipers) as question zero.

0. Is there an independent natural (social) world W?
1. Are true claims about W possible?
2. Are true claims about W possible beyond the observable?

The question is: are present day economists also inclined to conceive of theoretical economics as ‘finding the right vocabulary’? I think so. Let me devote some reflection to realism – even essentialism – in economics, and to some more indications that the economic science’s tacit self-conception is often essentialist.

5 Is economics involved with essences?

In 1976, Robert Lucas wrote an influential paper, which is now referred to as the ‘Lucas Critique’.29 Already in 1971 he had suggested that, if we take the meaning of the word ‘rational’ seriously, rational people use all the available knowledge, including economic models. The 1976 critique holds that the outcome of policy changes cannot be predicted without knowledge of the parameters that describe individual behaviour. In the Keynesian tradition, macroeconomic models would render policy advice assuming that markets (the agents who populate markets) would be kept in a network of invariant relationships if authorities such as central bankers changed policy. The exogenous variables of the model were supposed to remain unaltered. Lucas understood that the ‘deep structure’ of economic relationships between agents and authority would be affected by policy changes, for instance because expectations would be affected. If this is correct, the conclusion must be that macroeconomic policy requires microeconomic analysis even if the relation between macro and micro is fuzzy. Macroeconomists must again focus their curiosity on deep parameters concerning individual behaviour.

The Lucas Critique assumes that individuals entering (financial) markets form expectations on the basis of the models that policy making authorities build. In his Conversations with economists, Arjo Klamer reports an interview with Lucas. He asks how we (or rather: his students) have to believe that agents can be expected to form expectations on the basis of economic models that are beyond their comprehension. Lucas responds as follows.30

“I try to turn it around. People in business usually like to get into conversations about what they do all day and how they make their decisions. I’m always impressed with how sophisticated their thinking and information-processing is. What puzzles me is the number of economists who seem to believe the reverse. It would be a miracle if I could write down a model for the demand for shoes and the supply of shoes, cook up a little difference equation, solve it, and the solution would reveal profits available to me from the shoe business that weren’t obvious to people working in the shoe business for 20, 30, 40 years. It seems ludicrous that we could discover sizable rents with our simple equations without knowing anything about shoes. But some economists think we can get an insight into someone else’s business without knowing anything of the substance of his business.”

One page further down, Lucas says:

29 Lucas (1976).
“There is nothing descriptive in demand theory in terms of the process by which human people, families, and whole business firms make decisions. Economists have lived with that for years.”

It is laid out here that economists may be making models, but real people who make the decisions know their thing. Economic actors are instrumentally and cognitively rational. The theory makers have to model this situation for their own purposes, but this modelling is not a reproduction of reality in any descriptive sense.

This is reminiscent of PTK. Böhm-Bawerk also notes how ordinary people know what to do in everyday decision making without the sort of human deliberations that Austrian economic theory is modelling. Normal decision making is the result of experience and routine. The models of Menger, Böhm-Bawerk and Mises aim to reveal a formal structure of the decision making process without pretending to actually literally describe what it feels like, as it were, to decide as a consumer, as a demander or supplier on business-to-business markets or on the labour market. It is a ‘re-description’ of the story a real decision taker would tell you, even if this agent would not redescribe it in the same way. The quote shows that Lucas has similar reservations as these Austrians: he sees economic theory as a re-description of the layman’s description, which deviates from a description that is truthful about the concrete details of the world of economic agents. Moreover, he generalises this view so as to ascribe it to the broad ‘economists’ in the last quote.

It is possible to explain this view on the discipline with the help of the famous ‘instrumentalist’ account of economics known from Milton Friedman. His essay ‘The Methodology of Positive Economics’ has had much impact on economists around the world, not least at the LSE. It puts forward the view that neither economists nor their lay public need bother about the ‘realism’ (as he calls it) of the assumptions of economic theories. The way in which theories withstand tests is supposed to be the only criterion on the basis of which to judge their usefulness. This critique has become known as the ‘F-twist’. Social scientists see the conformity of the assumptions of a theory to reality as an additional requirement to the test of the predictions. Dead wrong, says Friedman. He stresses that:

\[\text{[t]ruly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions.} \]

\[\text{[…] The reason is simple: A hypothesis is important if it explains much by little, that is, if it abstracts the common and the crucial elements from the mass of complex and detailed circumstances}].\]

Friedman does not only claim that the realism of the assumptions of a theory is not needed, but that it is not even desired. Scientifically interesting truth does not come about by maximizing the accuracy of all the descriptions. The ‘crucial elements’ (from the mass of complex circumstances) are doing the job. The postula-

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31 Uskali Mäki shows how Austrian explanation can be seen as ‘redescription’. See appendix 3.
tion of such a scientifically interesting truth has metaphysical bearing. Indeed, the essay sometimes even suggests a strong type of realism:

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.

I draw the conclusion that Lucas has the same normative stance towards economics as Milton Friedman, although he does not mention him. Like Böhm-Bawerk, Lucas wants sufficient complexity, but he rejects descriptions and calls for a redescription in relatively simple terms, at least compared to the actual complexity of economic life. The question is if his critique is an attempt to get to the best theory.

Klamer asks whether Lucas is after truth. He responds:

Yeah. But I don’t know what we mean by truth in our business. I don’t see economics as pushing that deeply in some respects. We’re programming robot imitations of people, and there are real limits on what you can get out of that.

The answer is mixed. ‘Yes, I want true theories, but no, I don’t know whether I can get that deep’. One can’t help feeling that yes, he looks for deep structures, but no, for he is not as optimistic as Böhm-Bawerk with respect to the idea that economics can ultimately succeed in doing that. He does not pretend to be infallible.

John O’Neill blemishes an inclination to confuse essentialism as a metaposition with false claims done by essentialist authors. Especially postmodernists, he says, who ‘celebrate difference and diversity’ sometimes say that a belief in the explanatory value of the ‘essential nature’ of things is to deny difference. True, it is easy to fall into the trap of pinning down objects or worse, people, in the search of their essence. This is how women have been held fixed in their roles by reference to gender, or inhabitants of colonies by reference to race; it is how essentialist expositions can help exploitation. But one need not be insensitive to possible mistakes in explanations by reference to kinds. Essentialism and fallibilism are compatible.

It is relevant to this thesis that O’Neill also observes the following about essentialism and social science:

Contractual and personal relations are essentially different in virtue of the meanings constitutive of them. In this respect at least, the essential natures of social objects are different from those of natural objects. However, the essentialist need have no difficulty in recognizing that the objects of the human sciences do have such distinct properties.

33 Dan Hammond observed the realist, rather than instrumentalist, orientation of Friedman’s essay as he considered the other essays from the book. See Hammond (1990).
34 Friedman, Milton (1953), p.33. Emphasis mine. Mäki has noted the apparent essentialist, rather than instrumentalist, import of this paragraph, especially due to the word fundamental. Eric Schliesser reads Friedman’s essay as Kantian rather than essentialist. See Schliesser (2006).
37 Ibidem, p.171.
Those opposed against essentialist views of science often deny that true theories are an attainable goal in science. So many a theory seemed to give the answer to lots of what scientists wanted to know, only to be replaced by another, incompatible theory later. Newtonian mechanics was a paradigm after the once robust Aristotelian world view, but it was replaced by Einstein’s General Theory of Relativity. It is important to remark that essentialism merely holds that scientists have to try and find the best vocabulary in order to formulate the strongest true theory, not that they have already found it. So I say again: essentialism and fallibility are not contradictory.

The a priori plausibility convictions that Hamminga tracked in the work of international trade economists fit into a view of economics as essentialist. The world is believed to behave in a certain way and no other. Economists try to carve up that world in such a way that this particular behaviour is made intelligible. There is a truth to be discovered, if the carving is just right. One can encounter this preconceived notion of science among economists all over the place. Insofar as they propose new concepts and other explanatory building blocks that help erect alternative models, they insist that the new conception picks out something we lacked so far in order to construct satisfactory explanations. This is a natural attitude not reserved to Austrians. Take current experimental research aimed at feeding the theory of incomplete contracts. There is a paper by Fehr, Gächter, and Kirchsteiner, reporting an experiment by which subjects play the roles of agents in the labour market. After deciding on a price for the service and a quality level supplied, the subjects enter a second phase in which the labourer decides what level of quality to actually deliver. A high quality means a negative payoff, a low quality means fines and exclusion from trade. ‘Reciprocity’ is the crucial (essential) concept at stake in this research. The neglect of the presence of motives determined by the existence of reciprocity in human interactions is taken to seriously miss the point of the working of markets. Thus, Fehr et al. argue that:

the neglect of reciprocity motives may lead to wrong predictions [...] reciprocal behavior may cause an increase in the set of enforceable contracts and may thus allow the achievement of nonnegligible efficiency gains.\(^38\)

Modern principal-agent theory has so far not been concerned with the impact of reciprocity on contract terms and their enforcement. Our results indicate, however, that the neglect of reciprocity may render principal agent models seriously incomplete.\(^39\)

We can see that some of the building blocks of an explanatory theory are seen as essential. Perhaps the use of the word ‘seriously’ expresses this most clearly. All models, by their very nature, are incomplete. But Fehr et al. refer to something more severe than just incompleteness of the description of an aspect of the world. They find the missing links ‘serious’, they accuse competing economists of having done a bad job. Fehr et al. want to explain by a better conceptual apparatus. Is it the best conception? It is not likely that they would be so daring as to claim

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\(^{39}\) Ibidem, p.856. Emphasis mine.
this. But I guess that they would probably hold that economic research, in the end, is walking en route for the use of ever better conceptions.

I think it is only natural that any scientific work, including social scientific work, is upheld by a tacit ‘feeling of essentialism’, for the following reason. Daily scientific scrutiny into the nature of the material and socio-economic world does not invite a scientist to linger much over the fact that one’s prior conceptual schemes are contingent and that other people – the lay public, scientists from other cultures, or of the other sex – may conceptualise the world differently. The natural attitude of scientists will be to assume that their conceptual scheme is the most obvious and the most convenient one, if not the only one that leads to true theories. Only when we choose to consider, at the metalevel, as it were, the relativity of schemes the appreciation for a pluralist world view is born. This is not to say that scientists, by default, can never have such an appreciation. But it comes in very handy to fail to engage ourselves with pluralism so long as one conceptual scheme helps understand the meaning of our categorisations. I have little reason to engage in pluralist metaconsiderations while observing the world in ways that are suited to my interests, even if I can appreciate the possibility to do so; yes, even if I can appreciate the fruitfulness of doing so in other contexts.

As philosophers we can look down on this attitude. But I think it is weird to dismiss forms of essentialism purely on analytical grounds and just ignore the ubiquity of it in and out of science. It seems more plausible to accept its descriptive adequacy and see to what extent it can be made sound analytically. This is what I to do in the last chapter.