Conclusion

The first question I asked in the introduction was what it is that makes the Austrians so different from economists for whom equilibrium is a key notion as well. The answer is that Austrian analysis, so vigorously defended by Böhm-Bawerk, opens the black box hidden under the Invisible Hand. I suppose that Adam Smith had no explanatory pretensions when he used this mystical Hand but merely used it as a manner of speech. But an Austrian elucidation of mechanisms talks of intentional individual behaviour, which translates into unforeseen market outcomes. The market structure gives rise to a mechanism and this mechanism, given initial conditions, to market processes. Both this mechanism and the socio-economic market structure it depends on lie hidden and Austrian analysis digs deep to uncover these: Smith’s Hand made visible.

The second question was how the ubiquitous use of idealizations in economics can be squared with the quest for truth. I did not seek the answer in a description of the discipline as a non-truth seeking enterprise. I defended my conviction that many economists, regardless their occasional messages to the contrary, do seek truth in theorising. In addition, and stronger, I suggested that they often tend to be tacit essentialists. The answer to the second question is that falsity is the disguise in which truth enters. The falsity an idealizational clause is involved with is inserted in the antecedent of a possibly true counterfactual conditional and it is this falsity, which makes the entire subjunctive proposition a counterfactual. Often economists do however not idealise but abstract; they do not hedge their theories by well formed clauses but they just ignore some causally relevant aspects of the world. In this case there is no clear indication as to the precise circumstances or causes that are counterfactually being made provisos about. Only in retrospect – after more of the subject is understood – a past abstractive theoretical endeavour can sometimes be reconstructed as the use of a particular clause.

The accusation of theorising being ‘too abstract’, habitually uttered by policy makers, is often premature. Judgements of this sort must be placed in the context of the question whether it is ante explicationem or post explicationem abstraction that one wants to attack. Economists, in their turn, must take care not to disregard, by abstraction, the crucial aspects from the explanandum without paying attention to what other social disciplines have to offer. I return to this plea in the last paragraph of this conclusion.

The third question asked what part of the social world it is that economists focus on when they abstract it from all this spatio-temporal detail in which it is embedded. The answer seems trivial: whatever they believe is important for explanatory purposes. It is however not trivial that economists describe an explanandum such that they can gather the ingredients for abstract explanatory social kinds. In the act of abstraction and concept formation, they use pre-theoretic notions to structure the explanandum but, once they have strengthened their ontological commitments to more robust world views, they tend to behave like essentialists.
The fourth question was how the idea of abstraction as ‘summarising’ copes with the analytical problems essentialism tends to run into. Other philosophers have shown that there can be a degree of essentialism sufficiently weak to buttress a social scientific epistemology in accordance with natural kinds as explanatory tools. That is why I speak of ‘social kinds’ and I suggest that Austrian analysis finds these in those structural aspects of economic reality that give rise to mechanisms. Böhm-Bawerk explicated Menger’s market mechanism in terms of the Law of the Marginal Agents. The modularity that one expects to find in a genuine mechanism is a property of the mechanism-cum-structure. The mechanistic part is pronounced by its hypothetical character as expressed by the idea of Jim Woodward’s ‘invariance under interventions’.

I also say that a ‘perspectivist essentialist’ epistemology is thus conceivable. It is now expedient to refer to Kuipers’ scheme of the four epistemological questions, as summed up in the intermezzo. Recall from my intermezzo that one can ask, firstly, whether scientific propositions have truth value and, secondly, whether, if so, truth can also be a property of propositions with respect to unobservables. The third question one can ask is whether, apart from mere entities, entire structures as posited in true theories really exist. Is it fruitful to engage in claims to knowledge about the (natural, social) world even beyond the mere reference of theoretical terms? If so, there is reason to adhere to realism of an importantly stronger sort than referential (or entity) realism.1 In Kuipers’ scheme, two more degrees of freedom exist if one endorses this, given the fact that realists (and both structural and referential realists) are faced with a fourth question: is there one best conceptualization of theories? Kuipers’ own preferred option is to answer this question negatively. This is what he calls constructive realism. This is the view that although one type of vocabulary can be more useful than another, there need not be one unique vocabulary that carves the world at its joints better than any other. But essentialists precisely claim that there is one best conceptualization.

I propose a further refinement. Perspectivist essentialism can be incorporated in Kuipers’ scheme as providing yet an extra option. Perspectivist essentialism is different from constructive realism in the belief that scientists must look for kinds because of their explanatory value and in that these kinds dictate the conceptualisation. It is however also different from full blown essentialism in that it accepts that there are minimal and robust versions of essentialism, such that any essentialist inclination in science is a matter of rigid designation merely de jure. This view allows that one particular perspective on the world loads the meaning of concepts with one set of ontological presuppositions, while another perspective loads it with another.

One may object that perspectivist essentialism has no teeth. Its being different both from constructive realism and old fashioned essentialism shows that it has. There are reasons of descriptive adequacy for inserting this refinement. Phi-

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1 I pass over the complicating possibility of structural realism here, i.e. the view that terms do not refer but that laws nevertheless have truth value.
losophy of science is more than ornithology (‘scientists lay their eggs anyway’) but its normative bite is toothless if it bears little relation to what scientists (economists) in fact are doing. My emphasis in philosophy of science is therefore that a quest for normative adequacy must start with descriptive adequacy. Since I believe scientists to act as if they are essentialists of some sort, the question in chapter five was how to normatively defend this. The concept of perspectivist essentialism does the job.

Rests the scope for further research.

The need for a scrutiny of what further kinds scientists propose in their explanations stands out. I have here provided little more than a hunch of what a social kind is in Böhm-Bawerk’s theorising. Other examples have to be looked for in the work of any economists who complain that some essential explanatory aspect of the world has not been taken into account by existing analyses. In appendix 3, I discuss Uskali Mäki’s favourite cases of George Richardson, Ronald Coase, and James Buchanan. All three of these stress the importance of ‘a missing link’ in economic theorising, all three engage in the intuition that particular explananda must be endogenised. (I refer to this appendix for the respective links that these economists tell us are missing.) Quite another proposal (albeit without the notion of perspectivism) I know of is to give emotions a natural kind status in psychology.2 We have seen that the possibilities are infinite but that we have to guard the explanatory value of the whole enterprise. I have not discussed what counts as explanatory and what not, so there is work to be done here too (not least about the question what it is that makes an explanans explanatory).

The notion of abstraction as defended in this thesis can benefit from a further refinement. In my treatment abstraction comes with generalization, as it leads to logical weakness and an increased chance of truth; so concretization, conversely, comes with specification. But some very abstract and explanatory propositions of science – like Newton’s laws of mechanics – are in fact strong. Or at least, the meta-statement that a very precise pronouncement is true in many cases is stronger than that it is instead true in only a few cases. So why are the best theories the most abstract and the strongest at the same time? I am convinced that this has to do with their unifying power. The role of abstraction, then, in theoretical unification specifically has to be investigated. To allow myself a hint to reflexivity, the concept of ‘abstraction’ can be concretised further.

This brings me to an issue that I most dearly wish to examine. Contrary to Henk Folmer’s recommendation3, there is still little communication between the social sciences. But worse, there is little communication even between the respective research programmes both within psychology and within economics. As to psychology, this seems to be due to the lack of conceptual unification that one notices when talking to, for example, neuroscientists, cognitive psychologists, and psychoanalysts. In the case of economics, in contrast, there is little communication for

3 See Folmer (2000).
example between institutional economics, the neoclassical research programme, game theory, and experimental economics. This may be the consequence of a strong orthodoxy in the science, one making use of a well unified conceptual apparatus. If this is true, then both a low level and a high level of conceptual unification causes separate paradigms between which there is little interaction. If it is indeed true that both conceptual unity and disunity render excommunication within the two disciplines, the thought of an approach directed between these extremes is exciting. For instance, I would like to find out whether it makes sense to inspect the conditions for an optimum level of unification in each of the two sciences. This is especially interesting given the recent developments in experimental economics, for the experiments with real people are psychological in kind but they bear on our ideas about individual economic behaviour.