Philosophy of science is traditionally a normative enterprise. But without historical adequacy no metatheory is worth its salt. For instance, one cannot but notice that economics has been developing without any predictive success for a century and a half. The sort of progress it has been making is explanatory. An instrumentalist account of the development of economics must be inadequate, for the following reason. A description of the dynamics of a discipline in terms of explanatory progress implies a commitment to ontologies; instrumentalism is the rejection of such a commitment. It may well be possible to picture the predictive success of a theory while being agnostic about what the terms of this theory refer to. Black boxes can be compared in their predictive record. But to say that a scientific theory explains better (than yesterday; than the competing theory) is to indicate one’s (deeper) understanding of the explanandum. Black boxes do not give an understanding; instead they conceal the very nuts and bolts that are to provide such an insight. Hence, seen from the perspective of the meta-enterprise that philosophy of science is, it seems to me that in trying to give historical adequacy one is forced to be a realist about theories.

Besides, from the object perspective of the scientist, it is impossible to investigate the world in order to gain explanatory insight in it without prior ontological convictions. Pre-scientific beliefs are supposed when research is to generate scientific knowledge. Probably, most pre-scientific beliefs are implicit and even unconscious. For example, Bert Hamminga found that economists who work in International Trade Theory, as he dissected their reasoning, actively deploy ‘Systems of Elementary Plausibility Convictions’ in order to assess the acceptability of interesting theorems.

Ontological commitments are by no means limited to International Trade Theory, my intuition is that all science presupposes such commitments. Pre-scientific beliefs about what the world is like and how it works imply ontological realism, but also epistemological realism, for their being beliefs. Böhm-Bawerk’s endeavour, as analysed in chapters I and II, is an example of this realism. He provides explanatory unification, defining concepts anew and applying Mengerian Value Theory to the theory of interest. Of the possible epistemological positions running from inductive scepticism, via instrumentalism, and entity realism to constructivist and essentialist realism, Böhm-Bawerk seems to adhere to the last position. As he is convinced that the ‘Austrian toolkit’ fits economic reality, as it is, best, his intuition can be qualified as essentialist. Indeed, his own unifying conceptual apparatus appears to be more explanatory powerful to him than its competing
conceptualizations. In any case, Böhm-Bawerk would be ready to claim that, without it, many essential nuts and bolts of the market mechanism remain out of sight.

I present his intuition, that economic mechanisms make up the correct way of explaining in economics, as a search for social kinds. However, two caveats pertain. First, Böhm-Bawerk does not hesitate to dish up the customs of the physicist, who ‘greift die charakteristischen Typen in solcher Zahl und Auswahl heraus, wie die Natur seiner allgemeinen oder besonderen Erklärungsaufgabe es ihm zweckmäßig erscheinen läßt.’\(^1\) In other words, the research aims codetermine, together with the ‘way the world works’\(^2\), the choice for what is taken to be the essential cut. Second, essentialism is a philosophically difficult stance to defend. Essentialism seems to claim that good science fixes the explanandum, and disallows both vagueness of terms and fundamental changes in the world. Emancipatory action or discussion about meanings appear to be impossible. Hence, there seems to be a tension between historical and normative adequacy: although an essentialist attitude concerning the world and the knowledge about it merely is natural, it is philosophically difficult to be upheld.

Two questions arise. Which objects (or properties, or relations) in the economic environment are the credible candidates for social kinds? And how can such an essentialist interpretation of economics be defended? As to the first question, in Austrian economics these kinds are structures that determine the degrees of freedom of economic market mechanisms. These structures include market forms with intentional agents, endowed with preference orderings. The market mechanism, rooted in this socio-economic structure, is represented by what I label the Law of Marginal Agents. This is the supposed regularity describing how the subjective marginal utilities of the market parties, who operate ‘in the margin’, determine the objective market price.

As to the second question, the philosophical tenability, essentialist interpretations of knowledge acquisition rest on the specification of rigid designators. The Causal Theory of Meaning holds that instances of a kind help specify a kind term by a sort of causal baptism. Joseph LaPorte explains that it would be too much to ask from such a theory of language to weed out all vagueness in our speech and all change in the world. What such a theory may be asked to do is help recognise vagueness of speech and change in the world. The labelling of a kind term is no more than a proposal to carve the (economic) world up in the supposed unique best way. The rigidity involved in the specification of a kind term is a mere rigidity de jure, not de facto.

\(^1\) PTK, p.262.
\(^2\) Mäki’s phrasing, see his (2001).
The last chapter of this dissertation heavily depends on LaPorte’s new view on language and philosophy of science. One attractive aspect of it is that the idea of unique right conceptualizations does not preclude fallibilism. We may be wrong but whatever we think is the correct theory to explain the world and its workings is also a theory that usefully retrieves abstract kinds. Just like “water=H₂O” was the discovery of a necessary truth – and although the extension of a kind term differs across possible worlds – in every possible world where we find the kinds as we happen to have specified them, the kind terms refer to the same abstract kind. In this rather technical sense science is involved with (natural) necessity.

Although there is a certain deliberate weakness to this way of explicating essentalist explanation, kind terms do help explain. Just as the peculiar behaviour of a polar bear (LaPorte’s example) is explained by denoting the kind ‘polar bear’, Böhm-Bawerk’s case shows that many market phenomena can be explained by constructing the kind ‘market’. In the Austrian view, a ‘market’ gives rise to causal mechanisms and the dissection of a fundamental structure, in which this mechanism is supposed to be rooted, has explanatory power indeed.

Another question remains. What sort of epistemic operation characterises the specification of kinds? Chapter III answers this question, chapter IV gives some examples. Science proceeds by building theories. Theoretical knowledge is knowledge of abstract kinds. So, apparently, abstraction is the key operation in the construction of theories. A confusion as to what sort of operation this is seems to be ubiquitous. In consequence of the influence the Poznań School has had on some of them, philosophers often speak of ‘Idealization and Concretization’ as a term for the subsequent introduction and removal of limit values in theorising. It is also common to hear economists say that ‘the ceteris paribus clause helps to give an abstract representation’ of the economic realm. But hedging a general hypothesis against expected or unexpected interferences is not the same as abstraction. This becomes clear, for instance, when a clause gets in the way of a useful description of the actual state of affairs. In order to remove a clause, one has to be aware of what it is that is being specified by it. This means that the details summed up in the clause are actually being referred to. In case of abstraction, these details are unknown – hence they cannot be referred to – which is precisely the reason to abstract from them.

The difference is important, because abstraction does not introduce falsity, while idealization, in a sense, does.Clauses that require a ceteris paribus, a ceteris absentibus, or a ceteris neglectis are in fact deliberately false claims. They specify a state of affairs – that friction is absent, that transaction costs are close to nil – which clearly is not a property of the actual world. Such a non-actual state of affairs is, however, a property of a conceptually possible world. The question now arises how scientists (and, above all, economists) can think of themselves as seeking true
theories while inserting deliberate falsity in their claims. Self-proclaimed ‘instrumentalism’ does not immunize economics against this question. The answer is given in chapter III. Here I introduce the idea that such a clause is part of the antecedent of a counterfactual proposition, i.e. a proposition about conceptually possible but not actual worlds. According to a Kripke-Lewis semantics, the counterfactual can be true even if the clause is false of the actual state of affairs, that is, if the clause does not count the actual world among its models. This is important, because idealizational strategies in social scientific explanation are inevitable. The semantics developed shows that the falsity of the clauses, which hedge the explanations of real phenomena, is no threat to the truth of theories. Idealizational conditionals can themselves be true, *qua counterfactuals*. Note that the falsity of their clauses can nevertheless be an obstacle for policy intervention, when the false assumptions involved have to but cannot be dropped.

Abstraction, in contrast, is capable of leading to truth in a more straightforward way. I present abstraction as an operation by which an unknown number of spatio-temporal details are omitted from consideration, possibly unconsciously, so as to focus on some aspects only. These isolated aspects are rendered interesting. The logical form of it is existential generalization. Thus, abstract and concrete are matters of degree. Starting from a more concrete but true proposition, the existential generalization leads to a more abstract but still true proposition. But note the greater extension of abstracted claims relative to their more concrete counterparts. Due to this, starting from the more concrete but false proposition, the more abstract proposition may absorb the actual world among its models.

One problem, however, is that abstract claims can be too weak to be interesting. This is a feature that follows from such claims being generalizations. Stronger claims are those that exclude more possibilities, while abstraction, conversely, leads to the inclusion of more possibilities. The fruitfulness of an abstract proposition depends on our background knowledge and our interventionist interests. I hold, in chapter III, that it is obvious that there are abstract but interesting claims also in economics. One candidate is: ‘the peoples who engage in trade instead of in autarky will enjoy a greater welfare than those who do not.’ Another perhaps is: ‘there is a natural rate of unemployment, which is insensitive to budget policy’. I believe that these claims are true; they are in any case generally subscribed to by the core of economists and clearly relevant. Admittedly, there is an ideological component to this belief, which is excluded in cases of physical or chemical engineering. So, it is a political matter precisely how interesting these claims are. But it is a matter of scientific research whether they are true.

Thus, the verdict that economic theories are abstract *in itself* cannot sustain claims to policy irrelevance. At most, the verdict of theories being idealizational can.