

Dealing adequately with the political element in formal modelling

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Abstract

This chapter shows that the idea of positive economics is an inadequate conception of science that has been abandoned in much of the social science until the beginning of the 20st century, but experienced a revival in economics during the last decades. We will study the arguments underlying this development and show the relevance of acknowledging the normative element in economic modeling. We will develop an epistemological framework that illustrates the way subjective, and thus potentially normative, ideals of the researcher enter the formal models and that their mathematical nature does not make them value-free. Using the economic reports on the effect of the *Transatlantic Trade and Investment Partnership* (TTIP) we clarify that and how the quality, reliability, and societal usefulness of economic theory can be increased if economists understand the inevitability of economic modeling and draw the adequate conclusions. **(140 words)**

1. INTRODUCTION

Paul Samuelson once said that he doesn't care who writes a nation's laws, if only he could write its textbooks. This illustrates very well the importance of economics in the public discourse and its political relevance. In contrast to classical economists such as Adam Smith, most economists today usually claim that their science is not normative *per se*, but that there exists a clear distinction between positive and normative economics.

This has an important effect on the role played by economists and their models in the public discourse. Economists are often perceived (and present themselves) as scientists using models that help to understand the world as it is and which do not contain any implicit normative elements. And if economists make explicitly normative statements, these are usually said to rest on sound positive theoretical foundations. For example, if one considers the actual discussions about the *Transatlantic Trade and Investment Partnership* (TTIP), economists influenced both the public debate and the political decision makers with their reports. The way these reports are written suggests that they apply objective measures to assess the economic impact of the free trade agreement and that their (normative) suggestion to implement TTIP is based on sound, positive, state-of-the-art measures which do not carry any normative content on their own. But is this view adequate?

This chapter shows that the idea of positive economics is an idealistic, but wrong conception of science that has been abandoned by much of the social science at the latest at the beginning of the 20st century, but experienced a revival in economics during the last decades. We will study the arguments underlying this development and show the relevance of acknowledging the normative element in economic modeling. By this we will make clear that the quality, reliability, and societal usefulness of economic theory can be increased if economists understand the

inevitability of a normative element in economic modeling and draw the adequate conclusions.

The rest of this chapter is structured as follows: section 2 presents theoretical arguments on whether a value-free science of economics can exist and how methodological innovations in the 21st century had an influence on this question. Section 3 introduces a framework that shows how formal models can get epistemic meaningfulness, what the different components of a model are and how this relates to the question of positive economic modeling. Section 4 asks whether more data-oriented research practice can lead to more positive economics. Section 5 applies the theoretical arguments to the current debate about TTIP. Section 6 summarizes the essay and provides conclusions.

2. THE INEVITABILITY OF A NORMATIVE ELEMENT IN ECONOMIC THEORY

To what extent can science in general and social science in particular be *value-free* or *positive*? In economics one usually uses the two terms nearly interchangeable: whenever a theory describes ‘what is’, it is called *positive*. Whenever the theory describes ‘what should be’, it is called normative. This dichotomic taxonomy clearly suggests that ‘positive’ economics has nothing to do with ‘normative’ judgements, i.e. is value-free, or objective. But how this distinction is to be made in practice is not always clear: is the basic model of perfect competition part of positive or normative economics? It is not particularly concerned with the question of ‘what should be done’, but its (empirically dubious) assumptions imply the conclusions that any taxes yield a particular ‘deadweight-loss’. Even if the scepticism against government intervention and the principal optimism towards the market mechanism is not articulated explicitly, it is hard to believe that such a model

does not carry normative elements. But here I will not deal with the particular case of the theory of perfect competition – I will rather consider economic theory in general.

But before, recall that the fact of whether a theory can be considered positive (in the sense of *not-normative*) has important *political* relevance: value-free results can be expected to play a particular strong role in public and political discourse. Normative results are always scrutinized more thoroughly because people explicitly question the normative axioms of the results – a potential source of refusal (both in political and scientific debate). In this sense, the question of whether social science can produce *positive* results has immediate consequences for its place in the public political discourse and the way it shapes this discourse. Later, I will consider the economic assessments of TTIP – and all these studies contain elements that are allegedly positive, but which in fact result from very subjective choices and have enormous political relevance.

From the point of view of scientific progress and effective debate, to have value-free, or *positive*, theories would be desirable: beyond the increased power in the public discourse, such theories would be uncontroversial and could represent a solid scientific foundation on which normative debates could then be contended. Having a set of positive results would facilitate the discourse among scientists since the common positive beliefs could serve as an effective common language enabling constructive normative debate, even if the scientists had different normative beliefs.

Because of this attractiveness of positive theory, its possible existence had been extensively debated in the social sciences.¹ In particular social economists and

institutionalists made important contributions to this subject matter. These include, but are not limited to, the important pragmatist elaborations of Pierce and Dewey who laid the philosophical groundwork for Veblenian institutionalism, a research program that has always insisted that economics is inevitably also a normative science. Gunnar Myrdal explains in an entire (and very influential book) how and why implicit value judgement enter economics through the interdependence of production and distribution, and through its very basic utility theory. And Hans Albert, and later Jakob Kapeller, elaborated on how economists use *axiomatic variation* to save the results of their models from falsification, illustrating how malleable and open to subjective manipulation the economic formalism can be.

So, although this compilation represents only a small subset of the critical literature on the subject matter, it illustrates that for most scholars that are directly concerned with the philosophy of sciences, the debate on whether positive economics can exist is settled since some time ago – a fact that is, unfortunately, not known to many economists not working in the areas of the scholars mentioned above. This is noteworthy because an early and decisive part of this debate was conducted by members of the German Economic Association (*Verein für Socialpolitik*) in the so called *Werturteilsstreit* in the beginning of the 20th century.

In this chapter I will focus on the main results of this early debate and not on the many explicit contributions made by social theorists mentioned above. The reason for this is that it is not particularly surprising that institutionalist scholars disagree with more ‘mainstream’ economists about the epistemological foundations of economics – but it *is* striking is that even the conservative scholars formulating an objective and value-free science as the ideal practice, have reached the conclusion

that this state of affairs is impossible. I will therefore focus on their arguments because if even these scholars acknowledge the unavoidable normativity in economics, we do not need to deal with more elaborated arguments. We only need to check whether recent methodological advances suggest the necessity of a reconsideration of these classical arguments.

One of the main conservative actors in the *Werturteilsstreit* was the economist and sociologist Max Weber. He clearly expressed his belief that an ideal social science would be entirely value-free and would be driven only by the scientists' thirst for knowledge (Weber, 1922a). This is why he is often quoted as an example for scholars arguing for the necessity of positive science. But in his writings he explicitly acknowledged that values and normative beliefs (unfortunately but) *necessarily* enter any analysis through the predispositions and experiences of the researcher (Weber, 1922a,b). This makes researchers from different persuasions hold different interpretations even for the (allegedly) simplest causal relationships between observed 'facts' (Weber, 1922a, p. 151). Even the choice of which questions warrant scientific inquiry is said to be driven partly by subjective idealist beliefs.² In his famous essay on the objectivity of the social sciences (Weber, 1922a), he is very explicit:

“It is correct that in our discipline the personal worldviews [“Weltanschauungen”] always bias the scientific argument, and affect the judgement of the importance of scientific arguments, even if one tries to identify the simplest causal relationships of facts, always depending on whether the result would relate well to the personal worldviews and ideals, and how it affects the possibility to achieve a personal aim” (Weber, 1922a, 151, translation by CG).

Weber's point of view is helpful for our discussion because it shows that even those who are often considered of advocating a *positive* social science in fact acknowledged a subjective and normative element in the social sciences. Something Weber also demanded from his fellows since the most objectionable practice for him to claim one's research to be absolutely value-free – something that in reality is simply impossible (Weber, 1922b).³

Strikingly, today the conventional wisdom in economics (in particular economic textbooks) has changed completely. many economists now take the misleading distinction between positive and normative economics as self-evident.

Representative for many, Milton Friedman claims in his famous (and notorious) essay on economic methodology that “economics can be, and in part is, a positive science” (Friedman, 1953, p. 3). He later explicates that „economics as a positive science is a body of tentatively accepted generalizations about economic phenomena that can be used to predict the consequences of changes in circumstances.“

Has there been any particular methodological innovation in the 20st century that has finally facilitated a truly positive science of economics? One of the leading economists of this century, Gerard Debreu (1991), describes very well that after WWII economics entered a phase of “intense mathematization” that profoundly transformed the profession. This mathematization is often considered an objectification of economics since the rules of mathematics as such are usually considered entirely objective.

This view has been explicated most clearly by Debreu himself who claims that the mathematical form of economic models can and is completely separated from their economic content. From this “divorce of form and content” (Debreu, 1986, 1265) it follows that every new interpretation of the formal economic models represents a new theory itself and that the mathematical model in its bare form exists *independent of any economic interpretation*.

From this perspective it is clear that economists (who are in this sense pure mathematicians) working on the development of the formal structure of the economic models (which are then essentially purely mathematical models) are concerned only with abstract mathematical objects without any content and thus no normativity at all.

But such arguments ignore the fact that the whole architecture economists use to build their models, in particular the fundamental assumptions (what Debreu (1986, 1265) calls the “primitive concepts” and their “representation as a mathematical object”, the first step of building a mathematical-economic model), always contain at least a grain of subjectivity and normativity. And no matter how ‘value-free’ a deduction in the resulting mathematical framework as such might be: if the axiom is subjective, so is the conclusion.

Another important reason for why the mathematization of economics cannot eliminate the subjective elements in the theory is the fact that mathematics itself is a very diverse field. Economists use some mathematical tools significantly more than others. It is precisely this choice of the mathematical representation of primitive theoretical concepts (cf. Debreu, 1981) that is led by the subjective epistemological interest (*Erkenntnisinteresse*) of the researcher and often entails additional subjective (and thus potentially normative) elements: to approach a problem from a general equilibrium or an agent-based perspective both represents a

mathematical strategy. Yet the implications are very different. Interestingly, Friedman is pretty explicit about this when he argues that “the ultimate goal of a positive science is the development of a ‘theory’” and that such a theory is a “‘language’ designed to promote ‘systematic and organized methods of reasoning’.” But which kind of language is to be chosen to build a model is still a decision to be made by the researcher and there are a lot of different languages, even within mathematics (see Hannappi (2008) for more details).

The next section will provide a more precise account of how formal models gain epistemic meaningfulness and how this process illustrates their inherent subjectivity in their specification.

3. THE POLITICAL ELEMENT OF FORMAL MODELS IN ECONOMICS – AN EPISTEMOLOGICAL PERSPECTIVE

Economists regularly use formal models to study economic and social systems. Whoever uses models must, of course, be able to explain how the models generate knowledge about these systems despite the fact that the models never represent an exact representation of reality. Unfortunately, economists are often not very explicit about how their models gain this epistemic meaningfulness. But the following framework provides both a philosophically well grounded and intuitive answer. It is built upon Uskali Mäki’s concept of “models as isolation and surrogate systems” (MISS) (Mäki 2009a, 2009b) which gets enriched by the work of Miller & Page (2007) and Gräbner (2015b).

According to the MISS approach, all models share two fundamental functions: in the first place they are built to *represent* the target system. This is required as real systems are regularly too complex to be understood directly. One thus reduces their complexity by abstracting away unnecessary details and focusing on particular aspects of reality. The result is a picture of reality that we shall call either a *surrogate* or *substitute* models (Mäki 2009a). A *surrogate* results from an active and reasonable attempt to learn something about reality and is therefore a direct representation of the real world situations under study. In contrast, we speak of a *substitute* if the act of representing reality was a failure. There are numerous reasons for such a failure: either the researcher was not successful in building a representation, e.g. by choosing a wrong form of representation, or because the researcher was more interested in studying a model for its own sake, and not to learn something about the particular target system. This act is illustrated in figure 1 by the function on the left: taking the target system R at $t=0$ and reducing its complexity yields the surrogate S_0 . We can think of the process of reducing the complexity as a mapping from the target system to the model.

The second function of a model is called *resemblance*. In the course of time the target system changes due to the mechanisms operating within it. These mechanisms are usually not observable and are never directly understandable. What researchers usually do is to build some artificial mechanisms into their surrogate, let them operate within the model and hope that the changes in the model correspond to those in the target system. In serious models, even the mechanisms within the models are not trivial, so one has to study the behaviour of the model e.g. by simulating the system while altering certain parameters or by deriving mathematical proofs. This process is called model *exploration*. If we can learn something about the real world by the exploration of our model, the model

resembles the real world and we have successfully increased our understanding of reality. Understanding here comes from the isolation of particular aspects of the target system.

Note that the act of ‘understanding reality’ can be interpreted in different ways. One may be interested in how certain variables in the real world change over time. One then wishes to infer facts about the real world. “How does GDP per capita changes over time?” would be a typical question. The crucial inference would be from the variable GDP in the model to the variable GDP in the real world. Another motivation for research may be one’s interest in the mechanisms operating in the real world. That is, one wishes to infer from the mechanisms built into the model to the mechanisms operating in reality. “Why does GDP evolve as it usually does?” or “What are the determinants of income inequality over time?” would be typical questions for this case.

Again, we can illustrate this exercise with figure 1: the mechanisms of the model are represented as the mapping s , the (unknown) mechanisms of the target system are represented via the mapping r . After the model world has evolved from S_0 to S_I at time $t=I$, one can then compare the resulting system S_I with reality R_I via the mapping h (i.e. comparing the resulting ‘facts’). Or, one may assess the plausibility of the mechanisms operating in the model compared with reality, which may or may not lead to a similarity between R_I and S_I .

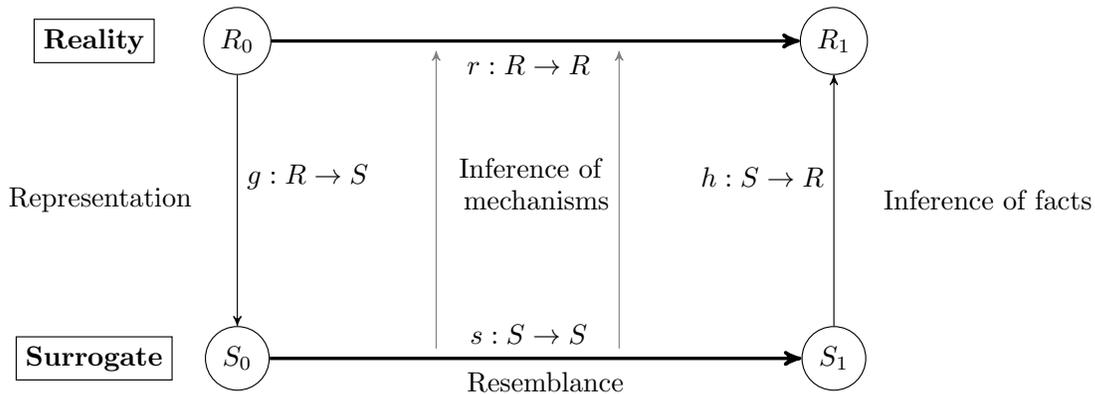


Figure 1: The MISS concept interpreted as a mapping process (Gräbner 2015b).

Here it becomes clear that different models differ both according to the mechanisms that operate within them (i.e. in their resemblance aspect), but also in the particular way they reduce the complexity of their target system (i.e. their representation aspect, or their complexity reduction function). The two aspects are related: certain mechanisms require a particular form of complexity reduction. If I want to model the dynamics resulting from the interactions among individuals and the emerging institutions. The complexity reduction function must preserve the notion of heterogeneous agents and their interaction structure. If I decide to model my target system using a single representative agent, such a form of resemblance is not possible any more.

This means that we can make a more general statement: it is not only the particular specification and interpretation of a model that is potentially normative - the choice of a particular *modelling framework* already explicates such kind of normativity: if a modeller decides to use a modelling framework that makes it impossible to study the consequences of direct interaction among heterogeneous agents then the

researcher might not actively decided against modelling this particular element of the target system, but she did so indirectly – with potentially important consequences: In the case of direct interactions, Albin & Foley (1992) showed that if one allows for direct bargaining in a general equilibrium model, then a unique equilibrium emerges, but with different distributional consequences than in the static version with a *tâtonnement* process. We will see in the section dealing with the models assessing the effects of TTIP that exactly this aspect is particularly relevant.

Such aspects of modelling are missed by Debreu's idea of a complete “divorce of form and content”. To capture all these aspects, Mäki (2009b) suggests a ‘functional decomposition approach to modeling’. According to this view, models consist of different components that serve different purposes, and that models are not only constraint by the characteristics of their target system (the so called *ontological constraints*), but also by the “modellers’ goals and contexts” (Mäki, 2009b, p. 179) – a clearly subjective and potentially normative element that can be explicated further.

Besides the mathematical form of the model, every model has a particular (potentially normative) *purpose* (Mäki, 2009b, Gier, 2006). But models are also addressed to a particular *audience*, usually to persuade it of the value of the model or to change its beliefs. There is also a model *description* and a *comment* on the model: a model in itself is just a bunch of equations or algorithms. For being a representation of reality it needs to be explained and interpreted by the modeller. Here, obvious degrees of freedom and thus room for personal judgements and tendentious descriptions exist – and researchers frequently use this freedom to

(implicitly) convey their messages. Again, excellent examples are provided by the studies on the impact of a potential free trade agreement between the USA and Europe as discussed below.

Thinking about models in this way is useful because it explicates the degrees of freedom researchers have, and the role played by subjective motives in using these degrees. Before we turn to the examples provided by the TTIP studies, we will make a few comments on the misconception that the degrees of freedom could also be eliminated by empirically derived ‘hard facts’ and that a positive economics can emerge from empirical, data-driven work.

4. A POSITIVE SCIENCE CANNOT BE CONSTRUCTED BY THE RETREAT TO EMPIRICAL ECONOMICS

During the last decades, theoretical economics has come under increasing critique. In particular when it comes to applied questions, more and more economists have become sceptical of economic theory. A common reaction is the retreat to less theoretical and more empirical work (Deaton, 2010). The intention is to substitute the imprecise theory for a more objective and scientific study of observed ‘facts’.

If it wasn’t the “mathematization of economic theory” that enables a positive science of economics, then could this trend be a potential evasion of the problem of the subjective element in economic theory: if there is no theory, there is no normative element either. This strategy has been articulated most clearly by the

advocates of *randomized controlled trials* (RCT) in economics. Banerjee (2007a), for example, calls RCTs “a new economics” that is able to produce the “hard facts” that lead to a thorough understanding of development processes and that is required to design reasonable and effective policy measures (Banerjee, 2007b).⁴

Note that in this argument, the clear distinction between positive economics in the sense of ‘what is’ and normative economics in the sense of ‘what should be’ gets explicitly blurred since the right policy follows directly from the ‘hard facts’. In this sense this statement implicitly acknowledges the failure of such a distinction.

Yet such an inductive and observation-driven strategy will always be unsuccessful in constructing a truly positive science of economics: Firstly, observations can usually be interpreted very differently (see again already Weber, 1922b, 463). Examples in economics abound. For example, there is serious disagreement of whether the spike in figure 2 represents either a speculative bubble (as argued by Richard Thaler on his presidential address at the AEA meeting in 2016, an interpretation that would not be consistent with the standard theory), or simply two successive changes of the risk preferences of people (as argued by Cochrane (2011), an interpretation that is consistent with the standard theory).

Secondly, even the positivist members of the *Wiener Kreis* admitted after the *Protokollsatzdebatte* in the 1940s that even the most basic observations, the so called protocol statements, cannot be considered fully objective elements.

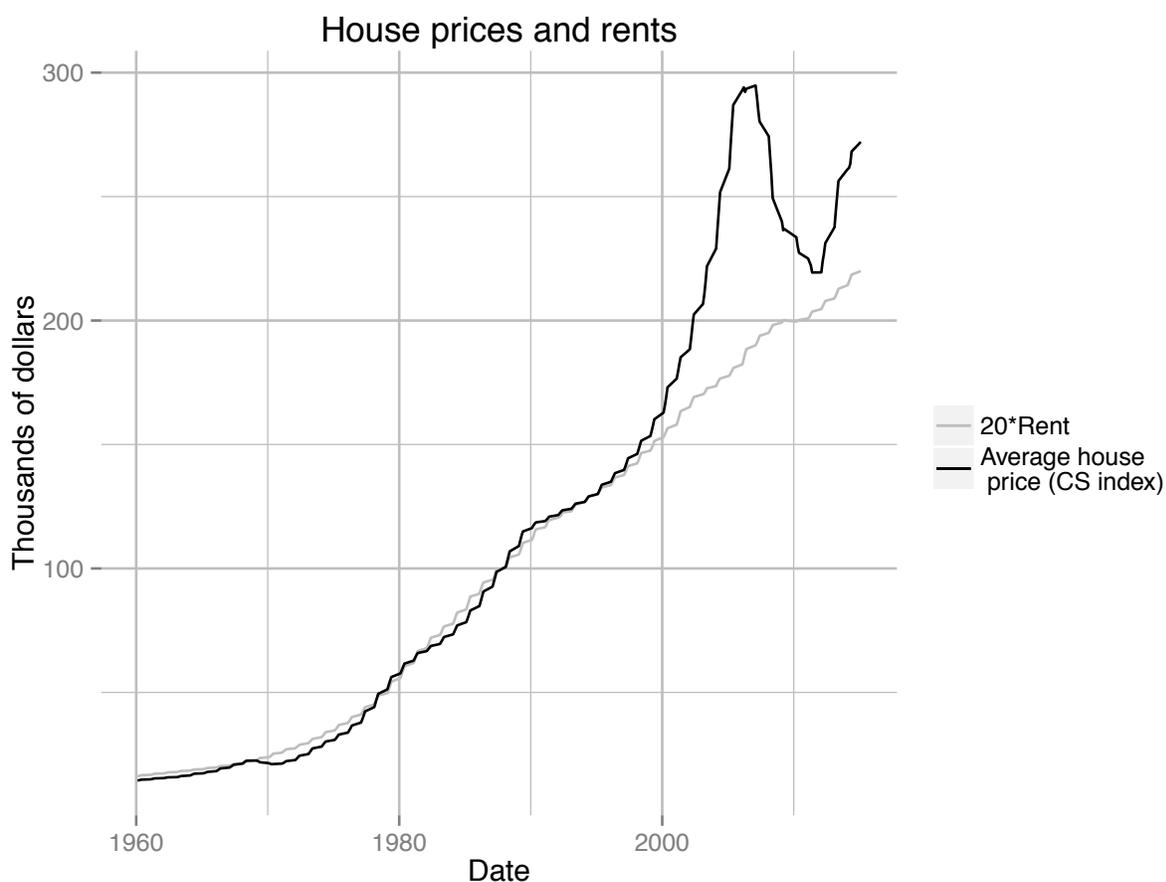


Figure 2: The evolution of rents and the Case-Shiller index measuring house prices. Is the spike a bubble or not?

But a strategy of focusing on empirical data to restore scientific objectivity will not only be unsuccessful in constructing a truly positive economics – it will also harm scientific progress in economics as such (Deaton, 2010). RCTs and other attempts to rely exclusively on the ‘data’ do not advance understanding at all because instead of explaining *why* a certain policy was (un)successful, such models focus on *whether* it was (un)successful. Speaking with figure 1: they only care about predicting facts, not about the understanding of how the model has resembled the mechanisms of reality. Without a reference on the precise mechanisms underlying

the success or failure of the policies nothing can be gained (Deaton, 2010, p. 426). And without reference to causal mechanisms, the results are almost useless for the design of policy measures since the policy implementation always differs from the trial – and since it is not clear *why* the trial was successful or not, nothing can be inferred about the potential success of the policy itself. The same argument applies for the external validity of the experiments: without a reference to the mechanisms that have led to the particular outcome of the study, it is by no means clear whether the results of the experiment can be generalized to other circumstances.

For example, the Mexican project of conditional cash transfer to parents who let their children attend school (called ‘Progresa’) was evaluated very successfully using RCT. But Levy (2006), the founder of this program, made very clear that one must not infer the applicability of the program to other institutional circumstances. Since the conditional cash transfer is likely to interfere with other habits, traditions or institutional arrangements such as particular poverty reduction programs. This relates to the argument of Page (2012) and Gräbner (2015b) that mechanisms can have very different effects when operating in the existence of other mechanisms, and that the resulting interaction effects are of great theoretical importance. Of course, to study such interaction effects between different mechanisms one requires particular methods, e.g. agent-based computational model (Gräbner, 2015a), which are still underdeveloped in most branches of economics.⁵

Of course, all these arguments in favor of theory-driven explanations do not only apply to RCTs and other experimental approaches, they address all of theory-free econometric work. Mechanisms are essential for understanding and mechanisms can only be suggested by good economic theory. Deaton makes his claim for the

focus on concrete mechanisms from a very pragmatic perspective. Gräbner (2015a) and Gräbner and Kapeller (2015) build upon Bunge (2004) and provide a general ontological and epistemological argument for a focus on mechanisms in providing economic explanations.

The direct consequence for applied economic research is that greater objectivity cannot be attained by focusing on the data alone. Rather, one must study a particular question with different models and theoretical approaches to reach a final conclusion. This is clearly beyond the scope of a single study. Thus, single studies should not sell their models as if they were the scientifically objective answer to a given problem. While this is increasingly common within the academic discourse (particularly in econometrics), studies addressed to policy makers reviewed in the next section are making exactly this mistake.

5. ILLUSTRATION: THE DEBATE ABOUT TTIP

The preceding elaborations were necessarily very theoretical. Here I will illustrate what has been said on the unavoidable normative character of economic theory by looking at an important example: The discussions about the TTIP agreement between Europe and the US are of essential political and societal importance and many economists have participated in the debate. Most of them argue in favor of the agreement, pointing to the potential gains of trade suggested by classical and neoclassical theory (Felbermayr and Larch, 2013). I will not enter the discussion about the precise effects of TTIP. Neither will I provide a thorough overview about all the existing studies and meta-studies. This work has already been done (for an

excellent critical overview see e.g. Raza et al. (2014)). Instead I will use the most influential studies on TTIP as an example for the theoretical elaborations made above: all these models clearly contain formal models associated with particular comments, interpretation, and audience-specific element – as suggested by the functional decomposition approach discussed above. As such, they also contain subjective and (in this sense) clearly political elements that are, unfortunately neither admitted nor discussed by the authors of the studies.

The studies thus also serve as an illustration that economic models consist of more than a pure mathematical model as it was claimed by Debreu (1981). Instead, the mathematical models are inseparable with the author's comments, interpretations, and economic ideals. This exercise shows the usefulness of Mäki's functional decomposition approach to modeling and illustrates a practice incompatible with Debreu's thesis of a separation of "mathematical form and economic content".

The result is a pseudo-scientific assessment of TTIP – exactly what Max Weber considered the worst case of scientific practice. This is representative of much of current economics and we will see that it is directly related to the narrow toolbox for the assessment of questions such as TTIP: an acknowledgement of the political element in formal modeling would imply the need for a methodologically more pluralistic assessment of policy relevant questions such as TTIP – and this is exactly what is not happening. In the following I will first give an overview of the models used for the TTIP studies, then provide some examples for their implicit political tendency, and finally make a constructive suggestion of how to improve the assessment of important political challenges such as the discussion about TTIP.

5.1. OVERVIEW OF THE EXISTING STUDIES AND THEIR METHODOLOGICAL FOUNDATION

While a couple of studies on TTIP exist, some of them have been of particular political relevance (Radax et al., 2014):

1. Berden et al. (2009) was the first study commissioned by the European Commission and it provides the (data) basis for many of the later studies on TTIP.
2. Felbermayr et al. (2013) is a report for the German Ministry for Economic Affairs, also carried out by economists from the IFO institute. It is available only in German.
3. Francois et al. (2013) is a more recent report by the Centre for Economic Policy Research (London, UK) commissioned by the European Commission and the basis for the following assessments of the Commission.
4. Fontagne, Gourdon and Jean (2013) provide a report for the French *Centre d'Etudes Prospectives et d'Informations Internationales*, which is dedicated to produce “independent, policy-oriented economic research” and which has produced *the* standard CGE model for trade analysis called MIRAGE (*Modelling International Relationships in Applied General Equilibrium*).

There are a couple of important similarities between the studies: all come to a positive conclusion concerning the welfare and employment effects of TTIP and suggest its implementation.⁶ Also, all of them use the same type of model, so called *computable general equilibrium (CGE) models*:⁷ Francois et al. (2013) use the CGE model developed by Berden et al. (2009), albeit with an updated version of the original data base. The model of Fontagne, Gourdon and Jean (2013) is of the

same modeling class, uses a very similar data base, and differs only in details (see Radax et al. 2014, 42).

Using the vocabulary of our methodological framework outlined above, the way these models reduce the complexity of the real world is – *by design of the models* – very similar. All of them use representative individuals and neglect heterogeneity of the actors involved, none of them considers uncertainty or negative externalities of abolishing regulations (such as pollution of impact on health) at all. Also the mechanisms considered in the models are very similar since prices dissipate through (by assumption) cleared markets which function through perfect or imperfect competition. This means that the technical design of almost all studies on TTIP are very similar. But the similarity is not limited to the technical design: also the other functional component of the models resemble each other:

they address a similar audience (policy makers). They provide very similar descriptions and the arguments for motivating the modeling frameworks (which is simply considered ‘standard’) and particular specifications. The absence of any methodological self-reflection, yet a particular (even verbal) sensitivity analysis concerning more critical assumptions is also missing from all elaborations. Even the commentaries that are used to relate these elements to each other are similar. All models consider themselves to be objective scientific assessments that in the end suggest a straightforward policy recommendation. This is interesting since one might argue that the choice of the CGE modeling framework is, given its prominent place in the economics curricula and its accepted place in the journal landscape, not surprising. But it *is* somehow surprising that all these different economists present and justify their models in such a similar fashion. In particular against the backdrop of the largely critical experiences made with CGE models, e.g. when estimating the

effects of the NAFTA trade agreement (see Radax et al., 2014, chap. 2.3), this is an alarming signal.⁸

5.2. EXAMPLES FOR THE POLITICAL ELEMENTS

A closer inspection of the models reveals that there is a large number of small but significant decisions to be made regarding the closure and parametrization of the models. In the following I will use three simple examples that illustrate the political element in the studies on TTIP. They address decisions made on different levels of specificity: firstly I argue that the choice of the CGE framework is normative as such since its form of complexity reduction excludes certain questions that can be of relevance for welfare assessments, e.g. the distributions of gains and harms of the agreement. The second point is similar but refers to the choice of using static models instead of dynamic ones. The third point does not refer to CGE modelling in general, but to the particular parameterization of the models.

The choice of the modelling framework has normative consequences and the description of the model results do not reflect this. Considering the presentation of the models, it has already been mentioned that authors do not discuss potential disadvantages of CGE modelling. The fact that the way these models reduce the complexity of the real world makes it particularly difficult to study questions of inequality, however, represents a particular political element in the choice of this framework – especially if this disadvantage does not get articulated and authors focus instead on describing other, allegedly positive effects. of the agreement that can be studied with CGE models. The fact, however, that inequality is an important

variable to be discussed in the context of free trade arrangements is, at least since the experience with NAFTA, very clear, (see e.g. Hanson (2003) or Abbot (2004)). So a direct reference to this potential disadvantage of the models in the model description of the comments on the results would have improved the analysis significantly. In the current form it is hard to neglect an implicit and intransparent political tendency in these studies.

The model framework and its specification: Static model and no adjustment

None of the studies considers the size and distribution of adjustment costs. This is clearly related to the modeling framework employed by the studies: CGE models often assume complete use of factor endowments – therefore all CGE models used do not feature unemployment by assumption. The authors justify this practice with their focus on the long-run effects of the trade agreement (i.e. at least ten years), but this clearly masks the negative effects of short- and medium-run unemployment. As illustrated by Radax et al. (2014), these adjustment costs are very likely to be distributed unequally with affecting low-skilled and older employees much harder (since they are more likely not find a new job at all). Again, this drawback refers to the fact that most CGE models are static by construction, and even ‘dynamic’ versions do not feature a dynamic analysis of the equilibrium adjustment paths.

The formal specification of the models One crucial decision within the model specification process is the specification or estimation of the model elasticities, in particular the *price elasticity of trade*. These parameters specify the sensitivity of exports to changes in the price of the product. Since price decreases have by assumption no negative impact on employment (and thus aggregate income), the

higher the price elasticity to trade, the higher the potential gains from a free trade agreement (Radax et al., 2014, p. 81). Several aspects are noteworthy: firstly, one might have expected to see a careful sensitivity analysis for such a fundamental quantity in the models. This is, however, not the case. Given the proficiency of the authors involved, this is surprising. Secondly, one might have expected the authors to use parameters well informed by the empirical literature. Again, the authors have decided differently: the elasticities used vary between 5% and 7%. Such a value is common for microeconomic studies – for macroeconomic applications values of 1.5-2.0% are considered realistic (Taylor & von Armin, 2006; Radax et al., 2014). Such choices make it hard to ascribe objectivity to these models – in particular because they do not explain their choice of parameters particularly transparently.

5.3. HOW AN ECONOMIC ASSESSMENT OF TTIP SHOULD HAVE LOOKED LIKE

How would a reasonable assessment of TTIP would look like if one had accepted the argument of section 2 that any economic model contains particular subjective (and thus potentially normative) statements, if one had considered the epistemological and methodological framework elaborated in section 3, and if one had been aware of the limitations of empirical work in yielding a truly positive science outlined in section 4? In the following I will make three very simple and immediate suggestions that – unintendedly – align very well with the research practice of Evolutionary Political Economy as outlined by Hannapi and Wäckerle (2015).

Firstly, any policy relevant study in economics should be transparent and fair concerning the (potentially arbitrary) assumptions one has made. This has already been emphasized by Max Weber and it is as true as ever: in any applied economic model, the modeller has certain degrees of freedom, e.g. by determining the elasticities of his CGE model, whose specification depends partly on their personal worldview. In this case he should just say so. It does not invalidate the model, in particular if he performed at least some rough sensitivity analysis. But it would certainly address the critique of making pseudo-objective science.

Secondly, any policy report must include a section on methodological considerations. With this I do not mean the description of the model, but a clear discussion of what modelling framework are principally at disposal, what are their respective advantages and disadvantages, why one has picked particular the one that has been used in the study, and what this means for the interpretation of the results.

Thirdly, ideally, extensive policy reports that are meant to form the basis for further political decision making (such as the study by Francois et al. (2013) for the European Commission) should include at least two competing (and distinctive) modelling frameworks. It is striking that there is not a single agent-based model used to analyse the effects of TTIP. Agent-based models would have yet another advantage: thanks to their flexibility that can be used to perform sensitivity analysis for the particular assumptions made in more analytical models. They are also predestined for a dynamic analysis of the TTIP effects, including adjustment costs. This argument does not only address the economists who write the report: politicians also should not think they can outsource their political decisions to an

objective scientific apparatus. Therefore they should consider different studies and demand a methodological and theoretical pluralism among the reports they base their decision on.

The last two points illustrate an important dilemma economics is currently facing: because it is methodologically extremely narrow, all studies on TTIP use – speaking with figure 1 – similar complexity reduction functions. An adequate assessment would have used different modeling approaches that have fewer results already built in in the assumptions of the model itself. No model will ever fit perfectly, and a rest of uncertainty about the outcome of policy measures will always remain. But this uncertainty could be reduced significantly if the variance of the complexity reduction strategies was larger. The current situation is thus also a direct consequence of the narrow curricula in economics education.

6. SUMMARY AND CONCLUSION

The starting point of the chapter was the question of whether economic modelling can be *positive* in the sense of *non-normative* or *value-free*. I showed that earlier social theorists did not believe in the existence of a positive economics in this sense – even if authors such as Max Weber considered it to be the ideal way of doing economics. We then continued to discuss the methodological trends and innovations in the 21st century and asked whether they enabled a positive economics. We concluded, however, that neither the mathematization of economics, nor a trend towards a-theoretical empiricism was able to reverse the original result.

Does the inevitability of a normative element in economic theory pose a threat to economics as a science? I do not think so. The fact that any economic model is to some extent normative does not mean it cannot be a good model that helps to understand the real world – even if such understanding will always remain provisional. But the current crises show that good economic analysis is needed more than ever.

But there are a couple of consequences that can be drawn from the elaborations above: firstly, a acknowledgement and transparent communication of normative elements in modelling and a transparent discussion of them increases the quality of scientific work. Secondly, flexible modelling techniques that are able to test for the particular consequences of certain complexity reduction strategies are required. Agent-based models immediately come to mind. They are able to test the relevance of assumptions that are inextricably linked with other modelling approaches such as CGE modelling: if one wishes to know whether agent heterogeneity is important, a CGE model does not help, since it cannot produce the counterfactual with heterogeneous agents. Generally, keeping in mind the case study of TTIP, the toolbox of economists seems to be far too narrow.

Therefore, a third conclusion would be a more pluralistic education of economists, including basics in economic methodology and epistemology. This should help them in communicating their research more transparently and engage in productive discussions of economists taking other approaches, instead of masking political elements in modelling and thus doing qualitatively poor pseudo-objective science.

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¹ The discussion was particularly important for the social sciences because observation of facts and mechanisms is much more difficult than in the natural sciences. But in particular in physics and similar debate took place with the result that all experiments and the corresponding interpretations always entail a particular subjective – and thus potentially normative – element.

² What Weber did reject was the possibility to derive certain normative values or *Weltanschauungen* scientifically. For him, these were ultimately a subjective matter derived from the fundamental ideals and beliefs of the individual researchers. He therefore also claimed that scientists should only study the potential effects of particular policy measures. But the ultimate decision which measures should be taken to achieve a certain aim is said to be beyond the realm of science and scientists should not engage in such discussions as scientists but only as politicians (Weber, 1922a).

³ A debate that was related to the *Werturteilsstreit* was the so called *Positivismusstreit* (Positivism dispute). But again, in this discourse both conservative thinkers such as Karl Popper, and progressive thinkers such as Jürgen Habermas agreed on the existence of a subjective element in the social sciences.

⁴ This form of argument has already been anticipated and criticized by Weber who writes: “I will not discuss, but explicitly acknowledge that particularly the alleged eradication of any practical judgment, in particular of the form that one ‘let’s the facts speak for themselves’, suggestively evokes exactly this kind of judgment” (Weber, 1922b, 460, translation by CG).

⁵ Similar arguments have been made by institutionalist economists ever since. But they are often sceptical of identifying general mechanisms in economics at all. I do not share this pessimism and think that mechanisms with a certain generality can indeed be identified (and classical institutionalists have indeed done this very successfully, e.g. Gunnar Myrdal with his *backwash* and *spread effects*, or his notion of *circular cumulative causation*). The argument is also similar to the elaborations of Pawson and Tilley (1997) who accentuate the importance of the environment in which particular mechanisms operate.

⁶ Their suggestion in favor of TTIP is a clearly normative statement. All studies, however, base this statement on an allegedly thorough and positive analysis of what are the effects of the free trade agreement. This kind of schizophrenia can also be observed in Felbermayr and Larch (2013), a journal article providing answers to the most urgent answers around TTIP. In this context it is striking that even in the explicitly normative part of the reports, the authors do not discuss the many implicit assumptions inherent in their welfare analysis. In particular, they do not discuss any negative implications of abandoning certain legal rules as a measure to reduce non-tariff trade barriers. To the contrary: lowering the regulation standard as a means to harmonize the regulatory framework is considered as welfare-enhancing. There is not a word about regulations addressing negative (and welfare decreasing) negative external effects or protecting the interests of the individuals.

⁷ One study that is not discussed here, Felbermayr, Heid, and Lehwald (2013), takes indeed another approach. It is a report resulting from a study commissioned by the business-friendly Bertelsmann-Stiftung and carried out by economists mainly affiliated with the IFO institute in Munich, Germany. Here the authors try to answer a different question, namely how much liberalization is required to reach a certain positive effect. While the authors use CGE modeling only sparingly, the theory underlying their econometric model is very similar to the ones discussed here.

⁸ An even more dubious practice is that some of the authors of a TTIP study write in another article (Felbermayr and Larch, 2013) that nearly all studies on TTIP find positive effects – and use this as an argument for the validity of the studies. No mentioning that all underlying models are very similar and use almost the same data seems to be an (at best unintended) deception.