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Intension, extension, and the model of belief and knowledge in economics

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Abstract: This paper investigates a limitation of the model of belief and knowledge prevailing in mainstream economics, namely the state-space model. Because of its set-theoretic nature, this model has difficulties in capturing the difference between expressions that designate the same object but have different meanings, i.e., expressions with the same extension but different intensions. This limitation generates puzzling results concerning what individuals believe or know about the world as well as what individuals believe or know about what other individuals believe or know about the world. The paper connects these puzzling results to two issues that are relevant for economic theory beyond the state-space model, namely, framing effects and the distinction between the model-maker and agents that appear in the model. Finally, the paper discusses three possible solutions to the limitations of the state-space model, and concludes that the two alternatives that appear practicable also have significant drawbacks.

Keywords: intension, extension, belief, knowledge, interactive belief, state-space model

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Much of current mathematical economic analysis is based, in one way or another, on the theory of sets initiated by Georg Cantor (1883) and Richard Dedekind (1888) in the late nineteenth century, axiomatically developed by Ernst Zermelo (1908), Abraham Fraenkel (1923), John von Neumann (1928), and others in the early twentieth century, and usually

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known as Zermelo-Fraenkel set theory. In the set-theoretic mathematical approach, the objects of economic interest are conceptualized as elements of some set, e.g., the set of commodity bundles or the set of actions available to a certain player. The features of these sets, e.g., their being closed or convex, are typically attributed with some important economic meaning. Preferences, technologies, strategies, and other economically relevant notions are modeled as functions that associate the elements of one set with the elements of another set.[1]

The model of belief and knowledge prevailing in contemporary mainstream economics and used especially in information economics and game theory is also set-theoretic in nature. This model was introduced by Robert Aumann (1976); it formalizes beliefs and knowledge as two set-theoretic operators B and K that map subsets of a universal space Ω into other subsets of the same space. Aumann's model and its generalizations have been variously labeled as "the event-based approach to belief and knowledge", "possibility correspondence model", "possible world semantics", "Aumann structures", and "the state-space model of belief and knowledge". This last name will be adopted here. In the state-space model, the difference between belief and knowledge is that, while belief can be false, knowledge is assumed to be truthful. In other words, knowledge is a special case of belief, namely true belief.[2]

As any other economic model, the state-space model displays a number of limitations. Some of them have been discussed already in the philosophical, economic, and artificial intelligence literature under the banner of the logical omniscience problem. In particular, it has been pointed out that the model implies the *monotonicity* of beliefs and knowledge, i.e., that an individual believes or knows the implications of what she believes or knows. For instance, if an individual knows the axioms of a mathematical system, monotonicity says that she also knows all the theorems that are valid in the system (Fagin, et al. 1995, ch. 9). Another implausible feature of the state-space model is that it rules out the realistic possibility that an individual may be completely

[1] On the history of set theory and how it spread into economics, see Ferreirós 2007; Giocoli 2001; Weintraub 2002.

[2] For philosophers, the truthfulness of belief is a necessary but not sufficient condition for knowledge, since the latter requires that the true belief is justified. A number of examples originally put forward by Gettier (1963) have suggested that even justified true belief does not warrant proper knowledge. For an introductory discussion of the definition of knowledge as justified true belief and its refinements, see Steup 2012.

unaware of an event rather than believing or not believing it (Dekel, et al. 1998).

In this paper, I focus on a less explored limitation of the state-space model, which is strictly related to its set-theoretic nature and, more specifically, to the first axiom of the Zermelo-Fraenkel set theory. This first axiom is usually labeled as the *axiom of extensionality* and it states that two sets are equal if and only if they collect the same elements, i.e., that the extension of a set as defined by the elements belonging to it fully characterizes the set. A consequence of this axiom is the so-called *principle of substitutivity*. According to this principle, if two sets collect the same elements, whatever holds true for one set also holds true for the other, and thus one set can be substituted for the other *salva veritate* (preserving truth).

The axiom of extensionality and the principle of substitutivity are less obvious than appears at first sight. At least since the late nineteenth century, philosophers such as Gottlob Frege (1948 [1892]) have pointed out that in a number of contexts the substitutivity principle may fail. Among these contexts are those involving beliefs and knowledge. For instance, although the expressions "Morning Star" and "Evening Star" identify the same planet, i.e., Venus, an individual may (truthfully) believe that the planet that appears in the eastern part of the sky in the morning is Venus, while she (erroneously) believes that the planet that appears in the western part of the sky in the evening is not Venus.

These kinds of situations can be conceptualized through the notions of *intension* and *extension* (Carnap 1947). The *intension* of a linguistic expression is what the expression means, i.e., the notion or idea it conveys. The *extension* of an expression is the set of things it designates or applies to. So, for instance, the intension of the term "computer" is the idea of an electronic machine that can store, retrieve, and process data, while its extension is the set of existing computers. In the astronomical example above, we have two expressions that have different intensions ("Morning Star" conveys a different meaning from "Evening Star") but the same extension (the planet Venus). Using the intension-extension terminology, Frege and others drew attention to the concern that the substitutivity principle may be invalid for beliefs and knowledge involving expressions with the same extension but different intensions, and therefore its unchecked application can lead to mistaken conclusions.

The limitation of the state-space model I focus on in this paper is that the operators B and K automatically implement the substitutivity principle also for expressions with the same extension but different intensions, that is, also if substitutivity is not warranted. In the first place, this feature of B and K generates puzzling results that concern what an individual believes or knows about the world. Furthermore, that feature of B and K generates puzzling results pertaining to interactive beliefs and knowledge, i.e., the beliefs and knowledge that an individual has about what other individuals believe or know about the world. In particular, the model may imply that an individual knows that another individual believes a certain event, even independently of any specific assumption about what the former individual knows about the way the information is imparted to the latter. I connect these puzzling results to two issues that are relevant for economic theory, namely those concerning framing effects and the distinction between the viewpoint of the model-maker and the viewpoint of the agents in the model.

In the final section, I discuss three possible ways of modifying the operators B and K and/or the state-space model, in order to make them capable of capturing the difference between intensions with the same extension. While a first approach based on a re-definition of B and K appears barren, the other two lines of attack—re-defining the universal space Ω or adopting the so-called syntactic approach—are more promising but they also present significant drawbacks.

The difficulties that standard set-theoretic economic models have in capturing the difference between different intensions with the same extension have rarely been discussed explicitly in the economics literature. I am aware of four exceptions. Kenneth Arrow (1982, 6) mentioned that, contrary to the implicit assumption of standard economic theory, preferences for commodities may violate the axiom of extensionality. Michael Bacharach (1986, 182-183) added that, contrary to standard probability and decision theory, beliefs may also violate extensionality. Arnis Vilks (1995, 195-199) pointed out that standard mathematical economics is rooted in the extensional setting of the Zermelo-Fraenkel set theory, and therefore is ill-suited for modeling preferences and beliefs, for which extensionality may not hold. Sacha Bourgeois-Gironde and Raphaël Giraud (2009) connected framing effects with violations of extensionality and proposed by-passing extensionality through a modified version of the so-called Bolker-Jeffrey decision model.

The present paper expands on these previous contributions in a numbers of ways: it analyzes in detail how the extensional nature of B and K generates puzzling consequences in the state-space model; it calls attention to the puzzling implications that the state-space formalism has in relation to interactive beliefs and knowledge; it brings into play the notion of intension and shows how the intension-extension dichotomy helps us to better understand the limitations of the state-space model; finally, it discusses some possible solutions to the limitations of B, K, and the model.

It is important to stress that the goals of the paper are "informative" and "diagnostic", rather than "therapeutic". The paper has an informative aim in the sense that it calls the attention of economists to problems related to the relationship between intension and extension that, although typically ignored in the economic literature, are relevant for economic analysis. The paper is diagnostic in that it explains why the state-space model has difficulties in dealing with the intension-extension difference. The paper also ventures into the therapeutic realm by discussing some possible solutions to these difficulties. However, it does not provide a systematic solution to the problems that it calls attention to.

Section 1 provides some further insights from the philosophical literature on the relationship between intension and extension, and shows how these insights can be used to conceptualize framing effects and the distinction between model-maker's and agents' viewpoints in the model. Section 2 reviews the state-space model of belief and knowledge. Section 3 illustrates how the set-theoretic nature of the operators B and K generates puzzling results concerning what an individual believes or knows about the world. Section 4 investigates the puzzling implications of the state-space model relative to interactive beliefs and knowledge. Section 5 discusses possible solutions to the limitations of B, K, and the state-space model. Section 6 sums up the paper.

1. ON INTENSION AND EXTENSION

As mentioned in the Introduction, philosophers have explored issues related to the relationship between intension and extension at least since the late nineteenth century.[3] These explorations are related to philosophical efforts to clarify the proper content and the cognitive value of linguistic expressions and thoughts. One may even say that large parts of twentieth-century philosophy of language and of mind are somehow related to issues concerning the relationships between intension and extension. I will not attempt to provide an overview of this immense literature.[4] Instead, I will mention some insights from this philosophical literature that may help the reader with an economic background to see the larger set of problems that the issues addressed in this paper relate to, and help the reader with a philosophical background to connect the paper with questions with which she is already familiar.

In the first place, situations involving beliefs and knowledge belong to a larger family of cases in which the relationship between intension and extension can be tricky. In the current philosophy of language and of mind these cases are usually labeled as "referentially opaque contexts". Besides those involving beliefs and knowledge, other referentially opaque contexts are those involving propositional attitude verbs like "prefers", "desires", "hopes", "wants", or "says"; in these contexts the substitutivity principle may also fail: Ann may desire to see the Morning Star while she does not desire to see the Evening Star. Other referentially opaque contexts where the substitutivity principle could become problematic are those in which modal verbs such as "it is necessary that" or "it is possible that" are used. For instance, although it is true that "the number of planets in the solar system is 8", and that "8 is necessarily greater than 7", it is false that "the number of planets is necessarily greater than 7".

Furthermore, the reference of an expression can be opaque not only because there are extensions with multiple intensions as in the cases mentioned above, but also because there are intensions without any actual extension, as in the case of expressions indicating fictional entities such as "unicorn" or "the round square". And there are also intensions whose references are context-dependent and thus opaque, as in the case of indexical expressions such as "I", "that", or "here".

A further point discussed in the philosophical literature is the failure of the substitutivity principle even for so-called *hyper-intensional*

[3] The expressions "intension" and "extension" derive from Carnap (1947). Frege (1948 [1892]) talked, in an analogous manner, of "sense" and "reference".
[4] For an introduction, see Bealer 1998; McKay and Nelson 2010.

propositions—that is, for propositions expressing mathematical truths and other supposedly necessary truths. Consider these additional expressions having different intensions but equal extension: “Tegucigalpa” and “the capital of Honduras”, “equilateral triangle” and “equiangular triangle”, and a hyper-intensional proposition such as “51” and “17 × 3”. Bob may know that the capital of Honduras is not in Nicaragua while believing that Tegucigalpa is in Nicaragua. Carl could know that the triangle in front of him is equilateral without realizing that it is also equiangular. One might think that the failure of the substitutivity principle in these situations depends on the fact that the extensional equality among those propositions is only accidental, that is, non-necessary. Tegucigalpa and the capital of Honduras have the same extension in the actual geopolitical world, but may have different extensions in another possible world. Equilateral and equiangular triangles coincide in Euclidean geometry, but may differ in some non-Euclidean system. However, the principle of substitutivity can fail even in hyper-intensional contexts where extensional equality holds in every imaginable universe. Thus, although it is always the case that $17 \times 3 = 51$, David may know that 17×3 is not prime, but not know that 51 is not prime.

In the last sixty-five years, philosophers and logicians such as Rudolf Carnap (1947), Alonzo Church (1951; 1973; 1974), Richard Montague (1960; 1970), Daniel Gallin (1975), Edward Zalta (1988), Melvin Fitting (2004) and others have developed logical systems called *intensional logics* that are aimed at capturing explicitly the difference between intension and extension. However, a number of problems still remain unresolved and none of these intensional logics has gained general acceptance (Garson 1998; Fitting 2011).

Although the notions of intension and extension have been used only very rarely in the economics literature, they can be fruitfully employed to conceptualize two issues that are relevant for economic theory, namely those concerning framing effects and the relationship between the viewpoint of the model-maker and the viewpoint of the agents in the model.

Framing effects occur when different descriptions of the same object lead to different beliefs, preferences, or decisions concerning that object. For instance, in a series of well-known experiments performed by Amos Tversky and Daniel Kahneman (1981), a group of individuals was asked to state their preferences between different lotteries.

A number of these lotteries were identical in terms of final outcomes and probabilities, but some of them were framed as one-stage games and others as two stage-games.[5] Tversky and Kahneman recorded that a majority of subjects changed their preferences between identical lotteries according to the way these lotteries were framed.

This effect can be aptly expressed in terms of intension and extension: the lotteries Tversky and Kahneman submitted to their experimental subjects were extensionally equivalent with respect to their outcomes and probabilities, but intensionally different because one-stage games and two-stage games frame uncertainty differently. More generally, framing effects may be conceived of as the effects, on beliefs, preferences or decisions, of intensionally different descriptions of an extensionally single object.

Orthodox economists tend to discard framing effects as manifestations of the irrationality of individuals who simply fail to recognize that identical things are indeed identical.[6] In opposition to this view, Tversky and Kahneman and other behavioral economists have argued that framing effects significantly influence economic behavior and therefore cannot be discarded without weakening the descriptive significance of economic theory; moreover, some framing effects seem to have a rational justification.[7]

If we look at framing effects using the notions of intension and extension, they no longer appear to be manifestations of irrationality. Rather, they seem to be just other instances of the failure of the substitutability principle in referentially opaque contexts. Therefore, when looked at from the intension-extension viewpoint, the relevant problem shifts from the issue concerning the individuals' rationality, to the question of whether standard, set-theoretic economic models are able to capture the intensional difference between extensionally equal objects. In Section 3, we will investigate in more detail why the

[5] For instance, Lottery F was described as a one-stage game offering a 20% chance to win \$45, and an 80% chance to win nothing. Lottery D was described as a two-stage game. In the first stage, Lottery D offered a 75% chance to end the game without winning anything, and a 25% chance to move into the second stage. In the second stage, Lottery D offered an 80% chance to win \$45, and a 20% chance to win nothing (Tversky and Kahneman 1981, 455). Simple math shows that the final combinations of outcome and probabilities offered by Lotteries D and F are identical.

[6] For instance, preference change between probabilistically equivalent lotteries is usually associated with inconsistency in dynamic choice. For a discussion, see Wakker 1999.

[7] For discussion on this topic, see Kahneman and Tversky 1984; Tversky and Kahneman 1986; Bourgeois-Gironde and Giraud 2009.

set-theoretic operators B and K tend to pass over framing effects in the state-space model.

The notions of intension and extension may also be useful in keeping distinct the information and reasoning abilities possessed by the model-maker from the information and reasoning abilities possessed by the agents in the model. In effect, the model-maker often tends implicitly to endow the agents in the model with the comprehensive information and the sophisticated reasoning abilities she possesses. As a consequence, the agents may display beliefs and behavior that appear puzzling, or the results of the model may in fact depend on a number of hidden assumptions concerning the agents' information and reasoning abilities.[8]

The puzzling results concerning interactive beliefs and knowledge in the state-space model—discussed in Section 4 of this paper—can be seen as depending on the hidden assumptions about the agents' information and reasoning abilities harbored by the state-space model. The point I would like to make here is that one way in which the confusion between the model-maker's and the agent's viewpoints may enter into economic models is through the identification of objects that are extensionally equal for the model-maker but intensionally distinct for the agents. Therefore, the notions of intension and extension may help the model-maker to avoid treating in an identical way objects that are equivalent for her but different for the economic agent, and thus enhance transparent and rigorous economic modeling.

After these extensive introductory considerations, we can finally move to the state-space model and discuss how belief and knowledge are modeled in it.

2. BELIEF AND KNOWLEDGE IN STATE-SPACE MODEL

In this section, I first outline the formalism of the state-space model and then explain how its various elements can be interpreted (more detailed presentations of the state-space model can be found in Osborne and Rubinstein 1994; Dekel and Gul 1997; Battigalli and Bonanno 1999; Samuelson 2004).

Consider a set Ω whose generic element is ω, and a correspondence $P:\Omega \rightarrow 2^{\Omega} \setminus \{\varnothing\}$ that associates each element $\omega \in \Omega$ with a set $P(\omega)$ of

[8] For a discussion of these issues in game and decision theory, see Brandenburger 1992; Battigalli and Bonanno 1999; Dardi 2004.

elements of Ω (2^{Ω} is the set of all subsets of Ω). Based on P, define an operator $B: 2^{\Omega} \to 2^{\Omega}$ as follows: for every $E \subseteq \Omega$, $B(E) = \{\omega \in \Omega : P(\omega) \subseteq E\}$.

Set Ω is the set of the possible states of the world. A state $\omega \in \Omega$ specifies all aspects of the world that are relevant to the situation, such as "it rains" or "individual i has transitive preferences". Relevant aspects of the world may include individuals' beliefs, such as in the case of "individual i believes that it rains" or "individual j believes that individual i believes that it rains". Only one state of the world is the true one, but the individual may be uncertain which one that is. This uncertainty is modeled by a correspondence P, which associates each state ω with the set of states that the individual regards as *possible* at ω. This is why P is called a *possibility correspondence*.

The possibility correspondence of an individual expresses formally the way information is imparted to her. In the economics of information, this information may be seen as coming from some signal: when the true state of the world is ω, the individual receives a signal suggesting to her that the true state is in the subset $P(\omega)$ of Ω. In game theory, ω and $P(\omega)$ may be seen, respectively, as a node in a game of incomplete information and as the information set associated with that node. At any rate, notice that possibility correspondences are just a tool that the external, omniscient model-maker employs to encode and represent individuals' beliefs, not something that these individuals themselves need be aware of.

A subset $E \subseteq \Omega$ is called an *event*, and can be thought of as the collection of all states that share a certain feature. For instance, the event "it rains" collects all states $\omega \in \Omega$ characterized by rain. Note that, if $P(\omega) \subseteq E$ in all states the individual regards as possible in ω, the event E occurs. The operator B is interpreted as a belief operator: if $\omega \in B(E)$, i.e., if $P(\omega) \subseteq E$, then at ω the individual believes that the event E occurs, and this is because in every state the individual regards as possible in ω—that is, in $P(\omega)$—the event E occurs. Observe that $B(E)$ is itself an event, the event "the agent believes E". As such, $B(E)$ may become the object of further belief for another agent. We will return to this in Section 4.

As a simple illustration of the model, suppose that an individual—let us call her Ann—is interested in a variable v that can take values from 1 to 4, like a four sided die, and that each state of the world is completely characterized by the value taken in it by v. There are thus four possible states of the world: $\Omega = \{\omega_1, \omega_2, \omega_3, \omega_4\}$. Imagine that the possibility correspondence of Ann in ω_1 is as follows: $P_A(\omega_1) = \{\omega_1, \omega_2\}$. This means that if $v = 1$, Ann is uncertain whether $v = 1$ or $v = 2$, and considers both states of the world possible.

Consider now the event R "v is equal to 1", which occurs only at state ω_1, i.e., $R = \{\omega_1\}$, and the event S "v is not greater than 2", which occurs at states ω_1 and ω_2, i.e., $S = \{\omega_1, \omega_2\}$. In ω_1, does Ann believe R? No, she does not, because in ω_1 she also considers possible ω_2, which is not included in R. Formally: $P_A(\omega_1) = \{\omega_1, \omega_2\} \not\subset R$. In ω_1, however, Ann does believe S, because in every state she regards as possible in ω_2—that is, in $P_A(\omega_1) = \{\omega_1, \omega_2\}$—the event S occurs: $P_A(\omega_1) = \{\omega_1, \omega_2\} \subseteq S$.

As it happens, beliefs may turn out to be incorrect, and in this case the event believed by the individual does not occur. The state-space model is flexible enough to capture this situation. Imagine that the possibility correspondence of Ann in ω_4 is as follows: $P_A(\omega_4) = \{\omega_1\}$. This means that if $v = 4$, for some reason Ann erroneously believes that $v = 1$. Consider again the event S "$v \leq 2$". In ω_4, does Ann believe S? Yes, she does, because in ω_1, the only state she erroneously regards as possible in ω_4, v is not greater than 2. Formally: $P_A(\omega_4) = \{\omega_1\} \subseteq S$. However, this belief is false.

As mentioned in the Introduction, the primary characteristic that distinguishes knowledge from belief is that while belief can be false, knowledge is always true. In the state-space model this property of knowledge is captured by the inclusion of the true state of the world ω among those the individual regards as possible in ω itself.

Formally, knowledge requires that $\omega \in P(\omega)$. In fact, if $\omega \in P(\omega)$, when at ω the individual believes event E, i.e., when $P(\omega) \subseteq E$, the event E actually occurs in ω, i.e., $\omega \in E$. Based on this, we can define a knowledge operator $K: 2^\Omega \to 2^\Omega$, which is a refinement of the belief operator B, as follows: $K(E) = \{\omega \in \Omega : \omega \in P(\omega) \wedge P(\omega) \subseteq E\}$.

In our example, when $v = 1$ Ann not only believes that the event v is not greater than 2, but her belief is true, and so she knows that event. On the contrary, when $v = 4$ Ann believes that v is not greater than 2, but since $\omega \notin P(\omega)$ she is wrong.

In the literature on the state-space model, the terminology concerning belief and knowledge is ambiguous. Sometimes the term "knowledge" is used in a broad sense to express what in this paper is called "belief". In this broad sense, knowledge may be false. This usage, however, seems at odds with the basic philosophical notion of knowledge according to which truthfulness is a necessary condition for knowledge. At other times the term "belief " is used in a strict sense to express what in this paper is called "knowledge", that is, it is assumed that beliefs are always true. This assumption is called the *truth axiom*, and formally it requires that $\omega \in P(\omega)$ for every state of the world ω belonging to Ω. This second approach also seems problematic, because eliminating false beliefs may prevent us from understanding the numerous situations in which actual individuals have indeed false beliefs about economic variables. I therefore maintain the distinction between beliefs, which can be false, and knowledge, which is truthful by definition.

It is important to stress that for the argument made in the paper the truth axiom is not needed. The intension-extension issues addressed here concern both true and false beliefs, i.e., the operator B as well as the operator K. In effect, we could have limited our discussion to the general case of belief, without dealing with the particular case of true belief or knowledge. But since the literature on the state-space model generally refers to knowledge, it seemed appropriate to discuss knowledge explicitly.[9]

3. BELIEF AND KNOWLEDGE, WITHOUT INTENSION

In the Introduction and Section 1 we have discussed intension and extension as related to linguistic expressions. In the state-space model we do not find linguistic expressions but subsets of Ω called events, which are however interpreted as set-theoretic images of linguistic expressions like "it rains" or "v is not greater than 2". The formal link between a given linguistic expression and its set-theoretic image is provided by a correspondence that associates the expression at issue with the subset of states of the world where the expression is true. In this set-theoretic translation, linguistic expressions come to be identified with their extensional correlates in Ω, and thus lose their intensional dimension.

For instance, in our illustration of the state-space model we have labeled the event "v is not greater than 2" as S, and observed that S occurs at states ω_1 and ω_2. Consider now the event "v is not greater

[9] In addition to the truth axiom, the state-space literature generally imposes a second axiom that warrants some nice properties of the knowledge operator K, and formally states that if $\omega' \in P(\omega)$, then $P(\omega') = P(\omega)$. Our discussion is also independent of this second axiom.

than the 12th root of 4096", which can be labeled as T. T exhibits an intension distinct from S, but in fact the two events have the same extension—for the 12th root of 4096 is 2—and thus the same set-theoretic image, given by $\{\omega_1, \omega_2\}$. By the axiom of extensionality of the Zermelo-Fraenkel set theory, S and T are equal: $S = T$.

Consider now the set-theoretic operators B and K, which map subsets of Ω into other subsets of Ω. By their very nature, B and K map and cannot avoid mapping equal input-sets into equal output-sets:

If $S = T$, then $B(S) = B(T)$ and $K(S) = K(T)$.

Therefore, since in the state-space model the events "$v \leq 2$" and "$v \leq \sqrt[12]{4096}$" have the same extensional, set-theoretic correlate, if Ann believes (knows) one event, she also believes (knows), and cannot avoid believing (knowing), the other event.[10]

This implication illustrates, albeit in the over-simplified setting of our numerical example, the failure of the operators B and K to account for framing effects. The events $v \leq 2$ and $v \leq \sqrt[12]{4096}$ are extensionally equal but intensionally different, and we may well imagine that Ann reasonably believes that $v \leq 2$ without believing that $v \leq \sqrt[12]{4096}$. But, because of their extensional character, the operators B and K automatically implement the substitutivity principle, and therefore are unable to accounting for the intensional difference between $v \leq 2$ and $v \leq \sqrt[12]{4096}$.

In attempting to connect this abstract numerical example with more realistic economic situations, we may interpret v as any parameter relevant to the individual, while "$v \leq 2$" and "$v \leq \sqrt[12]{4096}$" could stand for, respectively, a "plain" way and a "convoluted" way of presenting that parameter. For instance, v could be associated with the probability of defaulting on a loan, while $v \leq 2$ and $v \leq \sqrt[12]{4096}$ could stand, respectively, for a "plain" and a "convolute " way in which a bank can present that likelihood to a potential subscriber of the loan. We may imagine that the individual's beliefs about v, and accordingly

[10] Monderer and Samet (1989) and others added a probability structure to the state-space model in order to express different degrees of belief about an event, that is, to express statements such as "Ann believes event *E* with probability at least *p*". Since probabilities apply to sets, the problematic feature of the basic state-space discussed here concerns also its probabilistic extensions: if Ann believes that $v \leq 2$ with probability at least *p*, she also believes that $v \leq \sqrt[12]{4096}$ with probability at least *p*.

her willingness to subscribe the loan, depend on the way the bank presents to her the likelihood of defaulting. The point I want to make here is that the operator B fails to capture the intensional difference between the plain and convoluted framings of that likelihood.

4. INTENSION AND EXTENSION FOR INTERACTIVE BELIEFS AND KNOWLEDGE

Interactive beliefs and interactive knowledge are, respectively, beliefs or knowledge that an individual has about what other individuals believe or know about the world. Interactive beliefs and knowledge are often economically relevant since in many cases individuals take action on the basis of them. Consider, for example, a standard principal-agent situation in which both the principal and the agent are interested in a certain event E that typically is the result of the agent's effort. If the agent believes that the principal does not know his effort he may choose a low effort, while if the agent believes that the principal knows his effort he will probably choose a high effort.

In effect, there are multiple levels of interactive beliefs and knowledge. The first level is the one discussed above: it concerns what an individual believes or knows about what other individuals believe or know about the world. The second level concerns what an individual believes or knows about what other agents believe or know about her/his beliefs or knowledge of the world. The staircase of levels of interactive beliefs and knowledge escalates in a predictable way, and game theory has shown that interactive beliefs and knowledge of higher or even infinite levels are of great consequence in strategic environments (see, e.g., Rubinstein 1989).

The state-space model makes it possible, not only to represent what individuals believe or know about the world, but also to represent their interactive beliefs and knowledge at any possible level. However, the model's difficulties in capturing the intensional difference between extensionally equal events and the puzzling consequences this generates extend also to interactive beliefs and knowledge. To keep things simple, we focus here on the first level of interactive beliefs and knowledge.

Let us add another agent—Bob—to our illustration of the model. Imagine that Bob is interested in the same variable v (taking values from 1 to 4) in which Ann is interested. Assume that Bob's possibility correspondence P_B is as follows:

$$P_B(\omega_1) = P_B(\omega_2) = \{\omega_1, \omega_2\},\ P_B(\omega_3) = P_B(\omega_4) = \{\omega_3, \omega_4\}.$$

Consider now the event U "$v \leq 3$" that occurs at states ω_1, ω_2, and ω_3: $U = \{\omega_1, \omega_2, \omega_3\}$. In which states of the world does Bob believe that $v \leq 3$? It easy to show that these states are ω_1 and ω_2. That "Bob believes that $v \leq 3$" is itself an event occurring in those two states, expressed as $B_B(U)$: $B_B(U) = \{\omega_1, \omega_2\}$. Notice that the event S "$v \leq 2$" that we dealt with in the previous sections, and the event $B_B(U)$ "Bob believes that $v \leq 3$", occur in exactly the same states of the world ω_1 and ω_2: $B_B(U) = S$. Therefore, although the two events are intensionally distinct, their set-theoretic image in our simple Ann-Bob state-space is the same.

Imagine now that the true state of the world is ω_1. We have just seen that at ω_1 Bob does believe that $v \leq 3$. At this point, interactive beliefs enters the scene and we can ask: at ω_1, does Ann believe that Bob believes that $v \leq 3$?

From an intuitive viewpoint, the answer is that it depends on what Ann believes at ω_1 about the way information is imparted to Bob. Since in the state-space model the way information is imparted to an agent is formally represented by his possibility correspondence P, one may be tempted to formulate the same answer by saying that whether at ω_1 Ann believes that Bob believes that $v \leq 3$ depends on whether at ω_1 Ann knows Bob's possibility correspondence P_B. However, this formulation is misleading because it mixes up the viewpoint of the modeler with that of the agents in the model. As observed in Section 2, possibility correspondences are in fact just a tool that the modeler employs to encode and represent the agents' beliefs and knowledge, not something that the agents need to be aware of or even know. Therefore, possibility correspondences exist for the model-maker but need not exist for the agents in the model, and thus reasoning as if he agents may or may not know them is confusing.

At any rate, let us return to the intuitive answer to the question: "At ω_1, does Ann believe that Bob believes that $v \leq 3$?". In Section 2 we saw that at ω_1 Ann believes that the true state is ω_1 or ω_2. If Ann believes that in ω_1 and ω_2 Bob regards as possible both $v = 1$ and $v = 2$ (as is, in fact, the case for Bob), then in ω_1 Ann can conclude that Bob believes that $v \leq 3$. By contrast, if Ann has no clue about the way information is imparted to Bob in ω_1 and ω_2 then she has no idea about what Bob believes in these two states and so cannot conclude that Bob believes that $v \leq 3$. In other words, the intuitive answer to the above question turns out to be that we need to make some additional assumption about Ann's information about Bob's information in order to answer the question about Ann's beliefs about Bob's beliefs.

The formal answer offered by the state-space model is however different: since in ω_1 Ann believes S, i.e., $\omega_1 \in B_A(S)$, and the set of states where Bob believes U coincides with S, i.e., $S = B_B(U)$, then by the substitutivity principle $\omega_1 \in B_A(B_B(U))$. In the state-space model the latter expressions means that in ω_1 Ann believes that Bob believes that $v \leq 3$. And this conclusion is independent of any additional assumption as to Ann's information about the way information is imparted to Bob. One may add that in our Ann-Bob example, in ω_1 and ω_2 both Ann's and Bob's beliefs are true, so that in ω_1 Ann not only believes but also knows that Bob knows that $v \leq 3$.

This puzzling result is again due to the fact that the operators B and K are unable to distinguish between intension and extension. In fact, although intensionally distinct, in our model the events "$v \leq 2$" and "Bob believes that $v \leq 3$" are extensionally equal. Therefore B and K automatically implement the substitutivity principle even if, as in this case, it is unwarranted.

Aumann has always claimed that, if the state-space Ω is defined in an appropriate way, each agent knows by construction how the information is imparted to other agents, and that this knowledge "is not an assumption, but a 'theorem', a tautology; it is implicit in the model itself" (Aumann 1987, 9).[11] Here I argue that the fundamental reason for this counterintuitive feature of the state-space formalism when applied to interactive beliefs and knowledge lies in its extensional nature, which prevents the model from telling apart events that are extensionally equal but intensionally distinct.

This feature of B and K may preclude the state-space model from capturing the uncertainty that an agent has about the way the information is imparted to other agents, although that uncertainty is a

[11] For a discussion of Aumann's argument and its possible limitations, see Aumann 1999; Heifetz and Samet 1998; Heifetz 1999; Fagin, et al. 1999; Aumann and Heifetz 2002; Cubitt and Sugden 2003.

key element of the situation at issue and crucially affects the agents' behavior.

For instance, we can re-interpret our Ann-Bob model as representing a duopoly game, whereby the two agents are the duopolists, v is the level of next period's market demand, and lower levels of v correspond to lower expected demand levels. At ω_1, Ann receives a signal that in the next period the market demand will be quite low ($v = 1$ or $v = 2$) and has to decide how much to produce. If Ann considers it possible that Bob erroneously believes that the demand will be very high ($v = 4$), she is afraid that Bob could produce a large output, and therefore she might cautiously choose an extremely low output level. This behavior and its motivations cannot be captured by our state-space model because, as shown above, in that model Ann knows and cannot avoid knowing that Bob knows that $v \leq 3$, and thus that he rules out a very high demand.

One may easily imagine many other economically relevant situations in which the failure to capture the agents' uncertainty about the information that other agents may have through the operators B and K renders the use of the state-space model problematic.

5. POSSIBLE SOLUTIONS

Besides its failure to capture the intensional dimension of linguistic expressions, the state-space model displays other implausible features: it implies the monotonicity of beliefs and knowledge and precludes agents' unawareness. Since some solutions to these latter limitations have been proposed, one may wonder whether there are ways to modify the operators B and K and/or the state-space model in such a way that they become capable of capturing the difference between events that are extensionally equivalent but intensionally distinct. Here I discuss three possible ways in which one may imagine that this could be done.

First, the state-space model can make room for unawareness and block monotonicity by introducing a further set-theoretic operator $A : \Omega \to 2^{2^\Omega}$, that associates each state of world ω with a collection of subsets of Ω. A can be interpreted as an awareness correspondence that associates each state ω with the events that the agent is able to figure out in ω.[12] Based on the awareness operator A, one may define a modified belief operator B' as follows:

$$B'(E) = \{\omega \in \Omega : P(\omega) \subseteq E \wedge E \in A(\omega)\}.$$

The interpretation of B' is that believing an event requires not only that the event occurs in every state the agent regards as possible, i.e., $P(\omega) \subseteq E$, but also that the agent can figure out the event, i.e. $E \in A(\omega)$. For instance, if at ω_1 Ann is not able to figure out the meaning of "12th root of 4096", then although in all states she regards as possible at ω_1 the event T "$v \leq \sqrt[12]{4096}$" occurs, she does not believe it.

The operator A can also be used to block monotonicity. Monotonicity states that if event E implies event F—a situation that in set-theoretical terms is expressed by $E \subseteq F$—then believing event E implies believing its implications F, i.e., $B(E) \subseteq B(F)$.[13] Operator A can block monotonicity as follows: if $E \subseteq F$ and $E \in A(\omega)$, but $F \notin A(\omega)$, then $B'(E)$ is not a subset of $B'(F)$. In words, if event E implies event F and the agent is aware of E but unaware of F, then her believing E does not imply that she also believes the implications of E.

Is it also possible to use the operator A to block the automatic implementation of the substitutivity principle by the operators B and K, and thus account for the possible intensional difference of extensionally equivalent events? The answer appears to be in the negative, because A too is extensional in nature and therefore cannot distinguish between extensionally equivalent sets: if $E = F$, $E \in A(\omega)$ if and only if $F \in A(\omega)$.

In our example, the extensional nature of operator A has the problematic implication that Ann is able to figure out the event S "$v \leq 2$" if and only if she is able to figure out the extensionally equivalent event T "$v \leq \sqrt[12]{4096}$". However, one may well imagine that Ann is aware of "$v \leq 2$" while ignoring "$v \leq \sqrt[12]{4096}$". Since the operator A cannot distinguish between extensionally equivalent sets, not even

[12] The definition of the awareness operator proposed here follows Fagin, et al. 1995; for other ways of modeling unawareness, see Modica and Rustichini 1999; Halpern 2001; Heifetz, et al. 2006; Li, 2009.

[13] The property of operators B and K discussed in this paper, namely that if $E = F$ then $B(E) = B(F)$ and $K(E) = K(F)$, is strictly weaker than monotonicity. The latter property reduces to the former in the particular case when $E = F$.

the modified belief operator B' based on A can distinguish between them: if $E = F$, then $B'(E) = B'(F)$. The same holds for the modified knowledge operator K' that can be built upon operator B' in the predictable way.

The discussion concerning the operators A, B', and K' suggests that attempts to capture the intensional difference between extensionally equivalent events by introducing into the state-space model some new set-theoretic operator like A are bound to fail. In fact, any operator of this kind would map extensionally equal inputs into the same output, and so would be unable to overcome the problem.

A second possible way out of the problem consists of re-defining the set of the states of the world Ω, not as the external and omniscient model-maker sees it, but as each agent subjectively views Ω. From this point of view, the problem does not lie in the set-theoretic nature of the operators B and K, but in a specification of Ω that is not sufficiently fine-grained to capture all the relevant aspects of the situation.

For instance, if for Ann $v = 2$ and $v \leq \sqrt[12]{4096}$ are two distinct events, then her subjective state-space Ω_A is finer-grained than the model-maker's state-space Ω and includes, besides the state ω_2 where $v = 2$, also a state ω_5 where $v \leq \sqrt[12]{4096}$: $\Omega_A = \{\omega_1, \omega_2, \omega_3, \omega_4, \omega_5\}$. At ω_2 and ω_5, Ann may regard as possible different states of the world. For instance, it could be that $P_A(\omega_2) = \{\omega_1, \omega_2\}$ and $P_A(\omega_5) = \{\omega_5\}$, so that at ω_2 Ann believes that $v \leq 2$ without believing that $v \leq \sqrt[12]{4096}$.

I see two problems in the modeling strategy based on the subjective re-definition of Ω. First, defining the subjective state-space of each agent in the model requires that the modeler knows how each agent sees the world and the intensional meaning the agent gives to different expressions. This appears more demanding than defining the state-space as it is "objectively", or at least, as it is from the modeler's view. For instance, in our over-simplified numerical example, besides $v \leq \sqrt[12]{4096}$ there are countless other alternative intensions for $v = 2$, such as $v = 5 - 3$; $v = (5^2 - 3^2)/2^3$; $v = \sqrt[13]{8192}$, $v = 2\cos(2\pi)$, $v = 5!/(3^3 + 33)$. Which of these expressions are understood by Ann as equivalent to $v = 2$, and thus could be identified with the state ω_2? And which ones, instead, require the addition of further states of the world to Ann's subjective state-space Ω_A?

Second, fine-grained subjective state-spaces quickly become very complex and thus could easily make the state-space model difficult to handle for the purposes of economic analysis. For instance, we saw that when $v = 1$ Ann knows that $v \leq 2$. Imagine now that, when $v = 1$, Ann has little clue about Bob's information and regards as possible four situations: i) Bob knows exactly whether $v = 1$ or $v = 2$; ii) like herself, Bob only knows that $v \leq 2$; iii) Bob is less informed than herself and can only rule out that $v = 4$; iv) Bob erroneously believes that $v > 2$.

To model this situation we can split the state ω_1 into four states ω_1^i, ω_1^{ii}, ω_1^{iii}, ω_1^{iv}, each of which specifies the value of v as well as Bob's information about the value of v. Thus, in ω_1^i, $v = 1$ and Bob knows whether $v = 1$ or $v = 2$; in ω_1^{ii}, $v = 1$ and Bob knows that $v \leq 2$; in ω_1^{iii}, $v = 1$ and Bob knows that $v \neq 4$; in ω_1^{iv}, $v = 1$ and Bob believes that $v > 2$. If Ann is uncertain about Bob's information, she is unable to distinguish between ω_1^i, ω_1^{ii}, ω_1^{iii}, and ω_1^{iv}, and whichever state is the true one she regards all four of them as possible:

$$P_A(\omega_1^i) = P_A(\omega_1^{ii}) = P_A(\omega_1^{iii}) = P_A(\omega_1^{iv}) = \{\omega_1^i, \omega_1^{ii}, \omega_1^{iii}, \omega_1^{iv}\}.$$

By modeling Ann's beliefs and uncertainty this way, we can avoid the puzzling result that when $v = 1$, Ann not only knows that $v \leq 2$ but also that Bob knows that $v \leq 3$. In fact, when $v = 1$ Ann cannot rule out ω_1^{iv}, at which state, however, Bob believes that $v > 2$.

The cost of this modeling strategy is that we have to add three states of the world to the "objective" state-space just to account for Ann's uncertainty about Bob's information about the value of v, in the particular case when $v = 1$. If we also want to account for Ann's possible uncertainty about Bob's information about the value of v when $v = 2$, $v = 3$, or $v = 4$, we would have to add further states to Ann's state-space Ω_A.

We may also want to consider the first level of interactive beliefs and uncertainty, and model Ann's uncertainty about what Bob believes about her information about the value of v. This would require adding many more elements to Ω_A. Modeling higher levels of interactive beliefs and uncertainty would require adding even more states to Ω_A. And, if all this were not enough, we might also want to model Bob's subjective state-space Ω_B, his uncertainty about Ann's information about the value of v, his uncertainty about what Ann believes about his information about the value of v, and so forth. In brief, properly fine-grained subjective state-spaces easily become intractable objects that as such may be of little use in economic theorizing.[14]

A third possible way to capture the intensional difference between extensionally equivalent events is to abandon the state-space and the set-theoretic modeling of beliefs and knowledge, and adopt the so-called logic-based or syntactic approach to beliefs and knowledge that is prevalent among logicians and philosophers. The building blocks of this approach are *formulas*, which correspond to events and are expressed by alphabetical letters, and epistemic operators such as b (for belief) or k (for knowledge) that associate formulas with other formulas according to certain axioms that can be stated in an explicit way.[15]

The fundamental difference between the operators B and K of the state-space model and the operators b and k of the syntactic approach is that, *in principle*, the latter can be axiomatized so as to avoid the automatic implementation of the substitutivity principle. More specifically, one can design a syntactic system in which, although two formulas e and f coincide, even in every imaginable universe, this does not imply that when individual i believes or knows one of them she must also believe or know the other.

Formally, although $e \leftrightarrow f$ and $b_i e$, it can be the case that $\neg b_i f$ ($\leftrightarrow$ and $\neg$ are, respectively, the logical symbols for "necessary and sufficient condition", and "negation"). However, *de facto*, even those economists who have employed the syntactic approach have maintained the substitutivity principle, that is, they have still posited that if $e \leftrightarrow f$ and $b_i e$, then $b_i f$ (see, e.g., Lismont and Mongin 1994a, 1994b, 1995, 2003; Ferrante 1996; Dardi 2004; Heifetz, et al. 2008).

In part, the maintenance of the substitutivity principle is due to the fact that economists have adopted the syntactic approach in order to overcome other implausible features of the state-space model, specifically, that it implies the monotonicity of beliefs and knowledge or that it precludes unawareness, rather than its treating as equal expressions that are intensionally distinct. But another reason why economists have maintained the substitutivity principle is that not even

[14] Actually there are some situations in which no number of splits of the states of the world is sufficiently large to exhaust all interactive uncertainty of the agents, that is, to properly fine-grain the model. See Hart, et al. 1996; Heifetz and Samet 1998; Heifetz 1999; Fagin, et al. 1999; Aumann and Heifetz 2002.

[15] For an introduction to the syntactic approach, see Fagin, et. al. 1995; Aumann 1999.

logicians and philosophers agree as to how it might be given up. As mentioned in Section 1, while various systems of intensional logic aimed at capturing explicitly the intension-extension difference have been put forward in the last sixty-five years, none of them has ever gained general acceptance.

From a sociological viewpoint, we may add that the syntactic approach requires a familiarity with the language, modeling techniques, and key results of epistemic logic, none of which form part of the typical background of economists. The significant fixed costs associated with acquiring that familiarity discourage economists from using the syntactic approach.

If we put together the difficulty of giving up the substitutivity principle, even in the syntactic approach, with economists' lack of familiarity with epistemic logic, it appears less clear that adopting the syntactic approach is the best move to overcome the difficulties of the state-space model in accounting for extensions with different intensions.

To sum up the discussion: attempts to capture the intensional difference between extensionally equivalent events by introducing into the state-space model some new set-theoretic operator like A appear bound to fail; the other two routes—a subjective re-definition of Ω and the move to the syntactic approach—are more promising, but each of them contains significant drawbacks.

6. CONCLUSIONS

I have attempted to draw the attention of economists to a cluster of problems that have proved important to philosophers, at least since the end of the nineteenth century, and are related to the relationship between the intension and the extension of a linguistic expression. In particular, philosophers have long understood that such a relationship becomes opaque in a number of contexts, particularly those involving belief and knowledge. In the paper I have shown that the problems related to the intension and extension of an expression are relevant also for economics, and particularly for the model of belief and knowledge prevailing in this discipline, namely the state-space model introduced by Aumann in 1976.

In particular, I have argued that, because of its set-theoretic nature, the state-space model tends to miss the difference between expressions that have the same extension but distinct intensions. This feature of the model generates a number of puzzling results that concern not only what individuals believe or know about the world, but also interactive beliefs and knowledge. I have also attempted to connect these puzzling results with two issues that are relevant for economic theory even beyond the state-space model, namely framing effects and the distinction between model-maker's and agents' viewpoints in the model.

The limitations of the state-space model do not imply that this model should be abandoned. Indeed, I have argued that the two alternatives that appear practicable present significant drawbacks: a properly fine-grained subjective state-space Ω easily becomes intractable, while dismissing the substitutivity principle is *de facto* tricky even in the syntactic approach. At the present moment, I cannot envisage any general method to assess the trade-off between the advantages and drawbacks of these alternatives.

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On ceteris paribus laws in economics (and elsewhere): why do social sciences matter to each other?

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Abstract: Stipulating universal propositions with a ceteris paribus clause is normal practice in science and especially in economics. Yet there are several problems associated with the use of ceteris paribus clauses in theorising and in policy matters. This paper first investigates three questions: how can ceteris paribus clauses be non-vacuous? How can ceteris paribus laws be true? And how can they help in formulating successful policy interventions in a diversity of contexts? It turns out that ceteris paribus clauses are not always used legitimately. They are meant to fence *off* a theory from disturbing factors, but economists who do not specify the clause well enough tend to fence variables *in* rather than *off*. In such cases, it would be better to use theoretical abstraction, which is something very different from the use of ceteris paribus clauses. However, abstract theorising conceptually leads one away from the concrete detail of real world situations in which policies take place. Hence, a fourth question arises: how can policy interventions be properly designed on the basis of abstract laws? To answer this question, I defend interdisciplinarity in concept choice.

Keywords: ceteris paribus, abstraction, concretisation, logical strength, interdisciplinarity, socio-economic policy

JEL Classification: A12, B41, Z18

In many sciences it is necessary to model the workings of causally related phenomena under the proviso that other variables than the ones under investigation are constant, absent, or negligible. Scientific explanation comes with the use of ceteris paribus laws: lawlike generalisations hedged with clauses that specify under what conditions the generalisation can be expected to be applicable. In the literature

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there are roughly three approaches to the meaning and role of ceteris paribus laws. Either the ceteris paribus clause is seen as merely specifying the set of conditions under which the lawlike claim is true; or the clause specifies mere normality conditions; or the ceteris paribus laws describe the capacity (or disposition) of a system to behave in a certain way without the certainty that it will actually do so (it depends on the clause whether it will).

Without opposing any of these approaches, this paper defends five claims. First, what makes ceteris paribus claims interesting in general is the quest for truth when the ceteris paribus proviso is false. Ceteris paribus reasoning strategies only have to 'keep other things equal' when these things do not remain the same—otherwise there is no point in inserting such a proviso. This means that some form of falsity is involved when doing science and this is inherently interesting, especially for realists.

My second claim is that a ceteris paribus generalisation is always subjunctive, not indicative. Ceteris paribus laws are best seen as counterfactuals. This claim must be distinguished from the well known understanding that laws differ from accidents in that they sustain counterfactuals, as explored in a vast body of literature. The familiar idea is that laws have nomological necessity. The most recent work in this area is by Marc Lange (2005, 2009). The difference between my claims and Lange's is that my worry focuses on the scepticism in thinking that lawlike generalisations mean nothing in economics due to its ubiquitous use of ceteris paribus clauses. Both the metaphysics and the semantics of ceteris paribus clauses are poorly understood by many economists.

The third claim is about *the use* of ceteris paribus-hedged claims. In policy issues, it turns out that any ceteris paribus assertion requires a very strict resilience of the environment. The slightest difference in the distribution of helping factors in the way a policy intervention can be successful in one context tends to make a similar policy in another context unsuccessful. So the point is not that 'other things' abstain from behaving abnormally, but that they remain as stable as needed for the external validity of a policy evaluation.[1]

The fourth claim is that successful predictions enabling policy interventions in varying contexts require abstraction; not the recourse to

[1] I am grateful to an anonymous referee for noting that 'other things equal' often merely means that they do not behave in weird ways.

the sort of hedging for which ceteris paribus clauses have been designed. A genuine ceteris paribus clause defines a policy relevant problem just as concretely as the description of the disturbing variables demands. So I propose that abstraction from this concrete set of situational details is needed to make policies work, but I also warn that the description of a situation may overshoot the required level of abstraction, making it useless.

The fifth claim is that we need the interdisciplinarity of social scientific work for policy purposes. Interventions necessitate the conceptual approach provided by other (social) sciences in order to reverse the abstractive process toward concrete world where the interventions actually take place.

If a clause conditions a generalisation (lawlike or not), two issues arise. One concerns truth and the other concerns vacuity. As to the first, if the condition is false, the conditional proposition will always be true—which in turn leads to a form of (*alethic*) vacuity. In addition, realists like to think of economics as an enterprise that seeks truth: the habitual falsity of the ceteris paribus clause seems to be in conflict with the realist approach to a philosophy of economics. As to the second issue, in the philosophy of economics there has been a worry that generalisations fail to be empirical for the ubiquity of ceteris paribus clauses. The source of the worry is that if such clauses severely delimit the range of cases in which the generalisation can be expected to show itself, it will withstand testing. The claims of economics cease to be scientific.[2] In this paper I sharply distinguish between issues of truth and of vacuity.

Given the ubiquitous use of ceteris paribus clauses, however, it seems that the assumption that ceteris paribus laws are respectable parts of scientific theories is a good starting point of any normative theory of science.[3] Paul Pietrosky and Georges Rey (1995) took this as

[2] De Marchi has vividly described how economists of the London School of Economics tried to conform their theorising to the demands of the Popperian demarcation criterion, see De Marchi 1988. This is also one of the sources of Uskali Mäki's worry that the opinions polarise when it comes to the status of economics, see Mäki 2002.

[3] Already here I use two different senses of 'theory', one in object language and one in meta-language. In this paper I do not presume to have a precise concept of what a theory is—which concerns contested area. The point of the present paper neither depends on the outcome of this debate, nor does the use of the term reveal any well thought position about, for example, the semantic view of theories or the syntactic view, and so on. It suffices to view a theory as a set of hypotheses about lawlike relationships and a test hypothesis as deductively inferred from the theory under test. Of course, this approach loosely conforms to the Hempelian view of science.

their starting point when they asked how the use of ceteris paribus laws could be non-vacuous. Meanwhile, of all scientists, economists turn out to be the wholesalers of ceteris paribus clauses. The question, then, is to what extent this abundant recourse to hedged laws can be defended specifically in economics.[4]

In this paper I argue that ceteris paribus clauses as part of lawlike statements can be defended to a very large extent. However, two things should be well considered. First, economists themselves tend to have a blurred view of what economic theories buttressed with ceteris paribus clauses precisely amount to. They often coin this 'abstraction'. Second, the question how ceteris paribus laws can be *non-vacuous*—Pietrosky and Rey's research question—must be well distinguished from the question how ceteris paribus laws can be *true*. The importance of the latter question comes to the fore as soon as it is realised that the very nature of ceteris paribus caveats stem from the trouble that other circumstances do *not* remain unaltered. The semantic question is this: how can deliberate insertion of falsity be functional in the quest for truth? This paper answers both questions.

To do this, I distinguish four different cases. In the first case, ceteris paribus clauses may be purely ad hoc. This is a clear case of vacuity. A ceteris paribus law is vacuous because theorists who make use of ad-hoc clauses can offer false explanations for phenomena without ever having to revise their theory in light of counterevidence. This idea is the starting point of section 1 where a distinction is observed between two types of vacuity: vacuity *simpliciter* and trivial truth of the ceteris paribus clause. In the second case, explained in section 2, ceteris paribus laws may be non-vacuous: if the clause represents a finite list of possible disturbances. The third case occurs when the clause aims to hedge a lawlike statement from a change in variables induced by influences from within the system under study. We will see an example in section 3. Now the ceteris paribus law is inconsistent due to the fact that the items constitutive of the clause are not external

[4] Like theories, laws bring us in contested area. In my view lawlike statements merely describe causal connections that inform us that similar cases as the one under study may occur in the future. Among natural scientists it is normal to speak of (natural) laws as being more fundamental than any causal claims in the special sciences. Quite apart from the as yet unsettled question whether physicists really discover (fundamental) laws, I doubt that there are any such laws in social science. The answer to this question is however of no consequence for the present paper. All I need is that it makes sense to more modestly speak of 'lawlikeness' in social science. When I use the term 'law' as well as when I use 'lawlike statement', I refer to nothing else.

to the thing that falls under the description of the very lawlike statement that was to be hedged. The fourth case again allows the ceteris paribus law to be non-vacuous, but now along different lines. Pietrosky and Rey have proposed that if violations of the clause can be explained by a theory independently from the law in question, there is no case of vacuity. In section 4, I will use their approach to argue that interdisciplinarity matters in a non-trivial way.

From this point I move, in section 5, toward very different but related epistemic operations in economics: abstraction and concretisation. We will see that Pietrosky and Rey's condition seems to deal with abstraction rather than with the use of ceteris paribus clauses. Issues of the truth value of a lawlike proposition resting on a hedging strategy drive a familiar debate: about how false clauses can make such laws true. Section 6 provides an answer that I think is not often heard: ceteris paribus laws are counterfactuals. Next, section 7 discusses how abstraction relates to the quest for truth. Finally, armoured with these concepts—abstraction, concretisation, and counterfactuals—in section 8, I will defend interdisciplinarity of social research from a new point of view.

Throughout the paper, I assume without discussion that the differences between using ceteris paribus clauses and theoretical abstraction apply to science in general. However, when it comes to policy, I focus on social science specifically. In special cases, such as when considering the sloppy distinctions often made between various isolation techniques, I take economics as a paradigm case.

1. TWO SENSES OF THE VACUITY OF CETERIS PARIBUS LAWS

Why would anyone want to dismiss the use of ceteris paribus clauses if it is ubiquitous in scientific practice? Because theories that aim to explain the workings of the world must be subjected to test and any hypotheses furnished with caveats escape falsification. Such hypotheses are empirically empty. This view is puzzling. Do we dismiss all economic science as empirically empty?

Ceteris paribus *clauses* express the requirement that circumstances, external to the ones that are subject to the explanatory theory, are stable; only the variables under investigation are supposed to change. When any one of the circumstances to which the clause refers does change, the test itself fails, not the hypothesis under test. Ceteris paribus clauses immunise hypotheses—and with them the theories to which they belong—from falsification. Daniel Hausman has phrased this as ‘ceteris paribus, everything that is F is G’. F-things that turn out not to be G do not contradict this claim, however, if the *cetera* (that we can blame are not *paria*) have been chosen in a non ad hoc way (see Hausman 1992, 139-142).

Vacuity simpliciter and falsity as two types of vacuity

Demand responds negatively to changes in the price of the good demanded, except when it fails to do this (e.g., because incomes change). Interest rates rise when capital turns scarcer, except when they happen to drop, or do nothing (because a slump crushes investors’ appetite). Complaints about economic predictions are now that they are wrong, then that they are *inherently* untestable.[5] But is it not equally true that physicists are happy to talk of planets that move in ellipses around the sun except when they do not (because gravitational forces of other planets deform the ellipse)?

We need a good story to explain this. The trouble is not merely that we hope theories to be rigorous or strong. If ceteris paribus laws are immune to counterexamples they are also meaningless. There is little point in saying that ‘every F-thing has the property G except when this is not the case’. An entire canonical literature tries to find ways to circumvent the vacuity.[6] As Pietrosky and Rey put it, clauses are vacuous unless it is specified when the *cetera* are *paria*, and such a specification is hard to come by. They even speak of a panic that they want to quell (Pietrosky and Rey 1995, 82). And their answer is at least partly convincing: very loosely said, a scientist has to make sure that the ceteris paribus clause cites factors that can be explained with theories independent from the theory the putative ceteris paribus law has been derived from.[7]

I believe it is important to distinguish problems of mere vacuity due to the inherent resistance to testing from problems of truth. While ceteris paribus laws run the apparent risk of developing into immunisation strategies, a separate matter is that they must have truth

[5] Note that this is not the same as resistance to testing due to inferential, technical, practical, or ethical obstacles.

[6] The list of authors who more or less implicitly wrote about ceteris paribus seems endless. Apart from Hausman and the above mentioned Pietrosky and Rey, other authors have explicitly been dealing with ceteris paribus. See, e.g., Cartwright 2001; Mäki 1994; Niiniluoto 2002; and Nowak 1989.

[7] They elaborate on it much more rigorously and precisely. Section 4 below will do much more justice to their proposal.

value. Note that a common approach to ceteris paribus laws is to formalize them into material implications ('ceteris paribus, if it is an F, then it is a G') and these are the weakest kind of implications in logic. Clearly, it only requires falsity of the antecedent to make the entire conditional true. Integration of the clause into the antecedent is enough to show that, if (and only if) formalised in this way, all ceteris paribus laws are trivially true. So in sum, we have two issues at stake here: non-falsifiability and trivial truth. One is the issue of vacuity connected to immunisation; the other is lack of degrees of freedom of the truth function. I will label the first 'vacuity type 1' and the second 'vacuity type 2'.

There is a second argument for addressing the issue of truth. Social (or economic) theory has the obvious pretention to be useful for social (economic) policy. As I have set out to explain elsewhere (Rol and Cartwright 2012), without *true* lawlike claims inferred from explanatory social theories, the use of policy recommendations cannot be warranted either.

Pietrosky and Rey's quest concern vacuity type 1. Their starting point is, like mine, that ceteris paribus laws can be respectable parts of scientific theories in general. My other additional presumptions are that for economics such laws are equally acceptable even though it has been thought dubious that economic theory seems to be swamped in them; and that the related problem of vacuity type 2 deserves treatment independently of the problem of vacuity type 1. The answer to both questions—how ceteris paribus laws can be non-vacuous and how they can be true—leads to the same conclusion.

2. TYPE 1 VACUITY: HOW TO FENCE *OFF* THE EXPLANANDUM

A ceteris paribus law is a lawlike statement hedged with a clause. This clause reports exceptions to the lawlike statement. One obvious way to warrant a ceteris paribus law to be non-vacuous is by making the clause manageable, i.e., finite. This describes the second possibility listed in the introduction. If the finite set of possible exceptions to a lawlike statement is put forward *before* the test, both the phenomena and the test hypothesis can be saved. All that has to be proved is that one of the proposed exceptions applies. So both a judgement is needed about what exceptions are permissible and a guarantee is needed that this judgement *precedes* actual testing.

Finite ceteris paribus clauses and nomological machines

For instance, all first year economics textbooks sum up three possible exceptions to the rule that demand negatively correlates with price (let us call this 'the law of demand'): permissible exceptions apply when (disposable) incomes of demanders change, when prices of substitutes or complements alter, and when preferences vary. The ceteris paribus clause says that these possible changes are not to occur for the law to be strictly true, but the very reason why anyone would use a clause like this is that these changes *do* occur. If they did not, there would be little point in constructing the clause.

So testing the law happens under the caveat that it is *not* strictly true, because the clause may be violated. There may be no correlation between price and demand or after a price increase rising demand may even follow, reversing the law of demand. If the agents whose behaviour is investigated enjoyed a substantial pay increase after which they raised demand for almost everything, they will also increase demand for the good under investigation even when it turned more expensive. In the modelling practice we say that the demand line has shifted to the right. Only if it can be shown that incomes did in fact rise (or that either of the other two items on the list did not remain the same) we continue to accept the lawlike statement as true. Thus, the law of demand is saved even in an unsteady world. So in sum, falsifying instances must first be confronted with the ceteris paribus clause. In order to do so, this clause must be finite. If this is the case—provided the appropriate procedures are in place—a ceteris paribus law is not vacuous.

John Maynard Keynes noted in *The general theory of employment, interest and money* how important a well specified (i.e., finite) ceteris paribus clause is. After an exposition of how the relation between the 'rate of consumption' and aggregate income changes due to changes in the marginal rate of consumption—a possibility the classical economists had disregarded—he warns for the 'extreme complexity' of the model:

> […] these seem to be the factors which it is useful and convenient to isolate. If we examine any actual problem along the lines of the above schematism, we shall find it more manageable; and our practical intuition (which can take account of a more detailed complex of facts than can be treated on general principles) will be offered a less intractable material upon which to work (Keynes 1973 [1936], 249).

The classical model, according to Keynes, implicitly presumes the marginal propensities to consume and to save as constant. The possibility of changes in this 'rate of consumption' made possible by for instance the banking system or simply by the degrees of freedom that exist in the allocation of the budget went unnoticed by the classical economists. However, it seems 'to be useful and convenient to isolate' this aspect of economic life, as long as you realise that such a procedure involves a deliberate isolation. A finite list makes the subject matter concrete, because every interfering factor is accounted for.

The role of well specified clauses becomes clear if we consider laboratory practice. In the natural sciences, both explanatory research as well as the testing of hypotheses takes place in an engineered setting more often than is possible in social science.[8] The law under investigation is never investigated 'in the wild'; many provisions have to be made. All sorts of helping factors—clean glasswork in chemistry, well specified rules for subjects in behavioural economics—must be in place. Also, interfering factors preventing the phenomenon searched have to be absent, like friction. The design of a laboratory setting implies the subjection of reality to the will of the researcher. It is a model world with stable (enough) preconditions. In a fall experiment the conditions of the ceteris paribus clause can be mimicked by a near vacuum. Thus, the feather will drop with the same acceleration as a pebble. A test of the law of free fall requires that a model reality is created in which the ceteris paribus clause is *made true*.[9] In other words, *a laboratory is a world where the ceteris paribus clause applies*; it is a 'nomological machine', as it has been dubbed by Nancy Cartwright (see, e.g., Cartwright 2001).[10]

Note that ceteris paribus clauses do not alter the level of abstraction of the laws that they hedge. This is what brought Uskali Mäki to introduce the term 'horizontal isolation'.[11] I have often noticed—in

[8] Of the behavioural sciences, social psychologists make use of a kind of laboratory most frequently. For a nice controlled experiment, see Alter, et al. 2007.

[9] Even though a perfect vacuum cannot physically be created; but natural scientists are content with an approximate approach.

[10] Nancy Cartwright stresses that any phenomenon, triggered by human intervention or natural, is in fact the nomological machine at work. Her point is that (fundamental) laws never work in isolation of helping factors, due to which no laws of physics describe reality; they 'lie', see Cartwright 1983.

[11] Uskali Mäki's idea is that summing up details to be excluded at *the same level of abstraction* amounts to isolating sideways, or horizontally, one set of details from another. Abstraction would then be vertical in the sense that we derive concepts without reference to the detail of actual economic processes. See, e.g., Mäki 1994.

personal communication with economists—that they tend to forget this important point: abstracting from the world requires a choice of some properties out of many and labelling these as important for further scrutiny and theorising; in contrast, hedging requires reference to all properties, *especially to those that you do not want to study*.

Incomes, preferences and prices of competing goods must all remain stable for the law of demand to express itself in the data, so only the relationship between price and quantity demanded seems to matter. But the formulation of the clause reveals that we have to constantly focus on the details in it to make things happen, instead of on the price-demand relationship itself. A ceteris paribus clause anchors in the concrete world. But theorising involves a movement away from this world. It brings the economist from my erratic demand behaviour toward abstract concepts like 'demand', 'productivity', and 'inflation'. In consequence, the epistemic role of abstraction is very different from that of a ceteris paribus clause.

3. SYSTEM EFFECTS FEEDING BACK: HOW TO FENCE *IN* THE EXPLANANDUM

What if a price drop increases the budget of demanders, causing an increased appetite for a substitute, rather than for the cheaper good? This may happen even with stable independent preferences and stable income earnings—that is, even when none of the *cetera* stipulated in the clause is violated. Now demand *drops* as a response to a price *decrease*. We know that another lawlike statement can provide for an explanation here, about differences in the rate of diminishing marginal demand with rising disposable income.[12] Here we have a case of the well known Giffen goods, also known as goods with perverse demand behaviour. It is important to note that the budget is raised by the very price drop itself, not by external influences. System effects of the working of the law feed back. As the ceteris paribus clause aims to merely fence off *external* influences, that is, external to the particular causal connection described by the lawlike statement, the conclusion must be that we cannot refer to the expansion of the budget due to rising purchasing power in a ceteris paribus clause in a case of Giffen goods. Counter the more traditional microeconomic approach, I claim that ceteris paribus caveats do not apply to a change in variables induced by the very

Below I do however not follow his nomenclature entirely. Elsewhere I use 'idealisation' for horizontal isolation, see Rol 2008.

[12] This famously is Gossen's law.

lawlike behaviour under study, because such an admonition would be inconsistent.

Another example of this case would be the famous Lucas-critique: changes in parameters of a model cannot coherently be fenced off if the model also assumes that people are capable of learning. Once again a model uses an assumption that feeds back into the system, whereby isolation of its effect turns inherently contradictory.

Keynes, quoted above to show the importance of a finalisation of the clause, now shows us the importance of system feedback effects:

> Now, in so far as the classical theory assumes that real wages are always equal to the marginal disutility of labour and that the latter increases when employment increases, so that the labour supply will fall off, *cet. par.*, if real wages are reduced, it is assuming that in practice it is impossible to increase expenditure in terms of wage-unit. If this were true, the concept of elasticity of employment[13] would have no field of application. Moreover, it would in this event, be impossible to increase employment by increasing expenditure in terms of money; for money-wages would rise proportionally to the increased money expenditure so that there would be no increase of expenditure in terms of wage-units and consequently no increase in employment. But if the classical assumption does not hold good,[14] it will be possible to increase employment by increasing expenditure in terms of money until real wages have fallen to equality with the marginal disutility of labour, at which point there will, by definition, be full employment (Keynes 1973 [1936], 284).

The trouble Keynes draws our attention to in this quote is that the classical economists presume equilibrium as a result of the confrontation of labour demand and labour supply, but disregard that such an equilibrium must be reached dynamically from any position of disequilibrium. For ‘elasticity of employment’ to have any meaning at all, we must all but presume that demand for goods can change even when real wages do not, for instance by increased lending. After all, for a rise in employment due to increasing demand, someone must start demanding more. But the classical economists end up analysing such an increased appetite as having no effect because money

[13] In *The general theory* the elasticity of employment is the relative change of employment opportunities resulting from a relative change in demand for industrial produce.

[14] Keynes refers to what he calls ‘the second fundamental postulate’ of the classical theory of employment. It is an equilibrium condition, which says that the real wage is equal to the marginal disutility of labour for the employee.

wages (as opposed to real wages) will fall. All of these factors, such as changing preferences and propensities to consume and save, are kept in an ideal stable position.

Keynes's *The general theory* basically warns that some *cetera* cannot be fenced off in a clause without violation of the implicit assumptions of the very theory that puts the ceteris paribus clause in place. In other words, sometimes the ceteris paribus clause is incoherent and the theory of employment as proposed by the classical economists, Keynes tells us, is a case in point. My claim, then, is that Keynes's point against the microeconomics of his time is similar to the Giffen goods example in the relevant respect. It is fine that the ceteris paribus clause sometimes fences off variables but not when these variables form essential parts of the system studied.

An insufficiently well developed insight into what a model of a system under study entails leads to incoherence. The ceteris paribus clause must not be inherently denied by the other assumptions of the theory. Therefore, a theory about a small system can bear a more extensive ceteris paribus clause than one about the same phenomena but studied as a larger system (including more phenomena). To push things further, a consistent dreamed-of 'theory of everything' can have no ceteris paribus clauses at all; but then again, fortunately, it does not need one either. So the question is of course how big we assume the system to be. In partial analysis we consider very small systems, like single markets. General equilibrium theory is trickier.[15]

We now see that ceteris paribus laws fence off disturbing phenomena while the inevitable phenomena—in the Keynes example, in the Lucas critique, or in the Giffen goods example—are rather to be fenced in. In such a case, the problem is not of vacuity, but of inconsistency.

4. THE NEED FOR A LOGICALLY INDEPENDENT EXPLANANS

The law of demand predicts a correlation between price and quantity demanded only under the caveat specified. The design of a finite ceteris paribus clause requires prior understanding of all helping factors of a causal process that leads to a particular configuration of phenomena. But what factors are relevant to do the hedging? Scientific research

[15] I believe that one of the causes due to which macroeconomics often poses perplexing difficulties in specifying its conceptual apparatus is that there is little recourse to ceteris paribus clauses.

implies a lack of knowledge, not a surplus. In curiosity driven investigation we enter partially new terrain. Hence we *typically* do not know enough to design well specified clauses. Now, the use of ceteris paribus runs the risk of vacuity and economics is vulnerable to such criticism because its clauses are *typically* open. Indeed, economics is a science where the unspecified ceteris paribus clause is in extensive use. And this is true, if only to a lesser extent, of other sciences. Thus, Pietrosky and Rey note that "cp-clauses are needed in science precisely when it is *not* clear what the 'other things' are" (Pietrosky and Rey 1995, 87).[16] So the problem is that open or infinite ceteris paribus clauses leave unclear how many and which of the variables are to remain constant. How should one conceive of non-vacuous ceteris paribus laws in economics and in other social sciences?

Pietrosky and Rey seek a sufficient condition for a ceteris paribus law not to be vacuous. Modest as this objective may seem, their result is sophisticated: the ceteris paribus clause is a condition yet to be specified by the scientist, but specifiable in non-question begging terms. Now, conditions can be 'C-normal' or 'C-abnormal'.[17] "So the task is to find a characterization of C-normalcy that avoids this charge [of vacuity]" (Pietrosky and Rey 1995, 88). This is the trick that does it:

> Metaphorically: cp-clauses are cheques written on the bank of independent theories. These cheques represent a 'promise' to the effect that all C-abnormal instances of the putative law in question can be explained by citing factors that are […] independent of that law. If the promise cannot be kept, the cheque was no good to begin with. Or, in terms of a more traditional metaphor long associated with cp-laws: a cp law holds only in a 'closed system', i.e., a system considered in abstraction from other, independently existing factors (Pietrosky and Rey 1995, 89).

And, one has to observe:

> Our requirement that factors be 'independent' is intended to exclude factors whose only explanatory role is to save a proposed ceteris paribus-law (Pietrosky and Rey 1995, 90).

In sum, Pietrosky and Rey try to avoid vacuity by claiming that ceteris paribus laws are permitted provided that either the clauses

[16] Emphasis theirs. The extent of the problem is implied by the title of section 1.3 of their paper, where they note this: "A problem not only for special sciences".
[17] C is the yet-to-be specified condition. See Pietrosky and Rey 1995, 88.

specify interfering factors in a finite way or if the scientist uses the available evidence to include independent factors that explain an apparent interference. Thus, the crucial factor is the possibility of producing convincing evidence, perhaps in the future, in light of the requirement that such purported evidence is independent from the law. Here we have the fourth case from the list.

Three cases of non-vacuity

We speak of 'disturbing factors' when a phenomenon contradicts a lawlike regularity. The regularity has its exceptions and the phenomenon—e.g., rising demand with rising prices—needs an explanation. If the phenomenon is a disturbing factor this means that it cannot be explained by the theory that produces the lawlike regularity. The explanandum is seeking an explanans.

The scheme below summarises three possibilities for non-vacuity. In the first column we find the three cases of non-vacuity as mentioned in the introduction. (Note that the table does not list case number 1—the case of the ad hoc clause, which was one of vacuity.)

Table 1

Ways to non-vacuously deal with a closure regarding phenomenon **ph**			
case	**Explanans**	**Explanandum**	**Epistemic operation**
2	isolating clause violated	ph is a disturbance	idealisation, fencing off
3	theory about system	ph instantiates own theory	explanation, fencing in
4	other, independent theory	ph instantiates other theory	abstraction

The regularity was hedged, it was part of a closure, so if it can be shown that the hedging clause was somehow violated due to the particular disturbing phenomenon, the phenomenon is explained by reference to the clause being violated. This means that the instability of the environment external to the system under study explains the disturbance. Consequently, the hypothesis expressing the lawlike regularity is saved. This is shown as case 2 in the table.

Case 2 is the Giffen goods one, however, and in such cases, the causal factors breaching the regularity cannot be explained with reference to a hedging clause because theoretical knowledge tells us that these factors will come into play *whenever* the regularity does. The clause would contradict the very workings of the system under study. Now there must be a more abstract theory with a wider scope, explaining the more complex regularity—e.g., talking of substitution effects and income effects and their relative weight in a causal process. Both positive and negative relationships between demand and price instantiate this theory, provided all the conditions are in place (the income effect *was* in fact stronger).

It is, thirdly, also possible that the theory that renders the lawlike regularity does not account for any exceptions to it. Some other theory does. If this theory is logically independent from the theory at stake, the ceteris paribus law is saved and non-vacuous if the exception can in this way be explained. Now the explanans of the phenomenon is the other theory. If this theory really is independent from the theory to which the law belongs—as Pietrosky and Rey demand—this presents a genuinely new case 4. In the following section, I will explain why I think this a case of abstraction.

5. BEYOND THE CETERIS PARIBUS CLAUSE: ABSTRACTION

If the disturbance of economic regularity by variables, which we find listed in a clause, can be explained by citing particular factors that *really* are independent from the theory for which the clause was used, these factors will frequently reside in disciplines outside of economics. After all, the factors cannot originate in the system about which we theorise. If they did, their influence on the system studied would be part of the workings of the system. Moreover, hedging it from causal influences inherent in this system would be, as stressed above, inconsistent. So then this question turns up: how big is the system?

Economists research (models of) economic systems. Independent factors are most likely to be found, in consequence, in non-economic systems. Indeed, the sort of disturbances that challenge the ceteris paribus law do in fact often originate in domains studied by sociologists, psychologists, biologists, and others: psychological irrationality (such as cognitive dissonance or preference intransitivity), political instability, climate change, and so on. An economic system may of course also be embedded in a context that is itself economic in kind; that is, the independent factors that sustain the application of a ceteris paribus clause do admittedly perhaps originate in the economic discipline itself. But more often the concept of an 'external shock' refers to a factor or factors outside of the economic realm.

From ceteris paribus to the role of abstraction in science

Let us suppose that we study microcredit facilities to finance manufacture in an urban area and its effects on poverty. Economic theory predicts that better access to credit reduces poverty due to new possibilities for poor families to set up a small business. What is more, the theory predicts that this effect will turn up in New Delhi just as well as in London, and in the urban locality under study with respect to manufacture as well as in an agricultural local economy with respect to the financing of fertilizers. This means that the prediction is nothing other than the prediction of a lawlike regularity.[18]

But to think, as policy makers often do, that the lessons drawn in one case travel to a new case without any problem amounts to the presupposition that the institutional, sociological and psychological 'helping' factors remain the same. To assume that what we learn in one case also applies to another entails employing a ceteris paribus clause. This is tricky. If microcredit helped reduce poverty in New Delhi, will it do the same in London with its well developed financial institutions, its depth of the capital market? It is not clear that it will. And if the availability of microcredit in a densely populated city increases industriousness and family income, does it do the same in the countryside where markets are much more dispersed? Probably not, but we do not quite know what causes what. And if we did, the presumption that the effects of microcredit are the same in such diverse institutional and geographic contexts would amount to applying one and the same ceteris paribus clause equally in these different contexts. But we do not know much about all these variables that make up the context.

So if predictions on the basis of economic theory prove wrong, reference to each and every one of the independent factors is needed, for instance to the particular organisation of the local capital market or the proximity of consumer markets, in order to explain the instances that may falsify the prediction. The problem is that we need to ban *any*

[18] Note, by the way, that already the interest in microcredit shows that the institutional background is supposed to somehow matter: pure economic theory—abstracting from the role of institutions—would never distinguish between the availability of large sums of capital and microcredit financing by loans of 250 dollars. Pure economic theory would just sustain talk of 'the' capital market, not of markets that can only be told apart by reference to an institutional background.

disturbing factors from the stage. This formulation uncovers our ignorance, because we do not refer to specific elements of the 'other circumstances', our research is just troubled by the richness of the context in which the cited economic mechanism is supposed to work. We are, in other words, not capable of explicating a finite ceteris paribus clause. If we could list all the relevant background causal powers supposing to co-determine the success of our policies, we would have a road map towards non-vacuous immunisation of our prediction. We would have a ceteris paribus clause. Alas, we cannot.

We somehow want to say that microcredit boosts entrepreneurship because we believe that it mobilizes idle resources, *unless counteracted by external factors.* But this is an abstraction. It is an abstraction because it is an approach that focuses on a particular causal chain with no reference to any other causal mechanisms than the one cited by the economic theory we happen to employ. Again, this is not because we have a clear insight into what causal factors may alter the direction of events—due to which we decide to fence these off, but because we do not have sufficient access to knowledge about the staggering complexity of the concrete world and we speculate about one of the causal pathways that appear interesting. Had we known precisely which distribution of other causal mechanisms operate, then we would have little left over to research. The reader can now see that a ceteris paribus clause requires a lot of knowledge beforehand. It requires the research to already have been done.

An example from physics may further explain the point I am making. When Boyle investigated the gas laws, he was only interested in an empirical relationship between volume and pressure. The more than a century younger ideal gas law, in contrast, also relates temperature in addition to volume and pressure. In formulating his laws Boyle could not put temperature in a ceteris paribus clause, because he was not aware of the more complex relation between the three magnitudes.[19] Instead he had *abstracted* from temperature, just as he had *abstracted* from variations in the weather or possible elasticity of the gas container. Abstraction is the exclusion of (the influence of) objects, their properties, and the phenomena these objects take part in, in a non-explicit way. If the exclusion were explicit, it could not be abstraction;

[19] Boyle's relationship holds on an isothermal, which is of course *paribus*. Yet another century later, in 1873, van der Waals also entered the attraction and spatial extension of the molecules, generalising the law to liquids. Boyle could never have provided for a clause to deal with these influences.

using these concepts in the way stipulated above, it would be idealisation. If isolations must be well laid-out, a prior understanding of a lot of concrete detail is required. The specification of ceteris paribus clauses is only possible *after* the research is done, not before.

Abstraction is a mode of reasoning that transcends a particular level of concrete detail. It is what scientists do when they focus on an apparent causal power in the belief that the world has causal structure and that the study of the causal power in question helps understanding this world. Thus, scientists know that any phenomenon is driven by a jungle of causal powers, but it makes sense to focus on one mechanism which—to the researcher—seems to make the difference. So could Boyle not better have researched the relationship between temperature and pressure in stead of volume and pressure? Or should he have studied the pressure of the atmosphere and the pressure of the gases in the container? What pattern is essential and which ones possess superfluous detail?

Clearly, the creativity of the scientist remains a mysterious process. The generation of causal hypotheses is a speculative business and the trick for any scientist is to abstract well.

Economics needs the social sciences

Now consider the following. The success of policies depends on the application of socio-economic knowledge. But even if we had a social theory so strong as to give a universal pattern in social life, this theory would not teach much about the concrete detail that becomes relevant for policy recommendations. This is because such an implementation requires knowledge of the way in which the policy is going to work out in practice. As every new context provides the policy maker with a new distribution of the factors that are to help the policy to be successful, it is unclear how the purely theoretical knowledge has to be supplemented with information about the relevant facts of the situation given.

In comparison, take the Newtonian motion laws that do describe a universal pattern. To calculate the effect of friction also involves knowledge of the specific properties of the substance that causes the friction; the quantified outcome of the motion laws will be different in a swimming pool than in the air. So to apply knowledge in the abstract a reasoning mode inverse to abstraction is needed: let us call this *concretisation*. Detail of a target situation has to be introduced into the corpus of knowledge that had been abstracted from study situations.

So scientists solve two puzzles: theorising requires selecting the right focal points of interest by an operation called abstraction, application of abstract knowledge requires the insertion of the relevant details of the target situation.

The first puzzle is how to help science progress by abstracting fruitfully. The second puzzle is how to apply what we learned. The last row in the table above shows that abstraction in one discipline (say, pure economics) leaves some concrete aspects of the world (say, concerning institutions) aside. These concrete aspects neglected by the first discipline have to turn into objects of explanation by one or more other disciplines (e.g., institutional economics, anthropology, and the like). Interdisciplinary knowledge builds the more informed picture.[20] For effective policy, economics needs the other social sciences.

6. TYPE 2 VACUITY: HOW CAN CETERIS PARIBUS LAWS BE TRUE?

I started out observing that the issue of vacuity concerns two distinct problems, one about ceteris paribus laws being empirically empty (vacuity type 1) and one about such laws being true (vacuity type 2). So far I developed the argument about the first problem. This section deals with the second.

The logic of ceteris paribus

A ceteris paribus clause describes a state of the world which is possible, not actual. This must be the case because it says that things do not change. Only if potentially disturbing variables do in fact, i.e., in the actual world—exert their causal influence do we really need such a clause: they are false as a description of the actual world whenever we need them. Otherwise we could abstain from using them. So what about the *truth* of ceteris paribus laws? I can see only two strategies to answer this question. The first is to conceptualise ceteris paribus laws as *material* if-then implications. The second is to understand them as *counterfactual* if-then phrases. I believe that only the second strategy is feasible. The reason for this is that material implications are logically too weak. After all, if the antecedent of a material implication is false, the entire conditional is trivially true.

[20] Whether this picture is informed enough for effective policy depends on, among other things, the level of abstraction neither being too high, nor too low. For an elaborate analysis of why policies often go wrong, see Rol and Cartwright 2012.

Let us look more closely at the logical form of a material implication. All an 'if P then Q' sentence claims is that '*if not Q, then not P*'. The ceteris paribus clause is the condition, or the antecedent of the conditional claim. So considering the law of demand and the possible disturbing influence of incomes and the number of demanders, we get a construction like 'if ceteris paribus the incomes and number of demanders, rising prices decrease quantity demanded'. But then all we end up with is 'if rising prices did not come with falling demand, then apparently the ceteris paribus clause was not true'. This is the logical form of the immunisation that Pietrosky and Rey were bothered by. Meanwhile, with a false ceteris paribus clause in the antecedent the conditional sentence 'if the conditions hold, then the relationship between price and demand holds' can only be true. So now the lawlike expression is trivially true. This is an unattractive result. So we are faced with the trouble that, if a ceteris paribus law is a material implication, it turns semantically vacuous even if we can think of ways to test it.

Even without a solution—which I claim lies in the second strategy—this is interesting. So far the debate (see footnote 4 above) has been about how the falsity involved in a ceteris paribus clause can be handled by truth seekers. Realists, but also nearly all philosophical positions countering realism, such as instrumentalism, admit the need for true claims in order to implement policies successfully.[21] Is this policy going to work? Yes it is. We either defend the truth of such an answer or we end up in an extremist post-modernist position without interest whether it works. I conclude that truth matters and that it should not come trivially.

My claim, then, is that ceteris paribus laws are counterfactuals. They are conditional (if-then) sentences and the antecedent of the condition is phrased as a subjunctive: '*had* the *ceteri* been *paria*, the postulated regularity *would* turn up'. Now, mere subjunctive conditionals are silent about whether the condition is met or not. But note that ceteris paribus laws imply that the condition is not met. Therefore, the special form of such a subjunctive conditional that is at stake here is a *counterfactual*: a phrase about a non-actual state of the world, a world where the condition imposed is met although we know that in the actual world it is not. Semantically, one could say that

[21] This may be controversial. Many anti-realists tend to dismiss truth as a matter of interest. But note that I here focus on the implementation of policy. We cannot avoid an interest in truth values and *alethic* modalities if we want to know whether the policy worked or not.

ceteris paribus laws describe another world than our actual world. This understanding of the use of ceteris paribus laws in science gives a crucial insight into what science does. Science explains by citing causal patterns that we expect to operate even in states of the world that, so to say 'are not'. Science teaches us how to think about *possible worlds*, hence its lawlike character.

Possible worlds

It is common to view laws in science as counterfactuals, with initial conditions in the antecedent. Recently, for instance, Marc Lange tried to deal with the trouble that the antecedent background conditions must always be consistent with the laws cited. It is due to this condition that these laws delimit the range of permissible states of the conditions. Thus, he saves the nomological character of the law by answering the question *why did things have to come out this way* as follows:

> "In view of these initial conditions, things would have come out this way *no matter what*". The limits of what can count as "no matter what" are determined by context and […] by some set's range of stability (Lange 2005, 427).

The previous section stressed the role of context too, but in light of successful abstraction, rather than concerning laws. Note, however, that the application of the logic of counterfactuals that I focus on here is different. Economists tend to think in terms of conditions that do or do not *in fact* remain the same. Ceteris paribus laws are (implicitly) seen as universal material implications that escape testing when the conditions do not conform to the clause; we have seen that this is also how Hausman treats them. In logical consequence, the problem of trivial truth looms as soon as the antecedent is false. This problem fades if the ceteris paribus clause is understood as the antecedent part of a counterfactual, quite apart from problems of the nomological character of laws (these do not form the topic of this paper).

Once we accept that ceteris paribus laws are counterfactuals, the possible-worlds semantics literature opens up.[22] For my present purposes it suffices to note the following observations about the counterfactual interpretation of ceteris paribus laws.

[22] Kripke, Lewis, and Stalnaker have driven this literature. It takes us too far to present the logic behind counterfactuals in this paper. For an interesting analysis, see Lewis 1973.

A counterfactual claim describes a state of the world that differs from the actual state of the world; it describes how reality *could be*. First, this is not useless. We imagine other possible states of the world—in brief, 'other worlds'—all the time. In order to avoid a crash, I drive carefully. That is, I can see what the possible world in which I do not drive carefully will bring me: a crash. To say that 'if I do not drive carefully, I will crash my car' amounts to saying that I can have positive knowledge about particular states of the world that are not actual (but possible). Note that defending the credibility of the claim does not *require* any explicit recourse to laws. Secondly, it makes sense to say that the counterfactual claim is true or false. The description of the possible world where we all drive carelessly, such that the claim that there would be many car crashes is true, is a non-trivial description. It enables us to derive a warning when combined with the normative claim that we should try to minimise car crashes. Finally, descriptions of possible worlds may be true of any number of the infinite set of possible worlds. (Some such descriptions may even be true of the actual world, but this would, by definition, not sustain a counterfactual claim.) The more possible worlds fall under a description, the weaker the claim is. A weaker counterfactual claim has greater extensionality over the set of possible worlds.

To repeat, my proposal is to interpret ceteris paribus laws as conditional propositions, the antecedent of which contains the ceteris paribus clause, which in turn describes a non-actual state of the world. The entire counterfactual has truth value. If the antecedent is false—and it is when we really need a ceteris paribus clause at all—the counterfactual as a whole does not turn trivially true by implication. Counterfactuals provide precisely the logical form we need to make lawlike claims.

The logic of abstraction

I have distinguished abstraction from the use of ceteris paribus propositions. Abstraction does not require any explicit fencing off, so it does not deliberately use a ceteris paribus clause. Abstraction does not necessitate a conditional. But when it comes to the truth of abstractive claims, the proposition that is being abstracted *from* must be true if the abstracted proposition is to be true. If 'demand relates negatively with price' is true, we can be confident that 'demand relates to price' is true. So we see here that abstraction does not use counterfactuals; its logic is different. While ceteris paribus laws can be true or false, abstraction is truth preserving.

This makes sense. Theories have been derived from descriptions of concrete phenomena. If the description of a phenomenon is not correct to start with, anything may happen when we theorise about it. But if the description is truthful, the theory abstracts from it and the abstracted claims that follow leave out many details. Given that all the descriptions of detail are correct, whatever is selected in the abstraction continues to be described correctly. There cannot be a loss of truth value here.

7. THE PROBLEM OF LOGICAL WEAKNESS: REPRISE

While some proposition may be true of many worlds, even if not of the actual world, abstraction of this proposition leads to claims that are true of more possible worlds. This is because abstraction is a process by which concrete detail is left out. The more details, the fewer the number of possible worlds that fall under the description of these details. So, conversely, a less detailed—i.e., a more abstract—description counts more possible worlds in its extension. In other words, an abstract description of the world picks out more possible states of the world from the space of possible states than a concrete description. Thus, the very simple motion laws of Newton are highly abstract: they apply to the fall of objects in my room and to the elliptical orbits of planets. Such a level of abstraction allows for many possible states. The complex and detailed description of this particular feather and how it behaves in its environment, specifying air pressure and so on, allows for fewer possible states.

Abstraction and truth

Truth is a property of propositions that refer to the actual world. A proposition is true if it correctly describes the state of the actual world; false if it does not. This I call the naïve sense of truth. Now, in possible-worlds semantics, matters of truth are more sophisticated. Counterfactuals refer to other states of the world than the actual state, so they cannot be true in the more naïve sense. But they can be true in the sophisticated sense: a true counterfactual counts at least one possible world in its extension. We say that if the ceteris paribus clause were true, demand would negatively relate to quantity demanded. And we believe that this is correct, even though in the actual world the clause is not true.

Consequently, we have the sophisticated concept of 'being true of possible worlds', alongside the naïve concept of merely 'being true' (of the actual world). Although ceteris paribus laws tell us something about situations that are not actual, they do inform us about situations that are (or may become) actual. Consider the example I introduced above: 'ceteris paribus incomes, demand negatively relates to price'. An abstracted version of this could be: 'ceteris paribus incomes, demand has a functional relationship to price'. In a world with rising incomes, increased prices will perhaps not trigger a drop in demand. But the abstract functional relationship remains. In the case of Giffen goods, the relationship may even be positive. The more abstract proposition is true of Giffen goods too because it does not specify whether the relationship is negative or positive, while the less abstract proposition does.

This semantics answers the question of how counterfactual descriptions can be true. It also helps explain how they can be false. A counterfactual description may be judged false if the possible worlds it describes correctly are worlds where the laws of nature are different from how they are in the actual world.

Vacuity looms again

It follows that abstraction increases the extension of the proposition abstracted from. A very important property of abstracted propositions is that they are logically weaker than their more concrete counterparts. This is precisely because they exclude less. More appropriately, abstract descriptions exclude fewer worlds about which they speak falsely. This is both fortunate and a problem.

It is fortunate that abstraction more easily leads to truth (i.e., in the naïve sense: actual truth) for rather simple reasons. In order for scientific claims to have a bearing on the actual world, we better end up with propositions that are indeed true of the actual world, not just of some possible world. Also, actual policy takes place in the actual world. It has been noted above that policy recommendations are in fact predictions: if you implement policy X, you will get result Y. If the prediction is false, the policy will not work.

But, at the same time this could be problematic. The extent to which we can employ theoretical knowledge for the successful implementation of policy—knowledge which, for its theoretical qualities—depends on our capability to concretise the theoretical claims in the policy situation.

And this is easier the *less abstract* the language in which the theoretical knowledge is formulated. Abstraction tends to lead to truth, but also to logical weakness and this in turn causes difficulty in filling in the concrete detail.

Economists have often been criticised for their alleged abstract theoretical approaches to policy issues. The idea is that too much abstraction ends up useless. I believe that this need not be the case, but it is a warning nonetheless. Irrelevance is the other form of vacuity. And again we see that logical weakness leads to claims that are true trivially.

Engineers need to fill in a lot of concrete detail when they apply physics theories. Social practitioners do so when they apply social knowledge. In policy, what has gone up (producing theoretical knowledge) must come down too (producing useful knowledge). It appears that engineers have an easier life than politicians, because the natural environment processes less information and is more stable. So especially in social science it is important not to abstract too much. Abstraction must render claims both weak enough and strong enough. They must be weak enough to apply to the actual world and strong enough to enable useful concretisation. It is no easy task to find the right level of abstraction.

8. ECONOMICS NEEDS THE OTHER SOCIAL SCIENCES TO MAKE SENSE

Above I cited Pietrosky and Rey who stress the need to produce evidence from independent factors. The idea was that, if scientists can point to disturbing factors *independent* from the lawlike generalisation that is subject to the disturbance, then the generalisation is saved from vacuity. What if the independent factors are as yet unknown? I concluded that the epistemic operation at stake here is abstraction rather than the employment of a ceteris paribus law. Is there a correct level of abstraction and if so, what is it?

If theories should neither be too weak nor too strong—neither blocking the way to concrete implementation of policies nor being insufficiently informed by theory—how strong do we want to have them? There is no algorithm available for how to choose the level of abstraction.[23] If theoretical information derived in a study situation is

[23] Siegwart Lindenberg has proposed an interesting procedure for determining the “right” level of abstraction: the method of decreasing abstraction. It amounts to the idea that, at low levels of abstraction, too much information is processed for a

to instruct us in a new concrete target situation, policy makers have to know enough of the details of the environment in which the target is embedded; and, sometimes, they do not know enough. This is because disciplinary abstraction often drives the policy. Economic policy is driven by economic insights, psychological intervention by psychological concepts; we carve up the social world along our disciplinary lines. But it is not clear that the resulting picture is entirely helpful in shaping successful policy.

Elsewhere I defended the thesis that policy fails too often due to overconfidence in the external validity of policy evaluation (Rol and Cartwright 2012). I proposed that, in order to choose the right level of abstraction, scientists need an interdisciplinary view. The idea is that theoretical knowledge provided by one discipline has to be complemented with information from another discipline, so that the helping factors that really matter spring to the eye more easily. Raising chances of success is already a lot in a world of uncertainty. In other words, one social science increases its relevance if its practitioners develop an interest in what other social disciplines have to say.

So does especially theoretical economics, because economists enjoy a more isolated position among the sciences by default than, say, sociologists. Therefore this claim is not trivial.

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Adam Smith's theory of absolute advantage and the use of doxography in the history of economics

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Abstract: This article reconstructs Adam Smith's theory of international trade and compares it with the way it is presented in modern textbooks as the theory of absolute advantage. This textbook presentation falls short of Smith's original ideas. I argue that the reason for this is the doxographic reconstruction of Smith's theory to fit him into a Whig history of international trade theory. In this way the historiography of international trade theory has falsely established Smith as a forerunner of modern neoclassical trade theory. I conclude by discussing to what extent Smith's insights can still be relevant today and what can be learnt from the mistreatment Smith has suffered in the historiography of international trade theory.

Keywords: Adam Smith, absolute advantage, international trade theory, history of economics, doxography, Whig history

JEL Classification: A11, A20, B12, B31, F10

Adam Smith is recognised as the founder of modern economics and as one of the first and most famous thinkers who argued in favour of free trade. However, his theory of international trade is rather poorly known or appreciated. Today most textbooks of economics in general—and of international trade in particular—start their introduction to trade theory with a short chapter on Adam Smith and the theory of absolute advantage, a theory allegedly invented by him. These texts then swiftly discard the absolute advantage theory in favour of a comparative advantage theory, which is connected to David Ricardo. However, Smith's writings include a more sophisticated theoretical approach to international trade than he is given credit for in the textbooks. In particular, his account shows that unrestricted trade and free

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international competition are more beneficial to a nation than the mercantilist economic policy that existed in many parts of Europe during the 18th century.

This article presents a deeper understanding of Smith's original ideas. Before I take a closer look at Smith's writings, I will briefly discuss the different methods used to reconstruct the ideas and theories of past thinkers. Then I will examine Smith's works, and compare them with the textbook version of his theory. The result is that Smith's original account differs widely from its textbook presentation. Textbooks use a deficient and illegitimate approach to the reconstruction of Smith's ideas, namely doxography. This approach has been used with the aim of including Smith in a cohesive history of international trade theory that leads straight to modern neoclassical theory. In the last section, I will discuss what modern economics can learn both from Smith's original ideas and from the way in which his theory has been misrepresented.

RECONSTRUCTING PAST IDEAS

Richard Rorty (1984) differentiates between four genres commonly used in the historiography of philosophy: historical reconstruction, rational reconstruction, *Geistesgeschichte*, and doxography. These genres are used to reconstruct past ideas and theories and can also be applied to historiography in other sciences. For example, Mark Blaug discusses their application in economic historiography and shows that they are "identical to recognizable styles in the history of economic thought" (1990, 27).

Historiography takes the form of historical reconstruction if the terms, problems, and theoretical approaches of past thinkers are described as they were intended in their original context. Writings are reconstructed in order to reproduce their original intended interpretation without seeing them through the eyes of subsequent discussions, theories, or paradigms (see Ziegler 2008, 3-6). The focus lies on what a past thinker actually said and what he or she meant with the analytical concepts, the theoretical approach, and the language he or she used. Thus, past ideas are portrayed in such a way that the discussed thinker would be able to join the discussion.

Rational reconstruction, in contrast, tries to dress up "past ideas in modern garb" (Blaug 2001, 150). It treats "the great dead thinkers of the past as contemporaries with whom we can exchange views" (Blaug 1990, 28). This approach might lead to a contortion of what the past thinker actually said. It can be charged with anachronism, since it focuses on what past thinkers would have said and what their implications would have been if they had used modern analytical tools. Thus, this approach takes historical texts and propositions out of context and evaluates them against current scientific understandings. However, this is not objectionable, if it is "conducted in full knowledge of [its] anachronism" (Rorty 1984, 53).

Geistesgeschichte (literally 'history of the spirit') resembles rational reconstruction, but takes place on a bigger scale. It "works at the level of problematics rather than of solutions to problems" (Rorty 1984, 57) and tries to identify "the past writers' central issues and their origin" (Johnson 1992, 22). Its objects of investigation are ideas rather than a single scholar. It "wants to give plausibility to a certain image" and as such is used for "canon-formation" (Rorty 1984, 57).

All three genres—historical reconstruction, rational reconstruction, and *Geistesgeschichte*—are to a certain extent "interdependent and complementary with one another" (Johnson 1992, 22). There is not necessarily a conflict among them. In practice, most reconstructions use a mix of them. Which approach is taken depends on the vantage point and on the question that should be answered. Yet, all three genres are legitimate approaches and essential for historiography.[1]

There is also, however, a fourth genre which Rorty terms *doxography* and which he argues is a deficient approach to reconstruction (see Rorty 1984, 61). Here, the ideas of a past writer are not just interpreted with respect to the historian's interests, but the discussed thinkers are 'decorticated'. The starting point of this approach is to question: what should a past thinker have said? In economics, this is "the attempt to describe theories of the past in terms of some form of modern economic theory under the presumption that the issue, purpose, and goals of past economists are the same" (Johnson 1992, 22). It proceeds as if a past economist had an implicit view on a modern topic, even if this deforms the author's original ideas. The problem with this approach is that past ideas are reinterpreted in such a way that they lose their original meaning and are adulterated. Doxography is different

[1] Rorty emphasises the relevance and legitimacy of these three approaches: "Rational reconstructions are necessary to help us present-day philosophers think through our problems. Historical reconstructions are needed to remind us that these problems are historical products, by demonstrating that they were invisible to our ancestors. *Geistesgeschichte* is needed to justify our belief that we are better off than those ancestors by virtue of having become aware of those problems" (Rorty 1984, 67-68).

from a mere rational reconstruction. It attempts "to fit all texts into some recent orthodoxy to show that all those who have ever worked in the field have in substance treated exactly the same deep, fundamental questions" (Blaug 1990, 28).

ADAM SMITH'S THEORY OF INTERNATIONAL TRADE

The following analysis of Smith's theory is based predominantly on historical reconstruction.[2] The main aim is to stay close to Smith's original writings in order to understand what Smith meant, rather than "what later generations would like him to have maintained" (Winch 1978, 5). To prevent misunderstandings, it should be pointed out that I will not conduct a contextual analysis on how Smith's theory developed, who influenced him, or the contemporary discussions in which Smith positioned himself. Therefore, the historical reconstruction will necessarily be incomplete. This is because the focus of this article does not lie in the development of Smith's thinking but in the comparison of his original theory with its presentation in modern textbooks. And to achieve this, an analysis of Smith's original texts is sufficient.[3]

For Smith, international trade has the same underlying cause as all kinds of trade. In *The wealth of nations* (*WN* hereafter), trade is the consequence of the human "propensity to truck, barter, and exchange one thing for another" (*WN*, I.ii.1).[4] That does not mean that trade has no selfish motive. On the contrary, whenever people trade with each other they pursue their own interests, not some altruistic ones. They must benefit from trade otherwise they would not pursue it. Thus, merchants

[2] In practice, historical reconstruction cannot clearly be separated from rational reconstruction (see Blaug 1990, 28-29). The approach used here includes elements of the latter to a minor extent in order to facilitate a contemporary reading. But this does not change the meaning of Smith's account. I will, for example, use the term 'international trade' though in the relevant texts Smith speaks instead of 'foreign trade'. Both terms describe the same phenomenon, but today the first term is more common. The term 'foreign trade' indicates that economics was considered more from a national point of view in Smith's lifetime. Smith was, like most economists of his time, a patriot, and as such he is primarily concerned with the well-being of Great Britain. Today, economists may have a less nationalistic attitude.

[3] This does not mean that a contextual analysis of Smith's foreign trade theory would be futile. On the contrary, it would be a worthwhile inquiry that, as far as I am aware of, has not yet been done.

[4] Smith gives two possible origins of this propensity. It might be "one of those original principles in human nature of which no further account can be given" or it could be "the necessary consequence of the faculties of reason and speech", which "seems more probable" (*WN*, I.ii.2).

carry on commerce internationally because they earn profits by it. However, Smith endeavours to show that not only single merchants but the society as a whole benefits from international trade.[5]

The division of labour and its benefits

Smith's thoughts on the division of labour constitute the basis for his theory of international trade. For him, it is the division of labour that leads to "the greatest improvement in the productive powers of labour" (*WN*, I.i.1). As a result of a more advanced division of labour, more output can be produced with the same amount of labour. He illustrates this point with his famous pin factory example,[6] which shows that the division of labour produces an "increase of the quantity of work which […] the same number of people are capable of performing" (*WN*, I.i.5). Then he identifies three reasons for this development:

> first, […] the increase of dexterity in every particular workman; secondly, […] the saving of the time which is commonly lost in passing from one species of work to another; and lastly, […] the invention of a great number of machines which facilitate and abridge labour, and enable one man to do the work of many (*WN*, I.i.5).

The division of labour leads to quantitative and qualitative production improvements. This means that output is increased, technological development is stimulated, and workers' skills and productivity are enhanced. As a result, economic growth is promoted and national wealth increases.[7] This can be summarised as "the more specialization, the more growth" (Staley 1989, 43).

The only limitation on the division of labour is "the power of exchanging", i.e., "the extent of the market" (*WN*, I.iii.1). Consequently, if the market is expanded, an increase in the division of labour will be

[5] Though Smith wants to show that free international trade is generally best, his theory is also valid if trade is partly restricted. Smith himself is aware that complete free trade is unrealistic: "To expect, indeed, that the freedom of trade should ever be entirely restored in Great Britain, is as absurd as to expect that an Oceana or Utopia should ever be established in it. Not only the prejudices of the publick, but what is much more unconquerable, the private interests of many individuals, inevitably oppose it" (*WN*, IV.ii.43).

[6] In this example, ten workers, who are all assigned to specialised operations, can produce 48,000 pins a day, while one worker, who has to do all the separate operations on his own, can merely produce one pin a day (*WN*, I.i.3).

[7] Smith defines wealth as "the annual produce of the land and labour of the society" (*WN*, Introduction).

possible and, as a result, economic growth and wealth will increase. It is in this respect that international trade has to be considered.

Gains from international trade

According to Smith, international trade is advantageous for nations because

> [it] gives a value to their superfluities, by exchanging them for something else, which may satisfy a part of their wants, and increase their enjoyments. By means of it the narrowness of the home market does not hinder the division of labour in any particular branch of art or manufacture from being carried to the highest perfection. By opening a more extensive market for whatever part of the produce of their labour may exceed the home consumption, it encourages them to improve its productive powers, and to augment its annual produce to the utmost, and thereby to increase the real revenue and wealth of the society (*WN*, IV.i.31).

Here, Smith connects international trade to his ideas of the division of labour. If trade with another nation is established, an extension of the division of labour will be possible because the international market is bigger than the domestic market alone. International trade is thus advantageous to a nation because the enhanced division of labour leads to an increase "of the exchangeable value of the annual produce of the land and labour of the country" (*WN*, IV.iii.c.3). This means that the real wealth of the nation and its population increases.

Controversy has arisen over Smith's statement that international trade "gives a value to their superfluities". This has become known as the "vent-for-surplus" gain, namely: that a nation can exchange its overproduction for other goods which are demanded.[8] In this way, more of its population's wants and needs can be satisfied (as Smith mentions in various paragraphs of *WN*). However this "vent-for-surplus" concept is not a separate theory, as some suggest, but is merely an additional corollary of a wider (international) market. It is trade and the accompanying specialisation that create such surplus products in the first place. As a result of specialisation, each nation produces goods which cannot be sold domestically but must be exported.

[8] The term "vent-for-surplus" was introduced by John Stuart Mill (1965, 591).

The "vent-for-surplus" gain is therefore complementary to Smith's international trade theory (see also Blecker 1997, 530).[9]

To recapitulate, international trade exploits the quantitative and qualitative benefits of an extended division of labour. International trade leads to an increase in specialisation that raises productivity through technical and organisational innovations. Thus, more goods can be produced overall with the same amount of labour. This boosts economic development as resources are activated and industry is encouraged (see Bloomfield 1975, 473). It is obvious that Smith's theory of (international) trade "is closely interwoven with his theory of economic development" (Myint 1977, 233). Trade and development cannot be separated in Smith's theory. They are linked through the division of labour.

The gains from international trade are reinforced by the increased competition that domestic producers are confronted with. This is another advantage, because international trade decreases the likelihood of domestic monopolies (see, e.g., *WN*, IV.vii.c.102). Smith argues that free competition, though often not in the interest of the producers, is always beneficial to the public (see *WN*, I.xi.p.10, IV.iii.c.11).

Smith also mentions an additional beneficial aspect of international trade, namely that it transfers knowledge and technology between different nations. The adoption and use of new production techniques lead to productivity growth and thus to economic development and an increase in wealth. Smith points out that these gains can even be more important to a nation than access to a wider market, especially for a big nation. He discusses this point with regard to China. China already has a large domestic market and would therefore primarily gain from open trade with Europe by getting access to its technology rather than by widening its market (see *WN*, IV.ix.40-41).

Overall, international trade is beneficial to both the individual nations and the world as a whole. Smith has an optimistic view of

[9] Myint distinguishes between two benefits from international trade in Smith's theory, which he labels "vent-for-surplus theory" and "productivity theory" (Myint 1958, 318) and argues that the former applies only to developing countries. However, Smith did not make such a separation. Some of Smith's remarks suggest that the production of one good can also yield another by-product for which a nation has no need of (*WN*, I.xi.c.3-4). In this sense, Kurz (1992, 478) applies the vent-for-surplus concept to "joint-product processes of production". Magnusson argues that the vent-for-surplus idea might only be applicable in the short-run because "it was very difficult in practice for the producers [...] to change from one kind of production to another" (2004, 46). For further discussions of Smith's vent-for-surplus 'theory', see Staley 1973; Kurz 1998, 79-82; Myint 1977; and Elmslie and Sedgley 2002.

growth and economic progress. He never mentions any ceiling to the division of labour; and growth in his theory is boundless (see Darity and Davis 2005, 146-148). The division of labour is limited by the extent of the market, but the extent of the market is not limited in Smith's theory. Rather the market size itself depends on the division of labour and an extension of the division of labour leads in turn to a widening of the market (see Young 1928, 539-540).[10]

In general, it is always more advantageous to trade with a more developed nation that has a more mature economy, because it has a more developed and generally bigger market, which enables a more advanced division of labour. Since Smith is mainly concerned with Great Britain, he argues that free trade with France would be more beneficial than free trade with Portugal because France has a "superior opulence" and "would take more from us, and exchanging to a much greater value and in a much greater variety of ways, would encourage more industry in Great Britain and give occasion to more subdivisions of labour" (Smith 1978a, 578).

Smith's intention is to show that international trade is beneficial for all nations involved in trade. However, he concedes that nations do not necessarily benefit in equal parts: "trade which, without force or constraint, is naturally and regularly carried on between any two places is always advantageous, though not always equally so, to both" (*WN*, IV.iii.c.2). Just as domestic trade is not equally beneficial to all regions within a country, international trade is not equally beneficial to all nations. Trade can even amplify differences between them, especially if they differ in their wealth. In line with this idea, in his *Lectures on jurisprudence* Smith compares the trade relations between a rich and a poor man to that between a developed and underdeveloped nation:

[10] Parts of Smith writings suggest that nations will finally reach a stationary state and fall apart (see *WN*, I.viii.24, III.iv.20). Smith stands in the tradition of David Hume and James Steuart and the "theory of growth and decay". However, Smith is not unambiguous here. Though Smith certainly acknowledges that all nations will finally vanish, he does not argue that there is a cap on economic development beyond which no nation is able to go before it withers away. China, according to Smith, has "been long stationary". It reached this stationary state at least "five hundred years ago [...] perhaps even long before" (*WN*, I.viii.24). However, China could boost its economic growth and leave this stationary state by adopting technologies from Europe (see *WN*, IV.ix.40-41). Thus, China's stationary state is not caused by any a natural ceiling on economic development but rather by "the nature of its laws and institutions" (*WN*, I.viii.24).

> When a rich man and a poor man deal with one another, both of them will encrease their riches, if they deal prudently, but the rich man's stock will encrease in a greater proportion than the poor man's. In like manner, when a rich and a poor nation engage in trade the rich nation will have the greatest advantage, and therefore the prohibition of this commerce is most hurtfull to it of the two (Smith 1978b, 512).

Domestic growth and the patterns of international trade

Smith argues that domestic and international trade are determined by the same rules. The division of labour works internationally the same way it does domestically. A nation, therefore, specialises in the production of some goods while buying other goods from abroad. This is beneficial to a nation: "If a foreign country can supply us with a commodity cheaper than we ourselves can make it, better buy it of them with some part of the produce of our own industry employed in a way in which we have some advantage" (*WN*, IV.ii.12).

This means that a nation produces and exports those commodities which it can produce more cheaply than other nations, and imports those which it cannot. A nation will not produce a good that is produced more expensively at home than abroad—be it "a thirtieth, or even a three hundredth part more" (*WN*, IV.ii.15). As a result, international trade develops in the same way as domestic trade: "Were all nations to follow the liberal system of free exportation and free importation, the different states into which a great continent was divided would so far resemble the different provinces of a great empire" (*WN*, IV.v.b.39).[11] If free trade is operative, consumers will buy a good from whoever sells it at the lowest price. The nation (or producer) with the lowest production costs is able to sell it cheaper than every other producer and

[11] Smith differentiates foreign trade from domestic trade by its slower rate of turnover of capital, which is due to the greater time it takes to cover the greater distances involved in international transactions (see *WN*, II.v.27). However, this general rule does not apply for neighbouring countries, as he demonstrates using the example of England and France. Trade between Southern England and Northern France has roughly the same frequency of returns as domestic English trade, and for Southern England it therefore makes no difference if it trades with Northern France or the rest of England (see *WN*, IV.iii.c.12). Furthermore, Smith differentiates production according to "the quantity of [productive] labour, which equal capitals are capable of putting into motion" (*WN*, II.v.1). According to Smith, production for domestic consumption generally puts more productive labour into motion at home than does production for international trade. On this, see also Tribe 2006.

is able to undersell its competitors.[12] Therefore, every nation will produce those commodities which it can produce more cheaply than other countries.

Production costs are—according to Smith—all those incurred by bringing a product to the market. They include transport costs. Smith emphasises the importance of transport costs frequently in reference to international trade (see, e.g., *WN*, I.xi.c.5, IV.i.29, IV.ii.16, IV.ix.41). This means that different nations can each have an absolute advantage in the same good in different (domestic) markets, taking into account the transport costs from the place of production to the market in which it is sold.

As a result, the direction of international trade is determined by the current absolute production cost advantages, i.e., the costs that arise in producing a good and bringing it to the market. Nations will automatically specialise according to their respective advantages if trade is unrestricted. International competitiveness is determined in the same way as competitiveness inside a nation, i.e., by price advantages.

What are the origins of such advantages? Smith recognises that there are some differences between countries that yield specialisation. These include a nation's "soil, climate, and situation" as well as its "laws and institutions" (*WN*, I.ix.15) and its means of communication and transport (see *WN*, III.iv.20, IV.ix.41). However, Smith's overall approach towards specialisation is that trade and the division of labour lead to specialisation and not the other way around. He argues that specialisation is in most cases not the cause but "the effect of the division of labour" (*WN*, I.ii.4). He gives the example of a philosopher and a street porter who "were, perhaps, very much alike" (*WN*, I.ii.4) in their early childhood. The difference between them arose with their education for different jobs and continued to widen while they pursued those professions.

The same applies to the specialisation of nations. Thus, trade between nations is, in general, not based on the differences between them that existed prior to trade. Rather, it is trade which leads to specialisation and differences. Differences between nations, then, are

[12] During Smith's lifetime, mainly merchants were engaged in international trade. Their chief concern was the money price of a commodity because their intention was to sell goods in order to make profits, i.e., to get high returns on their capital (see, e.g., *WN*, I.v.20, II.v.14, II.v.37). At this time, transnational companies did not exist, nor were average citizens involved in international trade. There were companies that operated in different parts of the world, but they were chartered companies that operated only inside colonial empires and were organised along mercantilist lines.

mainly due to the level of a nation's division of labour, and thus of its productivity and its technology, rather than due to natural differences. The production cost advantages of a nation mainly develop endogenously, through the market-widening effects of international trade (see Blecker 1997, 534).

There is thus a mutual relationship between international trade and domestic economic development. They are dependent on each other and each impact the pattern of trade. A nation's production cost advantage "is endogenously determined by its development path, which is in turn affected by its trade pattern" (Maneschi 1998, 48). Both international trade and domestic development affect the division of labour. As a result, the absolute production cost advantages of a country are not fixed. They tend to be amplified by trade. And they may also change over time. A nation can gain an absolute advantage in the production of a good, for example, or it can lose such an advantage—like a producer in a domestic market.

To sum up this section, Adam Smith's theory of international trade is dynamic in that it is integrated into the broader economic framework of the division of labour. It considers economic growth that results from and affects international trade. Absolute production cost advantages and the division of the benefits from trade are not fixed once and for all. Rather, they develop and emerge endogenously as a result of trade.

ABSOLUTE ADVANTAGE IN MODERN TEXTBOOKS

Subsequent economists did not pay much attention to Smith's theory of international trade. In general, it is not seen as relevant because of the predominance of the theory of comparative advantage, which "has been the bedrock on which all subsequent developments in the theory of international trade have rested" (Maneschi 1998, 10). As a result, Smith's theory was barely noticed and not developed any further. Nonetheless, many of today's textbooks deal briefly with the theory of absolute advantage, which is ascribed to Smith. They portray Smith's theory as "a stepping-stone to a more sophisticated theory" (Staley 1989, 52), namely the theory of comparative advantage that is attributed to David Ricardo. Following this, most textbooks discuss the merits and failings of the Ricardian model and introduce the neoclassical version of the theory of comparative advantage, including the Heckscher-Ohlin model and the factor price equalisation theorem. In this way, Smith's theory is presented as the starting point of a theoretical development that leads directly to neoclassical trade models. Smith's concerns and ideas are thus aligned with those of neoclassical trade theories. However, they are dwarfed by the theory of comparative advantage, which is one of the most praised theories in economics.

The theory of absolute advantage itself is normally presented with an example of two countries and two commodities (2x2 model). Each nation can produce one good with less expenditure of human labour than the other and thus more cheaply. As a result, each nation has an absolute advantage in the production of one good. An example is given in Table 1: Nation A has an absolute advantage in the production of commodity 1 because it needs only 3 labour days to produce one unit of it while Nation B needs 6 labour days. Nation B has an absolute advantage in commodity 2.[13]

Table 1: textbook example of absolute advantages

Days of labour required to produce one unit of	*Nation A*	*Nation B*
Commodity 1	3	6
Commodity 2	8	4

If both nations start trading with each other, each nation will specialise in the production of the good it has an absolute advantage in and obtain the other commodity through international trade. More units of both commodities can be produced overall because the given resources are utilised more efficiently. Through trade, both nations are able to consume more units of at least one commodity. In our example, Nation A would specialise completely in commodity 1, and Nation B in commodity 2. There are no further gains from international trade besides this one-off increase in the overall production and thus consumption. Nothing more happens.

[13] For similar examples using unit labour inputs, see Bieling 2007, 35; Chacholiades 2006, 16-19; Eicher, et al. 2009, 14-16; Engelkamp and Sell 2011, 328-330; Heine and Herr 2003, 617; Koo and Kennedy 2005, 11-13; Mankiw and Taylor 2006, 51; Söllner 2008, 213-214; Wildmann 2010, 58-59; Zhang 2008, 24-25. Others use the reciprocal value, which shows how many units can be produced per labour year, e.g., David and Stewart 2010, 11-12; Markusen, et al. 1995, 69; Mehmet 1999, 46-48; Peng 2011, 151-152; or per labour hour, e.g., Carbaugh 2011, 32-33; Ingham 2004, 12; Salvatore 2011, 35-37. Still others use of labour productivity in general, e.g., Ison and Wall 2007, 393-394; Kjeldsen-Kragh 2002, 11-12; Ströbele and Wacker 2000, 9; Yarbrough and Yarbrough 2006, 27-28. This list is far from being complete. There are many more textbooks that use such numerical examples. The economics textbooks listed here are considered representative.

The presentation of Smith's international trade theory in textbooks is essentially standardised and does not vary significantly. Textbooks emphasise that the theory of absolute advantage "can explain only a small part of world trade" (Salvatore 2011, 37). Thus, it is seen as a special case of the theory of comparative advantage and both theories are seen as complementary (see Dieckheuer 2001, 50). Smith is often criticised for not being able to come up with the more sophisticated theory of comparative advantage (see Zhang 2008, 3). In comparison to Ricardo, Smith is described as a "poor trade theorist" and his theory as a "naive theory" (Mehmet 1999, 47).

However, the textbook account does not fairly represent Smith's theory.[14] In particular, as claiming that trade is only beneficial because it leads to an increase in the amount of both commodities that can be produced with existing production technology and capabilities. This falls short of Smith's theory and is not merely a simplification of it but a false interpretation. The textbooks only present a comparison of two static situations, namely before and after the opening of trade. Smith himself neither uses such a comparison nor does he give a numerical example of this kind. Furthermore, gains in the form of technological change and economic growth are excluded altogether. Thus, the modern presentation lacks the depth of Smith's original theory.

A useful way to understand this type of distorted account is to note that it conforms with Quentin Skinner's (1969) notion of mythology. Skinner distinguishes mythologies from proper history or historiography. They are characterised by historical absurdity. Mythology is defined by Skinner as a methodology for writing history which "can scarcely contain any genuinely historical reports about thoughts that were actually thought in the past" (Skinner 1969, 22). Mythologies are exercises in doxography and therefore Skinner's concept can be used to understand how modern economics uses

[14] It should be added that not all textbooks are guilty of this misrepresentation. Some textbooks forgo Smith in their treatment of the historical development of trade theory, mainly because they reject the idea that the theory of absolute advantage can explain any part of international trade, e.g., Krugman and Obstfeld 2009. On the other hand, there is, for example, a book by Douglas Irwin (2009), which includes Smith's dynamic gains and gives a more accurate account of Smith's theory. Irwin does not reduce Smith's approach to a static numerical illustration of absolute advantage. However, such a book is the exception that proves the rule. By far the majority of textbooks misrepresent Smith. And even Irwin argues that "the standard gains" from trade, according to Smith, result from the use of "limited productive resources (such as land, labor, and capital) more efficiently" (Irwin 2009, 40).

doxography to reconstruct Smith.[15] Mythologies are repeated over and over again and thereby become an integral part of the historiography of a subject and thus part of the collective knowledge of the scientific community.

The textbook presentation of the theory of absolute advantage can be classified as a mythology in Skinner's sense. He defines different types of mythologies, several of which are used by textbooks, namely the *mythology of doctrines* and the *mythology of prolepsis*. First, only a fraction of Smith's theory of international trade, namely that countries specialise according to absolute production cost advantages, is used and it is reinterpreted into an entirely different theory. This exemplifies the first type of the mythology of doctrines: "mistaking some scattered or incidental remarks by one of the classic theorists for his 'doctrine' on one of the themes which the historian is set to expect" (Skinner 1969, 7). Second, the charge or criticism that Smith did not identify the mechanism of comparative advantage and failed to come up with this theory exemplifies the second type of the mythology of doctrines because it presupposes that he tried to (see Skinner 1969, 12-16). Third, because it describes Smith's "work and its alleged significance in such a way that no place is left for the analysis of what the author himself meant to say" (Skinner 1969, 22) the presentation of the theory of absolute advantage also exemplifies the mythology of prolepsis.

Thus, what economic textbooks present is not a legitimate anachronism which could be the basis of proper rational reconstruction or *Geistesgeschichte*. Rather, they adulterate Smith's theory and use the deficient methodology of doxography to reconstruct it.

A WHIG HISTORY OF INTERNATIONAL TRADE THEORY

In order to understand why modern textbooks use doxography to reconstruct Smith's ideas one has to understand how the history of international trade theory is written. As was shown above, Smith is discussed at the beginning of a linear theoretical development that leads directly to the neoclassical formulation of the theory of comparative advantage. As a result, Smith's theory of international trade is adjusted in order to fit into the story of modern economics and, thus, into the

[15] Though Skinner's approach to the legitimacy of historiography is stricter than Rorty's, mythologies in Skinner's sense present modes of doxography as defined by Rorty. See also Rorty 1984, who discusses and relates his own genres to Skinner.

neoclassical paradigm. This paradigm, which is based on marginal analysis, dominates international economics.

Neoclassical theory is the vantage point of the most important and the most widely read textbooks in international economics. Smith is construed "as a forerunner to Ricardo's theory of comparative advantage" (Kjeldsen-Kragh 2002, 89) and, thus, as a precursor of contemporary neoclassical international trade theory. From this vantage point, Smith's ideas are seen as rudimentary and incomplete. Smith himself is not regarded as an ingenious trade theorist and he is criticised for having failed to discover the principle of comparative advantage. In this story, his theory was improved on first by Ricardo (and other classical economists), whose theories were in turn further developed by neoclassical economists. The history of international economics starts with Adam Smith and evolves step by step up to today's standard trade models. It is argued that Smith, together with Ricardo, laid the foundations of modern trade theory.

Since neoclassical thinking dominates current economics, it also dominates the writing of economic history. Neoclassical economists can choose their predecessors, i.e., who are seen as forerunners of its theoretical approach. "Winners" are able to write the history of their subject.[16] This, however, is not a sufficient reason to reject a historiography as doxography. It is an established method of *Geistesgeschichte* and rational reconstruction to interpret and represent the history of a subject as a Whig history (Blaug 2001, 151). A Whig history claims a linear development from past to modern theories, leaving out anything that does not fit into the story. However, if past theories are misrepresented and their original meanings are changed considerably in order to fit them into such a Whig history, this does not constitute a legitimate reconstruction.

A Whig history without any historical reconstructions to keep it honest can easily turn into doxography, as is the case with Smith's theory of international trade. His ideas are reconstructed deficiently and his theory is adulterated to suit the neoclassical history of international trade theory. Such a reconstruction is, in Skinner's words, "a means to fix one's own prejudices on to the most charismatic names, under the guise of innocuous historical speculation. History then indeed becomes

[16] Rorty stresses this point: "Like the history of anything else, history of philosophy is written by the victors. Victors get to choose their ancestors, in the sense that they decide which among their all too various ancestors to mention, write biographies of, and commend to their descendants" (1984, 70).

a pack of tricks we play on the dead" (1969, 13-14). Smith has suffered such a treatment in economics before as the father of free trade. Magnusson (2004) shows how, in a similar Whig history also relying on doxography, the tradition of free trade that was invented and established in the 19th and 20th century made Smith the alleged founder of its ideological movement.

How Smith's theory has been altered

In order to be included in the Whig history of international trade theory, Smith's ideas are fitted into the neoclassical paradigm of the theory of comparative advantage. This is why there results such a discrepancy between Smith's original ideas and their deformed representation in modern textbooks. Smith's ideas are taken out of their contexts and imported into another theoretical framework. To achieve this goal, two main alterations of Smith's approach have been required.

First, his approach has to be translated into a static setting. In contrast to Smith's dynamic approach, later trade theory is predominantly static. Shortly after Smith's death "Ricardo and J. S. Mill increasingly formalized the international trade element of classical economics in terms of the static theory of efficient allocation of given resources" (West 1988, 20). This static approach prevails in the neoclassical theory of international trade. As a result, international trade theory focuses on efficient resource allocation, whereas Smith includes this only as a minor advantage of free international trade. In this respect, neoclassical trade theory falls short of Smith's theory. Smith's approach was, however, reformulated to fit into this static framework. His trade theory was dissociated from economic development so that it could be illustrated by the comparison of two static states, one before and one after the countries started trading. In this illustration, each nation has an absolute advantage in the production of at least one commodity. This framework leaves no room for dynamic developments.

The second alteration is that Smith's approach is fitted into the "Ricardian logic of trade". Buchanan and Yoon (2002) identify two logics of trade, which they label Smithian and Ricardian. The Smithian logic of trade is characterised by the assumption that countries do not need to be different before they start trading. It is through trade and the subsequent specialisation that countries start to differ in their production. Advantages emerge and develop endogenously as a result of trade. It follows, as shown, that Smith's dynamic approach towards trade is coupled with growth. The reason for this is that specialisation leads to an enhanced division of labour and a positive feedback mechanism.

The Ricardian logic, on the other hand, assumes that countries already differ before they start trading. Differences between countries are the only reason why trade takes place, and these do not change after nations start trading. According to this logic, advantages are exogenously given before trade takes place and are not influenced by trade. The Ricardian logic dominates the theory of comparative advantage while the Smithian logic is rejected or ignored. As part of the Whig history of international trade theory, Smith's approach is fitted to the Ricardian logic, and the self-reinforcing mechanism of the division of labour is discarded. That is, textbooks mistakenly argue that Smith assumes that each nation needs an absolute advantage in the production of at least one good to benefit from trade. Moreover, they claim that absolute advantages are given prior to trade—otherwise trade would not be possible—and that those absolute advantages do not change or develop after trade is established (see, e.g., Mehmet 1999, 47).

How Smith was incorporated into modern trade theory

The historiography of international trade theory incorporated Smith step by step. His direct successors did not value his trade theory highly. John Stuart Mill (1965, 591-593) largely argued against Smith's vent-for-surplus approach, and others saw Smith as a poor trade theorist (e.g., Bastable 1897; Hollander 1910; Angell 1926).[17] But gradually Smith's contribution came to be valued more highly—in line with his scientific authority—and he was interpreted as a direct forerunner of the theory of comparative advantage. Many have argued that he paved the way for Ricardo (see, e.g., Kobatsch 1907; Eßlen 1925; Bickel 1926; Viner 1931 and 1937; Sinclair 1932; Haberler 1933; Young 1938; Killough 1938; Samuelson 1948). Although it was noticed that Smith's starting point was the division of labour, rather than static advantages, the division of labour argument itself became more and more appropriated by the Ricardian logic of trade (see, e.g., Kobatsch 1907; Litman 1923; Harrod 1933; Young 1938; Killough 1938).

[17] As noted above, Smith is also widely seen as a poor trade theorist today. The difference is that today his theory is seen as an important part of the development of modern trade theory, while previously his theory was rejected altogether.

The concept of absolute advantage was first used by economists, without referring to Smith, to explain the theory of comparative advantage (see, e.g., Mill 1965 and 1967; Cairnes 1874; Bullock 1913), sometimes with numerical examples (see, e.g., Griffin 1924; Taussig 1927; Sinclair 1932; Haberler 1933). This changed, however, and Smith's name was connected to the concept of absolute advantage (see, e.g., Eßlen 1925; Bickel 1926; Viner 1931 and 1937). By the 1950s and 1960s, this view had become generally accepted and incorporated into textbooks (see, e.g., Harris 1957; Wasserman and Hultman 1962; Wexler 1968).

Numerical examples like the one discussed above were also, wrongly, attributed to Smith (see, e.g., Wexler 1968; Södersten 1970; Adams 1972). Such numerical examples imply both a static approach and the Ricardian theory of trade. With their help, Smith's theory was established as a precursor of the theory of comparative advantage, which is normally illustrated by a numerical example.[18] The use of a similar, though more primitive, numerical example insinuates that Smith used the same method, though in a less elaborate way. This misleading numerical illustration of Smith's theory thus helped to establish the idea of Smith as a direct forerunner of later classical and neoclassical theory.

Other assumptions used by neoclassical trade theory are similarly ascribed to Smith's theory so that it fits well into the Whig history. For example, textbooks wrongly claim that Smith assumes unrestricted domestic mobility of labour and capital. Actually, Smith assumes that neither factor of production is perfectly mobile, whether domestically or internationally. In both cases they are assumed to be only partly mobile.[19] Another example is transport costs. As shown above, they play an important role in Smith's trade theory. However, textbooks falsely assert that Smith abstracts from transport costs as neoclassical models do. This claim was established long ago and is repeated in modern textbooks (see, e.g., Viner 1937, 440; Engelkamp and Sell 2011, 329).

[18] Such numerical examples have been part of the presentation of the theory of comparative advantage ever since Ricardo's formulation of it. Ricardo's own formulation used an example with two countries and two commodities. However, today's numerical examples more resemble John Stuart Mill's numerical presentation than Ricardo's.

[19] Capital can exist in the form of "buildings or in lasting improvement of lands" (*WN*, III.iv.24), which hinders its mobility. Labour is also not perfectly mobile because humans are not willing to move freely and often: "After all that has been said of the levity and inconstancy of human nature, it appears evidently from experience that a man is of all sorts of luggage the most difficult to be transported" (*WN*, I.viii.31). See also Bloomfield 1975, 460.

In this way, doxography became an integral part of the "official" or mainstream history of international trade theory. Today, Smith is universally connected to the theory of absolute advantage and as such he is seen as a pioneer of the theory of comparative advantage. This is a commonplace in economics nowadays and part of many reference works like economic handbooks and encyclopaedias (see, e.g., Jones 2001; Reinert and Rajan 2009; Rutherford 2000).

The use of doxography in the history of international trade

The question remains, why did neoclassical economists use doxography in this particular case? There are two main reasons, namely legitimacy and custom. As to legitimacy, Smith has a "well established reputation as the founder of modern economics" (Tribe 1999, 609). He is renowned and eminently respectable, not only in economics but also in social science as a whole (see Winch 1978, 6). Sceptical readers are more likely to be convinced if one can claim that an established scholar with a high reputation supports one's argument; the status of one's own theory gets more authority inside the scientific community. Therefore, a theory of international trade that refers to Smith increased legitimacy and acceptance. Smith is used by neoclassical trade theory as "as a source of intellectual support" (Magnusson 2004, 23). In this way, Smith "can be regarded as a victim of his own fame and success" (Magnusson 2004, 23). Since Smith was already established as the founder of the free trade movement (see Magnusson 2004), it was convenient to also make him the father of the theory that is primarily used to support free trade, namely the theory of comparative advantage. As a result, Smith is not only seen as a forerunner but even as laying the foundations of neoclassical international trade theory, even though this theory has nearly nothing in common with Smith's actual ideas.

As to custom, Smith's name has been connected to the textbook theory of absolute advantage by virtually all (trade) economists in the history of the discipline. Smith's theory is misrepresented in the present, because it was misrepresented in the same way in the past. As Stigler notes: "If a theory once acquired an established meaning, each generation of economists bequeaths this meaning to the next, and it is almost impossible for a famous theory to get a fresh hearing" (1958, 367). It is questionable whether every textbook author takes the trouble to read Smith's economic opus from beginning to end. Rather most authors get their information about Smith's ideas from secondary or tertiary sources, and thus merely repeat what is thought to be in Smith's original texts. Smith's misrepresentation is repeated over and over again and is thus reinforced.

WHAT ARE THE LESSONS?

After discussing different methods of reconstructing the ideas of past economists, I turned to Adam Smith's approach towards international trade and compared it with its presentation in economic textbooks. I found a great difference between Smith's original ideas and the textbook version. As was shown, neoclassical economists use their theoretical framework to reinterpret Smith's theory in a way that fits their "preconceived ideas of what modern economics ought to be about" (Johnson 1992, 23). Smith's complex approach towards international trade is translated into a different theoretical framework. The history of economic analysis is reduced "to an elegant theoretical exercise in historical positivism" (Johnson 1992, 23). We can now ask what international trade theory can learn from Smith's original ideas and from the way his legacy was treated.

In the last decades, new trade theory has criticised the narrowness of standard trade models and tried to enhance them with new models and assumptions. This can be seen as a movement in the direction of Smith's original ideas.[20] New trade theory focuses on issues long neglected by mainstream trade theory, most famously economies of scale or increasing returns, technological change, and other productivity effects (see, e.g., Krugman 2002 and 1990; Fujita, et al. 1999). Similarly to Smith, it is recognised that "inherent advantages to specialization" (Krugman 1990, 2) play an important role. Economies of scale, technological change and learning by doing are included in most of today's textbooks, mainly described as a supplement to the theory of comparative advantage. In this way, some of Smith's insights, though not Smith himself, have been reappraised and are again acknowledged by mainstream trade theory.

[20] Krugman interprets it in this way by saying that the "long dominance of Ricardo over Smith—of comparative advantage over increasing returns" (1990, 4) is over and both are now seen as more or less equivalent. However, textbooks do not normally connect increasing returns to Adam Smith. Additionally, new trade theory does not refer directly to Smith, and it developed mainly without considering him. However, many new trade theory models could claim Smith, at least partly, as a progenitor because they raise similar issues.

However, Smith's ideas, though only rudimentarily theorised by today's standards, can still be a source of inspiration for modern trade theory. His dynamic perspective can be a useful starting point. His concept of the division of labour and the resulting positive feedback mechanism are much richer and more complex than the neoclassical concept of economies of scale.[21] The connection between trade and growth that is part of Smith's approach deserves more attention. In Smith's approach both production cost advantages and technological change develop endogenously as a result of trade. Most contemporary models still assume that these are exogenously given. For Smith, the division of labour is central and self-reinforcing. Endogenous development results directly from trade: a market expansion leads to a more advanced division of labour, which in turn leads to further market expansion.

This dynamic approach goes beyond both static gains and the Ricardian logic of trade. It entails permanent change and development. Another thought that might be worth considering is that it is more beneficial for a rich, industrial nation to trade with another rich, industrial nation, rather than a poor one, because its bigger market allows for a more advanced division of labour. In contrast, the standard theory of comparative advantage argues the converse, namely that an industrial, relatively capital-rich country benefits most from trade with a poorer, relatively labour-rich country. Smith's claim that a rich country gets a greater *share* of the benefits from trade with a poor country might also be worth some consideration.

Additionally, lessons can be drawn from the use of doxography in the reconstruction of Smith's trade theory. Smith's insights were reinterpreted into a neoclassical framework and fitted into a Whig history of international trade. This version of Smith is merely used to reflect the dominant thinking. Instead of advancing his original ideas, trade theory forgot them. By using doxography rather than a more adequate genre of historiography, the chance to learn from Smith was missed. Though mainstream trade theory improved the formalisation and predictive power of trade models, they ignored at the same time some of the most important issues of international trade, which with

[21] The neoclassical concept of increasing returns implies that when all factor inputs are increased by an identical proportion, output increases in a greater proportion. Furthermore, textbooks often fit increasing returns and technological developments into a static approach and into the Ricardian logic of trade which still prevails in neoclassical theory.

Smith had dealt.[22] It took a long time for ideas, which Smith had put forward nearly 250 years ago, to be again considered by mainstream trade theory.

This article has shown that reconstructing Smith's economic ideas based on one's prejudices about his intentions can easily lead to an interpretation that is historically meaningless. An honest historical reconstruction of Smith's approach towards international trade, on the other hand, can show how his ideas can still be interesting for modern economics. Contemporary economics can benefit from proper historiography. Though his overall approach might be outdated, it is still worthwhile to study Smith because doing so may spur a new perception of a given phenomenon. As was shown, Smith's misrepresentation is firmly established in international economics. It will be hard to establish a more adequate presentation of Smith's original trade theory. It is, however, not impossible and well worth trying.

CONCLUSION

In this article, it was argued that economic textbooks use doxography to interpret Smith's ideas on international trade. Smith's original theory was discussed and compared to its representation in modern textbooks. It was shown that these textbooks do not reproduce Smith's theory slightly inaccurately, but adulterate it completely. They attempt "to impose a canon on a problematic constructed without reference to the canon" (Rorty 1984, 62). The static neoclassical canon is imposed on Smith's dynamic trade and growth theory, which is constructed neither as a static model nor with neoclassical assumptions. Likewise, the Ricardian logic of trade is imposed on Smith's 'trade-cum-specialisation' approach, which is based on a very different logic. In this way, Smith is fitted into a Whig history of international trade theory, and his name is misleadingly attached to the textbook theory of absolute advantage.

It is, however, not uncommon in science for a name to be wrongly attributed to a theory or concept. This phenomenon is known as "Stigler's Law of Eponymy", which states that "[n]o scientific discovery

[22] Krugman (1990; 2002), for example, argues that elaborate concepts and ideas were disregarded by neoclassical economists because they could not be formalised with the mathematical models existing at the time. In the case of trade theory, formalisation can thus be seen both as a step forward and as a step backwards at the same time.

is named after its original inventor" (Stigler 1980, 147).[23] Usually, this mistaken appreciation is a somewhat undeserved honour. Eponymy after all is "a mnemonic and a commemorative device" that is "the most enduring and perhaps most prestigious kind of recognition institutionalized in science" (Merton 1973, 300). But in our case, Smith is not the mistaken recipient of an undeserved honour. Rather, Adam Smith's theory of absolute advantage is a huge diversion from the recognition that Smith's original ideas deserve.

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[23] This is true for Stigler's law of eponymy itself, which was first stated by Robert K. Merton. It is also true of many cases of eponymy in international trade theory besides Smith's theory of absolute advantage. Cases of this law include, for example, Ricardo's theory of comparative advantage (the way it is commonly known today was first phrased only later by J. S. Mill; additionally Torrens formulated this theory similarly shortly before Ricardo), the Ricardian model and the Heckscher-Ohlin model. There are also exceptions where this law does not apply, e.g., the Lerner-Samuelson theorem.

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Identity problems: an interview with John B. Davis

JOHN B. DAVIS is professor of economics at Marquette University (USA) and professor of the history and philosophy of economics at the University of Amsterdam (Netherlands). He holds PhDs in both philosophy (1983, University of Illinois; under the supervision of Richard Schacht) and economics (1985, Michigan State University; under the supervision of John P. Henderson and Warren J. Samuels).

He has published on many areas in the philosophy, history, ethics, and methodology of economics. His published monographs include *Keynes's philosophical development* (Cambridge, 1994); *The theory of the individual in economics* (Routledge, 2003); and *Individuals and identity in economics* (Cambridge, 2011). He co-authored *Economic methodology: understanding economics as a science* (Palgrave, 2010) with Marcel Boumans. In addition to his research on identity and the theory of the individual, he has written extensively on recent changes in economics. He is a past president of the History of Economics Society (HES), the International Network for Economic Method (INEM), and the Association for Social Economics (ASE), and past vice-president of the European Society for the History of Economic Thought (ESHET). He is a past editor of the *Review of Social Economy*, and is currently co-editor with D. Wade Hands of the *Journal of Economic Methodology*.

In this interview, Professor Davis discusses the evolution of his career and research interests as a philosopher-economist and gives his perspective on a number of important issues in the field. He argues that historians and methodologists of economics should be engaged in the practice of economics, and that historians should be more open to philosophical analysis of the content of economic ideas. He suggests that the history of recent economics is a particularly fruitful and important area for research exactly because it is an open-ended story that is very relevant to understanding the underlying concerns and concepts of contemporary economics. He discusses his engagement with heterodox economics schools, and their engagement with a rapidly changing mainstream economics. He argues that the theory of the

EJPE'S NOTE: This interview was conducted by Thomas Wells, co-editor of the *Erasmus Journal for Philosophy and Economics*.

individual is "the central philosophical issue in economics" and discusses his extensive contributions to the issue.

EJPE: ***Professor Davis, you are unusual in having PhDs in both philosophy and economics. Is there a story behind that? How do you manage your identities as philosopher and economist?***

JONH DAVIS: Like many people, much of the story of how I happened to do what I have done was the result of the chances of life. I began in philosophy at the University of Illinois after dismissing my adolescent assumption that I would be a lawyer, but found as I moved to complete the degree that the job market was very poor and that my prospects for teaching philosophy anywhere were not good. At the same time, though I came from a suburban Chicago solidly middle class Republican background, I was radicalized in the 1970s by the Vietnam War, and decided that philosophy was too ivory tower and that economics (whatever that was) mattered. So before I finished my philosophy thesis I started at the University of Michigan in economics. But my first micro course with Hal Varian, where solving problems was more important than interpreting them, quickly demonstrated to me that I had to get my comparative advantage straight. That turned out to be the connection between the history of philosophy (one of my fields at Illinois; ethics was the other) and the history of economics.

Up the road was Michigan State University, where they then had four historians of economics and multiple courses in the field. Moving there, I was able to finish both degrees, finishing my philosophy dissertation while I was studying for my economics prelims. Youth has its advantages! That I was able to do both degrees, I think, was in good part due to the low cost of living and teaching assistant income then for graduate students at public universities (though mentors were also very important). One could survive, and even raise a family, while studying most of the time. That world, unfortunately, is now long gone, at least in the United States, where most people must indenture themselves to lenders to pursue advanced study.

I have managed my two identities by following a particular career pathway. I do not specialize or publish in the professional philosophy literature but concentrate on the history and methodology of economics literature. Partly this has been strategic: it is difficult to write and be successful in publishing if one has to communicate with two rather disparate audiences at the same time, and even philosophers of economics tend to look at issues quite differently from methodologists of economics. Partly it has been because I thought economics more important for what happens in the world. Still, this choice placed me in two small subfields in economics (history and methodology) which also do not communicate very well. Nonetheless, I have always thought the philosophical or methodological dimensions of the history of economics a fertile intellectual domain (as have others in the history of economics: Smith, Marx, Keynes, and Sen, for example). Whether this kind of strategy is workable in the future is hard to say. Without the current system of secure long-term employment in academia, which may be endangered, the forces for 'homogenizing' research that discourage interdisciplinary niche research may be too strong.

Also on the subject of identities, I have taught in economics my whole career. This has meant I have learned to think like an economist, where one moves step by step in a fairly linear way, which is quite different from thinking like a philosopher, where rival foundational assumptions are always being juggled and traded-off against one another so that the whole explanatory picture can transmutate before one's eyes with a small change in assumptions.

I like both types of thinking, but there is something to be said for focusing on the explanatory task economists see themselves addressing for grasping the logic of economic thinking. Some philosophers and methodologists of economics, in my view, fail in this regard. They come forward with good philosophical arguments, but they do not quite get at what the issues are for economists. So I was opposed to the idea, floated a number of years ago in the history of thought community—and the subject of a 1992 *History of Political Economy* symposium responding to the 'breaking away' proposal of Margaret Schabas (1992)—that historians of economics ought to migrate away from economics to find homes in history and philosophy of science programs. Teaching economics and having economics colleagues is in my view important for properly understanding the philosophical and methodological issues in economics.

I think you are also unusual in the range of areas you have published on, from the history and philosophy of economics to recent history of economics to heterodox economics (especially social economics) to identity. How do these link together, if they do? How have your

interests evolved? Would you recommend this approach to anyone else?

I think there are more people than one might think who maintain multiple research programs, even in quite different subjects. Often there are links that one discovers in a natural way as one just happens on connections between things. But researchers can also have different interests without quite knowing how they connect. Or the connection is a somewhat path-dependent product of one's history of interaction with other researchers (often at conferences, outside of sessions), whose work strikes one as interesting, and who suggest ideas and ask critical questions.

Broadly speaking, the reason for my attachment to heterodox economics—aside from my politics—goes back to my philosophy training. Philosophy, with its attention to conceptual depth and the multiple interconnections between ideas, naturally invites one to ask whether tightly defined behavioral relationships, as in utility maximization analysis and competitive market theory, are not dependent on a host of underlying assumptions and ideas regarding institutions, norms, social values, and so on, that lie behind these behavioral relationships. So one (methodological) definition of heterodox economics—one not used by many it seems—is that it is an approach that insists on going beyond surface explanations to more holistic, in-depth explanations.

Mainstream economics says this is unnecessary on the grounds that the more immediate analysis/model sufficiently communicates cause-and-effect relationships. Heterodox economists reject that, and indeed argue that a tight logic can be wrong or misunderstood absent an appreciation of what the analysis/model more deeply presupposes. This makes the difference between mainstream and heterodox economics less a matter of content and politics and more a matter of different philosophies of science. I think the recent financial crisis demonstrates that the heterodox approach to science in terms of conceptual depth is better. But the surface model of science that dominates the mainstream is well-entrenched (perhaps reflective of the strong influence of American culture on science). This all ties in also to the mathematization of economics and the expulsion of narrative from scientific explanation, as associated with the recent decline of history of economics and economic history in economics departments.

My long involvement with social economics (particularly in eighteen years of editing the *Review of Social Economy*)—which led me to work on the individual and identity—derives from a combination of philosophical curiosity and a preference for a heterodox economics that emphasizes social values. My training in ethics and philosophy of science made it clear to me from early on that science is always value-laden. When you see the world both in-depth and as pervaded by value you find you need to think of individuals as social and not atomistic. This all led me to what I saw as the central dilemma in the theory of the individual—how a person can be social and individual at the same time—which I tried to work out for myself by formulating two identity conditions for what individuals are which enable us to formulate social and relational conceptions of individuals.

There was another important influence on my thinking about the individual in economics. When I first began working on the individual I could not get over the ideological character of the *Homo economicus* conception, i.e., that it was not just a benign tool of economic analysis but figured centrally in liberal society's vision of itself and economics' one-sided promotion of that vision. One of the things I learned from Warren Samuels was that economists commonly put themselves in service to 'mythic devices', as he put it. Thus for him an important task of methodological analysis in economics was to ferret out these attachments and expose them to fair analysis. In his late life work he performed a similar kind of analysis of the invisible hand, arguing in his *Erasing the invisible hand* (2011) that this idea central to economics is a largely ideological one that functions as a 'psychic balm' and means of social control. So I think Warren disposed me to looking critically beneath the so-called 'scientific' surface of economic ideas to the silent work they often perform. Indeed, the invisible hand and *Homo economicus* seem to occupy coordinate roles in this regard.

Regarding moving back and forth across multiple personal research programs, I recommend this for a number of reasons. There is the practical matter of diversifying one's credentials. I think it is also intellectually more satisfying, particularly over a long work life, to investigate many things. One should be open to where research takes you, since unanticipated subjects of investigation often drive one to develop new ideas, and one does not want to foreclose these possibilities at the cost of one's research becoming repetitive and tedious. I was thus fortunate to teach for ten years at the University of Amsterdam, since this gave me the opportunity to do something new, namely investigate new research programs in economics from an historical perspective—something which cannot be separated, I should add, from being able to work with the interesting and talented colleagues I had there.

Moving to particular themes from your work. How would you describe your approach to the history of economic thought?

The Amsterdam group in the History and Methodology of Economics (HME)—which was closed in early 2011 (more on this below)—argued that the history and methodology of economics are inseparable, a view not shared by that many historians and methodologists of economics, who tend to be fully specialized in one or the other field. Indeed some in the history of economics community do not hesitate to say that methodology/philosophy of economics type arguments have no place in history of economics journals, and for this reason submissions to history of economics journals that identify philosophical arguments in the history of economics are sometimes rejected without serious review.

Why do many historians hold this separability view? I think the answer is connected to a change in recent years in the way the history of economics is done (something I think shows up when you compare contemporary historians with the generation who founded the *History of Political Economy*). The history of economics used to be practiced as the history of economic *thought*, where this was seen as the study of theory, ideas, and economic doctrine. In the reaction against this approach (beginning perhaps in the 1990s), historians of economics increasingly argued that their job was not to explicate and evaluate different doctrinal positions, but that it *was* their job to describe how economic views were developed by their proponents. Contributing to this view at the time were two developments: (1) History of Economics Society conferences in the 1980s and 1990s were often the site of contests between rival heterodox economists, and (2) many methodologists of economics adopted sociology of scientific knowledge views which described how economists/scientists behaved and happened to come about their views rather than what the rational content of those views might be. The first development generated professional concern that the history of economics was not a legitimate subfield—at a time when the economics profession was already skeptical about its value—while the second development provided a model of scholarship which was neutral regarding the status of economic doctrine (if not also positivistic).

The outcome of this was that by the end of the millennium historians had made archival work foundational to the practice of the field—something that was rarely done previously. 'New' evidence was not surprisingly a fairly solid route to publication (in a time when pressure to publish was being extended to historians of economics), and perhaps more respectable in the eyes of economists generally. And perhaps there were also diminishing returns by then in a fairly mature history of economics community to further doctrinal analysis. In any event, at least in my view, a kind of historiographic positivism became characteristic of much work in the history of economics, and this made—as a not entirely unintended consequence—methodological/ philosophical reasoning regarding the history of economics relatively unwelcome in the field.

Of course it would be wrong to say that archival work (which I have done as well) is not valuable, just as it would be wrong to say that published materials are never sufficient for understanding the ideas in question. The immediate issue is rather the practice of excluding philosophical and methodological reasoning from the history of economics; the longer term issue is whether history of economics becomes impoverished when it avoids philosophical argument.

I will not enter here into the general arguments in favor of the view (defended at Amsterdam) that history and methodology/philosophy of economics are inseparable—though I think they are compelling once one looks at the issue—but rather comment on why I personally hold this view. It comes from my being trained in philosophy prior to being trained in economics. Essentially I believe philosophical positions underlie all positions in economic theory and practice, and the view that the former (if acknowledged) can be bracketed off from the latter seems to me mistaken. This is not to say that one cannot focus on economics and its history without raising methodological and philosophical issues. Of course one can. Rather, one just does not get down to the key foundations for the views people have when one stops short of the deep conceptual commitments they assume (knowingly or not). So if many people prefer to stop short in this way, this seems to me to be a nice argument for having some people specialized in methodology and philosophy of economics, as we have now. The latter just should not be excluded from the history journals.

At Amsterdam, the HME group also emphasized (though not exclusively) the history of recent economics, namely the second half of the sixty year postwar period when, after 1980, new research programs began to appear in the field. We saw this as an important extension of the history of economics, both in time coverage and historiographically speaking. Regarding the latter point, an important difference about the recent history of economics is that the story remains significantly open, unlike the earlier history of economics, where historical episodes are largely complete in the sense that old ideas have been replaced by new ones in current practice. We used this difference at Amsterdam to argue that one needs economic methodology to understand unfinished histories, because it provides grounds for assessing the merits of research programs. This, it should be added, is a different historiographic procedure than usually employed with completed (albeit interpretively open) histories of economics, because there we tend to put aside their epistemic and ontological credentials, simply charting why some programs prospered and others did not. The fact that history goes one way or another is important, but the window that methodology/philosophy opens on history has its own analytical advantages that historians risk not appreciating.

A corollary of this view is that practitioner economists in the current contested terrain of competing research programs in economics also think in methodological terms, albeit not in the professionalized language of economic methodologists and philosophers. Since history has yet to separate the winners from the losers, practitioners are not reluctant to defend their views in general methodological terms (as in the extensive debate over the merits and methods of experimentalism). An inadvertent consequence, then, of the de-emphasis of philosophy and methodology of economics in the history of economics is a general lack of interest in the recent history of economics. This, I suggest, may have two unfortunate effects on the history of economics as a field: it may make the field even more remote for economists generally; and it tends to leave historians of economics rather ignorant about the current changes in economic methods and theory. Imagine that in the not-so-distant future economists look back and wonder about how economics evolved at the end of the twentieth century. As things stand now, they are unlikely to receive much assistance from current historians of economics who by and large seem to be waiting until the story is fully over (though there are important exceptions).

Is it fair to say that Keynes is a central figure for you? Why is that?

Keynes was a central figure for me early in my career, seen as a philosopher-economist and as an inheritor of (a much revised) classical political economy devoted to understanding the economy as a whole. At least this was an assumption of my training in the history of economics at Michigan State University (under John P. Henderson, who wrote a comprehensive intellectual biography of David Ricardo—the subject of my dissertation—and who was active for many years in the History of Economics Society).

But that I worked on Keynes (rather than Ricardo) came by way of an accident. I was assigned at the 1987 Cambridge (MA) HES conference to discuss a paper by Suzanne Helburn on Keynes's unpublished early Apostles papers, written under the influence of the philosopher G. E. Moore. I was surprised to find that I basically knew what those papers were about in virtue of my having studied Moore and the early twentieth century meta-ethics tradition in Anglo-American philosophy in my philosophy training. I also knew what the critiques were that had developed within philosophy regarding this tradition, and concluded that Keynes had lived long enough to have known what they were too. This meant to me that he had probably modified or abandoned many of his early views, including those from about the same time in his *Treatise on probability* (Keynes 1921)—whose underlying epistemology had also subsequently been soundly criticized by philosophers—particularly as the philosophical assumptions in his later economics were so different. Since the standing view at the time the Apostles papers emerged was that Keynes's later thinking about uncertainty flowed from the *Treatise on probability*, I believed the story had to be retold, which I did in my book *Keynes's philosophical development* (Davis 1994), basically in order to rescue Keynes's economic thinking from association with faulty philosophical positions I believed it could be shown he had rejected.

Separately from all this, I also believed that Keynes was essentially correct in his diagnosis and analysis of mixed capitalist market economies, and that the post-Keynesian research program with its particular emphasis on finance and uncertainty is superior to more standard contemporary macro reasoning. So my views as an economist interacted with my views as a philosopher and historian of economics.

Though my economics PhD and first *HOPE* publication were on Ricardo, following Henderson's lead and my original interest in classical political economy I was not much interested in the philosophical aspects of Ricardo's work, and so this focus died. I guess he was not enough of a philosopher-economist to sustain my interest. In an indirect way, however, my work on Ricardo got rehabilitated in a number of papers I wrote on Piero Sraffa, who reintroduced Ricardo's thinking as a rehabilitation of classical economics and a critique of neoclassical economics. In fact my experience was similar to what happened to me with Keynes. When I first worked on Sraffa's 1926 *Economic Journal* paper criticizing Alfred Marshalls's laws of returns analysis, I saw that the critique Sraffa was generally believed to have delivered against Ludwig Wittgenstein's early *Tractatus* was entirely parallel to Sraffa's critique of Marshall and neoclassical economics of about the same time (Davis 1988). So it was again clues from the history of philosophy that led my investigation and my writing in the history of economics. I subsequently wrote on a number of links between Sraffa, Keynes, and Wittgenstein (Davis 1996; 1998; 2002), assuming that their philosophical positions were what were ultimately at issue. Most recently I have a paper rethinking the Sraffa-Wittgenstein relationship based on new information from the Sraffa archive (and also from Wittgenstein's letters) about Sraffa's attachment to the anti-logical positivist physicalism view of early twentieth century philosophy of science (Davis, forthcoming).

Many commentators and critics still talk about mainstream economics in terms of a single dominant ('hegemonic') neoclassical school, but you argue that this is actually out of date, e.g., in "The turn in recent economics and return of orthodoxy" (Davis 2008).

My take on this, as I argued in the 2008 paper, comes from taking the long view on the history of economics. I think anyone who studies the history of economics must come to the conclusion that paradigms do not last forever, and new dominant paradigms are substantially different from old dominant ones. The idea that history does not really change things, or that there is some kind of eternal recurrence of mainstream theories, strikes me as being without any basis historically, though these kinds of views are popular in economics, including among heterodox economists (most of whom are not historians of economics). Further, if we use (sociology of scientific knowledge) reflexivity reasoning that invites us to ask what drives our own behavior, we must note that our lifetimes are short, and if through much of our careers things have not changed much (the thirty years of the first half of postwar economics), it is natural for us to infer that there is no change in economics.

Of course it is not hard to make such continuity arguments about postwar economics if one selects broad enough themes. To be fair, my own view that there is significant change in economics is also subject to criticism in terms of what I focus on. So I doubt this debate is going to be easily resolved (maybe not until more time has passed), and how it is waged will depend on how people understand the details. For example, no one denies that experimentalism is something new in economics (like econometrics was decades ago). So if things are still the same in economics today as circa 1970, one must show that the thousands of experiments that have been done over the last several decades only confirmed for the profession past theories and doctrines, and have not impacted mainstream economics in any significant way. Many economists, including mainstream economists, would dispute that. My impression, then, is that people making the argument that things are the same have not really looked at what is going on in experimentation in relation to standard theory (for example, in regard to ultimatum games and the public goods voluntary contribution game). According to Vernon Smith, who has a pretty good handle on the history of experimentation and is surely in the mainstream, standard theory, especially rational choice theory, has been largely shown not to be empirically supported (Smith 2010). Many experimentalists share his view. So how economics is changing, if it is, I think needs to be more carefully investigated.

One of the problems for heterodox economists in this regard, I should add, is that since they often emphasize their differences from mainstream theory (which is reasonable given the latter's dominance in economics), a changing mainstream makes for a moving target. This is reflected in the rise of behavioral economics: what we are to make of it for the overall development of economics is yet unclear, especially with rival behavioral views (the "old" Simon plus computation approach versus the "new" Kahneman-Tversky approach). Further, what is going on in the mainstream is very fine-grained, as for example in the extensive debate about the nature of motivation (post the simple self-interest hypothesis). So this means there is a considerable research burden for heterodox economists (and historians of economics) in terms of what they need to review to form judgments about the current state of economics. But people's own research programs usually crowd this out. My guess, then, is that there are generational issues in training here. Scholars tomorrow, historians, and heterodox economists, will be simply better able to judge these questions about the state of economics because they will have grown up in the middle of these debates.

Does this have implications for economic methodology?

Very much so. The past history of economic methodology, with the critique of logical positivism, Popper, Kuhn, Lakatos, sociology of scientific knowledge, and so on, was very much a general philosophy of science approach applied to economics. Needless to say this was of little interest to practicing economists, and accordingly probably served to isolate and marginalize the field of methodology. Economic methodology now is quite different in its focus on the epistemological, ontological, and normative commitments underlying new research methods in economics (as reflected in what gets published in the *Journal of Economic Methodology*). So it is much closer to economic practice than it was before, but this also makes it hard to say what economic methodology is about, since there are so many threads and issues. As one example, agent-based modeling, as in Alan Kirman's (2011) work, attempts to explain markets as somehow 'self-organizing' rather than being ordered in a traditional micro-foundational way. Thus, one methodological issue is what are the epistemic credentials of the concept of self-organization as compared to those of the traditional foundations idea? There are many new questions of this sort in recent economics.

Within heterodox economics you have been particularly involved with 'social economics' (e.g., as an editor of the Review of Social Economy and president of the Association for Social Economics). What is social economics? Is it a school of heterodox economics, like Marxian or post-Keynesian economics, or something more like a movement?

Social economics is a school of heterodox economics, not only in light of the characterization of heterodox economics I give above, but also because of its rejection of the fact-value distinction embraced by orthodox (and some heterodox) economists. For social economists, both our thinking and the economy are irreducibly value-laden. Sometimes we can reasonably put value associations aside, but many times we do so at our peril. In addition, social economics is pluralistic with respect to values in economics. Whereas mainstream economics is explicitly welfarist (and implicitly libertarian), social economics recognizes equity, justice, fairness, dignity, human rights, responsibility, and the like—the full gamut of human normative concerns—as involved in economic life. So social economists reject the view that there is a distinct economic domain of life in which other values are not involved, and argues that the mainstream view that the economic domain is separate and distinct is just a means of promoting one system of values at the expense of others.

In addition, social economics is the economics of forms of social organization distinct from the market and state associated with the cooperative non-profit sector. This sector is in fact amazingly large and diverse, but remarkably it is little studied by economists, even hardly recognized, though it can be argued that both the market and state depend upon it in a variety of ways. In my view, the profession's overlooking of the social economy is due to a long history of ideological debate over the relative merits of market and state. This may change with new currents in recent economics, since one of the main findings of experimental and behavioral research is that people often cooperate, and do so on account of how their local interaction is organized.

One major ambition of the Association for Social Economics, then, is to convey its view that values matter in economics, and contest the fact-value distinction. Unfortunately there is not a lot of reason to be optimistic here, since the positivistic view that economics is a value-free science is very strong among economists and in society's desired view of economics and science. On the other hand, since much current behavioral and experimental research is now devoted to investigating 'pro-social' motives for behavior and coordination problems, mainstream economists may be moving toward allowing that the economy is not value-free even if they continue to believe that economics is value-free!

You have mentioned the fact-value dichotomy several times. There seem to be three distinct ways of understanding the fact-value distinction in economics, though they are much entangled in practice

and in the rhetoric of heterodox economists: metaphysical, normative, and methodological. Firstly there is the metaphysical 'dichotomy' (which Hilary Putnam has criticized so effectively, e.g., Putnam 2002) associated with the now somewhat anachronistic philosophical position of positivism which claimed that facts and values were of quite different kinds and only empirical facts (and deductions) could count as knowledge. Secondly there is the normative proposition that economists should stick to empirical and formal analysis because the pursuit of objective truth, rather than ethical analysis, ideology, or activism, is what proper scientists do. Hence it is wrong (a failure of professional ethics) to insert one's value judgments into one's technical analysis. And thirdly there is the methodological position that values do not matter for economic life and therefore for economic analysis.

My question is what do you think of the normative interpretation of the fact-value distinction? Are there not good reasons for economists to have a professional identity as scientists rather than as ideologues, moralists, or activists? For example in allowing easier communication and debate between economists, and making their policy claims and advice to governments and the public more trustworthy.

I think there are two propositions operating in the normative interpretation you offer. First, there is the proposition that economists and scientists should not be ideologues, and should not inject their value judgments into their work. Second, there is the proposition that empirical and formal analysis are a domain of objective truth, where objectivity is a matter of being value-free. The second proposition is in my view a species of what Putnam rightfully complains about. When I argue that ordinary scientific discourse is value-laden, I reject both the idea that there is an objective domain of investigation that is fully value-free and the idea that the value domain itself is not objective.

The claim that the value domain is subjective derives from the 1930s logical positivist doctrine regarding values, namely, emotivism. That in turn draws on Hume's old is-ought dichotomy, which many have argued is a false dichotomy in that there are many 'is' statements which smuggle in 'ought' statements (Myrdal 1953; Boumans and Davis 2010, 173ff.). For example, it appears that statements using the concept of equilibrium are value-free, but it can also well be argued that explaining the market system in terms of some natural balance idea rather than in terms of social conflict and power is ideological, thus value-laden. I do not say *all* economic ideas are *significantly* value-laden. But many of them, including some of the most fundamental, do imply or subscribe to various values about how we ought to see the world, even if the statements using them neither employ ought language nor point clearly to implied values.

This puts the first of the two propositions I distinguished above in a different light. I agree that economists and scientists should not be ideologues, but I interpret behaving in this way as a matter of denying and concealing the values they hold under the banner of objectivity and the Humean dichotomy. Most economists, for example, are strongly pro-individual freedom. Freedom is obviously an important value, but why pretend that an economics that makes it central (often in such a way as to exclude other values such as equity and justice) is not employing that value? So objective science for me is about being clear about your values. I regard policy-makers as trustworthy when I feel they are open about their value agenda. Again, to be clear, not everything in economics turns on values, so there is much empirical and formal analysis which can be engaged in by economists who have quite different values. Generally speaking, my view of objective science is a pluralist science in which different views over what we value interact with our investigation of the way the world works in a causal sense.

You have become very interested in another heterodox school of economics that many will find surprising to be classified as economics at all: the capability approach. Firstly, could you explain why you see it as a school of economics, and second, what lies behind your particular interest in it?

I find it paradoxical that so many economists see the capability approach as outside economics. That is due, I believe, to the hegemonic dominance of welfare theory in economics with its utilitarian individual basis. But in all other regards the capability approach is very standard. It is about resources, choice, economic growth, and well-being and freedom. Its dismissal by much of the profession thus demonstrates to me the unacknowledged power of welfarism, and utilitarianism generally, as philosophical assumptions. The capability approach has an entirely different view of what a person is—a deliberating, active being—whereas mainstream economics operates with a nineteenth century mechanical psychology view.

Unlike many people I am fairly optimistic about the future for the capability approach. Its view of the person and human development resonates with what I believe people today generally think, that is, that people can develop their capabilities over their lifetimes. Given, however, that the economics profession is so locked in and path-dependent in its commitment to the welfarist-utilitarian view, we should not expect it to significantly embrace the capabilities approach in the near future. Assuming, then, that the capability approach continues to be employed by other social scientists concerned with human development, it seems we should expect considerable schizophrenia in economic social policy deep into the 21st century.

As for my own interest in the capability approach, as is clear from what I say above, it derives from my interest in the theory of the individual, which I regard as the central philosophical issue in economics.

You are well known for your particular interest in what may seem an obscure issue in economic theory—personal identity. Could you explain why economists should take identity seriously?

There are two reasons for my concern with individuals and their identity, one historical and one scientific. Historically, there is no obvious reason to think, from the record of humanity, that individual people count for anything in their constant slaughter and terrible abuse over thousands of years in the name of 'higher' causes. But despite this history people around the world seem to believe individuals *are* important (an important expression of which is the pervasive desire for democracy). I think this is a fundamental historical discovery about human life, made over the last several centuries, that today we often take for granted but which needs much more thought. It begins with asking what an individual is, or what personal identity consists in. Unless you are offering a religious answer to this question (which I am afraid may be one underlying basis for *Homo economicus* in the analogy between the doctrine of the human soul and an atomistic individual), I think one finds this one of the most difficult questions to answer.

Scientifically speaking, on the other hand, what economics offers us regarding explaining the individual, despite its reputation as being the one social science that is about individuals, is not very helpful, since it assumes without scrutiny an essentially ideological view—that people are independent and untrammeled in their exercise of choice. While economics has generally been good at examining the exogeneity-endogeneity logic of economic processes, it has nonetheless failed to investigate the degree of endogeneity (or boundedness) of individuality itself. Nor does it even have plausible grounds for supposing that individuality is exogenous. The *Homo economicus* preferences conception of the individual, as I emphasize in my recent (2011) book, is circular, meaning that it assumes individuality—a person is defined as a collection of their *own* preferences—in order to say that the person thus understood *is* an individual.

So if economists take a concern for individuals as a central historical value underlying their work—a normative individualism—and take that seriously, then their scientific work requires that they explain the nature and influence of individuality on the economy better than they do. For me, whether economists are able to do this will be a crucial test of the relevance of economics as a discipline in this century.

Is it true that economics is about individuals rather than individual choice? Some might say (for example, Teschl 2011, 75) that in representing individuals as unique preference orderings economists are merely constructing an abstract model for use in studying rational choice, and do not intend that model to be taken seriously as an account of what people are.

There are two problems with this view in my opinion. First, the formalist, anti-realist impulse it serves tells us that whether economics has any connection to the world is irrelevant. I do not believe people who advocate this view actually think this, so their problem seems to be that they have not thought out the whole range of issues associated with explaining how economics connects to the world. Second, if a formal model of choice can be applied to any and every candidate agent (single person, group of people, part of a person, animal, machine, and so on), why should we believe it applies to any in particular? That is, the formal model of choice is essentially indiscriminate and so is in no position to make any ontological claims.

You brought out a well-received book on how orthodox and heterodox economics conceive of the identity of economic agents in 2003. Last year you published a new book that seemed to go much further in proposing how mainstream economics should think about identity.

Could you outline how you now think about identity in economics, and what economists should do about it?

The 2003 book, *The theory of the individual in economics*, contrasted the standard un-embedded *Homo economicus* individual with individuals seen as socially embedded to examine whether a person could be both socially embedded and individual. Traditionally, heterodox economics was about groups and not individuals, and neoclassical economics about individuals and not groups. I thought this was a false dichotomy: neoclassical economics does not succeed in showing that an atomistic being is an individual and heterodox economics actually has grounds for saying that people have a sort of individual autonomy, albeit one that depends on their relations to others.

What drove the argument were two ontological criteria of identity (individuation and reidentification through change), implied by the concept of an individual, which I used to evaluate different conceptions of the individual. Failing those criteria means that one does not have a conception of the individual that can be said to refer to real world individuals. I argued that *Homo economicus* fails both criteria and that most heterodox conceptions can satisfy the individuation criterion but not the reidentification criterion. The latter matter has not been adequately worked out by heterodox economists, in my view. But the 2003 book only evaluated neoclassical mainstream economics. So the 2011 book, *Individuals and identity in economics*, evaluates the conceptions of the individual in the new research programs in economics (behavioral, experimental, game theory, evolutionary, and so forth). It also tries to go further than the previous book to set out a capabilities conception of the individual that satisfies both criteria, and thus tells us what personal identity consists in (at least in economics). The argument of the book progressively assembles what this involves. It starts by emphasizing, through critique of behavioral economics and game theory views of the individual, how individuality depends on relations to others. It then puts this into evolutionary terms with a role for learning. Here I draw on Herbert Simon and the idea of self-organization (Simon 1955; 1956). Finally, it frames this relational-evolutionary conception in terms of capabilities (for a capabilities conception of the person); includes social identities among a person's capabilities; and then defines personal identity as a special capability one may (or may not) develop for maintaining a changing narrative one keeps of oneself with the help of others.

Given the philosophical character of the book, my ambition for it is modest. At the very least I hope that the issue of what individuality involves becomes an issue in economics, and that economists recognize that taking individuality as exogenous is unscientific and not in keeping with their standard method of asking what happens when something previously thought exogenous is re-conceived as having determinants within one's analysis. One way I think this might begin to come about is through an examination of the social identity-personal identity connection. People's social identities change over their lifetimes, and change who they are. So choices people make in this regard reverberate back upon their future choices, showing endogeneity in individuality.

I discovered in reading your CV that you have been involved in the nominations process for the Economics Nobel Prize. That sounds tremendously exciting. Could you say something about why you were selected, what it involved, and what new insights or perspectives this gave you on the prize?

I became a nominator when I began at the University of Amsterdam in 2002. I don't know why I was selected—though I assume it had to do with the long standing European respect for the University of Amsterdam in the history of economic thought and economic methodology, going back to the original chair of Johannes Klant, and through Mary Morgan and Mark Blaug. I am not involved in later rounds of vetting individual candidates, which plays an important role in the determination of the Prize. But I was struck from the beginning by the nature of the nomination itself: one is asked to give a one sentence statement of the "discoveries, inventions, and improvements" of the nominee(s), and then add a longer statement explaining this.

As an historian of economics and methodologist, an emphasis on originality seems to me naïve for a number of reasons. In any event, for many years I nominated Mark Blaug, arguing that he had 'created' the field of economic methodology (though of course there was methodological reasoning long before Blaug), which I took to be an invention and improvement for economics *par excellence*. Of course this is likely not quite what the Nobel committee means by "discoveries, inventions, and improvements", and had Mark received the prize (which I genuinely believed he deserved), I and everyone else would have been astounded, given the general disrespect for history of economics and methodology in the profession. Nonetheless I thought the case should be made both for Mark and for the history of economics.

As well as a substantial publication record, you have served in a number of institutions (journal editorships and associations) associated with your research interests. What is your view of the health of the institutions of philosophy and economics? Do they benefit or lose from their inter-disciplinary orientation?

I think the trend in general is clear regarding interdisciplinarity in economics and science: there will be more of it. The natural sciences are significantly ahead of the social sciences in this regard, but people who think institutionally about the long term strategies for the development of knowledge and science in foundations, universities, and government fully recognize this trend and generally support it. One might say that well established disciplines tend to exhibit diminishing returns to doing the same thing, and that the real gains are from going beyond identifiable disciplines. The good news in my view on this score is that all the new research programs in economics have important origins in other sciences. So the door is more open than it has been.

As for philosophy and economics in particular, it seems that there is considerable philosophical reasoning throughout science, though it is not always framed in terms of the issues and debates in philosophy itself. So increased interdisciplinarity could raise the profile of philosophy in economics. Perhaps it might be argued that in a world in which interdisciplinarity increases, philosophical thinking gains in importance as a broadly shared conceptual apparatus. I think this is a partial explanation for the rise and professionalization of methodology and philosophy of economics in the last several decades, and so I am optimistic on this score about economics and philosophy as a distinct domain of research. We now have well established journals in the field that have created space within which research can be done. That the *Erasmus Journal of Philosophy and Economics* has so quickly become successful (!) seems a reflection of this.

How has your own interdisciplinary work been received by the mainstream economics profession and the economics departments in which you work?

I have been fortunate at Marquette University where my colleagues have supported my research, though my department does mainly empirical research. It helps that I am genuinely interested in economics, represent myself as an economist, and am interested in my colleagues' empirical research and modeling intuitions as an instruction in economists' practice. I regularly tell myself I have been missing something when a colleague explains what he or she is trying to do in some piece of research. It is also worth saying that the Catholic mission of Marquette has made my type of research with its emphasis on social values and the dignity of the individual more acceptable than it might have been in a state-supported university. In fact I was hired at Marquette to replace a long-time member of the department, Peter Danner, who taught economics and ethics, as I continue to do. Finally, I am in a college of business, which means I work in an environment of different business fields, which might be argued to provide a more pluralistic environment.

For most of my time over ten years at the University of Amsterdam my research and that of my colleagues was strongly supported. (I taught three courses every second fall term, and took leave from Marquette.) Unfortunately over the last two years people in leadership positions there at the faculty of economics decided that the history and methodology of economics (HME) was not important, and in conditions of a financial emergency associated with chronic budget shortfalls closed down the HME group. That included sacking my very accomplished and, in our field, well-respected colleagues Marcel Boumans and Harro Maas, who had been associate professors there for many years, and ending the chair position in HME, which I held, which had been at the faculty for decades. We had six courses in the history and methodology of economics; engaged and enthusiastic students; a research group of up to a dozen people; a master degree in HME; PhD students; and a required methodology course for bachelor students. I do not think there was a better program in the world in our field. We also had great interaction with the London School of Economics, the history of economics people at Duke University, history of economics people in Paris, and the Erasmus Institute for Philosophy and Economics. The HME group was internationally recognized, and attracted students from across the world. Our financial footprint, in fact, was quite small compared to other groups, and by a number of measures of output per person we were more productive than many other research groups at Amsterdam.

Since I fully believe the faculty financial emergency could have been addressed without eliminating the group, I can only put what happened down to prejudice against our field, plus the usual on-going territorial aggrandizing that has been a key factor in the elimination of history of economics from most American universities. It is interesting to me also, that with a few exceptions, members of the economics faculty at Amsterdam made no effort on the HME group's behalf to resist what happened or even personally expressed regret or concern to those who lost their jobs. I find this reprehensible.

The loss of this program was a blow to our field. There are now few places in the world training PhD students in history and/or methodology of economics. So in the final analysis the situation for economics and philosophy is mixed: considerable achievement with an uncertain future. Great weight, in my view, should be placed on restoring PhD training in the field, something that is being done, for instance, through generous grants from the Institute for New Economic Thinking at Duke University under Bruce Caldwell.

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A review of Till Düppe's *The making of the economy: a phenomenology of economic science.* Plymouth: Lexington Books, 2011, 241 pp.

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As its subtitle indicates, this book is a reflection on the idea of economics as "science" from the vantage point of the phenomenological tradition in philosophy. Düppe asks a two sided question: what in the life-world[1] creates the opportunity for the expertise that might go by the name of "economic science"; and what interests have led would-be economists to respond. The book is very interesting from a number of vantage points within economics itself, giving depth and perspective to themes in the history of economics, to economic theory and methodology, and to contemporary conversations about what "economics" is (or is not). Philosophy, it turns out, can still yield useful insights, and this book yields an abundance of them.[2]

Düppe begins the book with Husserl's critique of modern science as an enterprise that forgets the primal, existentialist meaning-making character of thought/theory. He argues that in economics this forgetting happens through a 'formalism' (of theory) and a 'structuralism' (of economy). He then goes on, in the second part of the book, to perform a phenomenological scan (details to follow) on various chapters of the history of economic discourse, starting with the case of *oikovoμia* (economic life without "economics") and ending with the current state of affairs, in which he sees a dissolution of "economic science". Düppe approves of this dissolution, seeing in it a victory of the phenomenological instinct; seeing in it, that is, the possibility for

[1] The "life-world" is Edmund Husserl's term for the primal, existential conditions out of which humans make meaning. The life-world comes before meaning. The phenomenological tradition is thus different from any tradition (even hermeneutics) which would speak of meaning as inscribed in some pre-given situation: "[…] the life world is not the original world, but rather the originating world. […] The life-world is not the world that 'makes' sense, but it is the locus of the need to make sense" (p. 32).

[2] There are, of course, some points of contact between Düppe's approach and work in the philosophy/methodology of economics tradition. But whereas the latter inquires into what the standards of authority/validity might be, Düppe raises existential questions about the nature of, and need for, such standards.

economic discourse to return to the life-world, to the horizons of social policy and to the realm of existential meaning.

Central to Düppe's argument are two points. The first point is that the idea of "the economy" is totally internal to economic science. Far from having a natural correspondence in the world, the *idea* was created, in theoretically precise ways, in response less to the needs of citizens for something with which to make sense of their lives, and more to the need of economic science for an object of its own. The second point is that, for modern science, the loss of connection to the life-world (to the meaning *making* nature of thought) comes in the form of a claim to objectivity, in the form of the figure of the scientist as an overcoming, at the limit, of subjectivity, of human frailties. These two points intersect, and, in line with Husserl's philosophical transformation of the claim of objectivity into a charge of objectification, Düppe argues that the function of the idea of the economy has been not only to give an "object" to the science, but also, and most importantly, to guarantee the *ethos* (and *pathos*) of the economist as "scientist". As such, the economist/scientist is the figure who is above the ordinary interests and passions of economic agents; who is objective and calm; who is detached from "the world" except as something to purify into an object of knowledge; who is, or strives to be, above suspicion.

In the end, Düppe argues, both "the economy" and the economist qua scientist prove untenable. He sees evidence of an end to "economic science" in the lack of any exclusively "economic" axis shaping "research" in economics departments—a lack, a void, filled in by a panoply of themes/objects/questions taken from other fields: psychology, mathematics, history, philosophy, sociology, anthropology. For him, the proof of the dissolution of economics occurring under our very eyes lies in the fact that researchers so engaged could migrate from economics departments to departments in these other fields without any resulting loss of content for their specific research projects. Economic science dissolves in its own terms, under the imperative of an object (a research framework) it can no longer lay exclusive claim to.

To support his conclusion, Düppe constructs a narrative of the history of economics as an almost teleological process.[3] Economists,

[3] I write "almost" because Düppe is too sophisticated to claim some systemic necessity to the history of ideas (that would be in open conflict with phenomenology), and yet his narrative does give a certain substantive weight to the imperative of modern

once having imagined "the economy" in order to give their science an object (and themselves authority), but having also to obey the imperative to inoculate knowledge from the contaminations of interests and passions, of politics, culture, ideology, or even from the mere *suspicion* of such contamination, were led, by the force of this imperative, to empty their "object" ("the economy") of all substantive contents which could act as channels of contamination. The product of this evisceration of life from "the economy", Düppe argues, is the highly formalist, structured yet empty, concept of Debreuvian general equilibrium in which questions about the nature of economic agents and processes are muted.

Having thus come to lose all specific content, Düppe concludes, economics, as *science*, has no reason, indeed no right, to exist. He calls for an institutional sanctioning of the dissolution of economic science, counseling the "critics within" to call for the abolition of economics departments. The demolition of these houses of economic science would allow those so interested to leave behind the pathos of disinterest (objectivity) that the modernist ethos of science cultivated and to move on to being able to face questions of meaning and engage in discussions of interests.

At the end of this review, I will return to this question of the end of economic science and economics departments. The reader, however, first deserves a flavor of the intellectual yield of the book's phenomenological history. Düppe is quite rigorous in his philosophical refusal to take "economics" as a given, which is something almost impossible to do in any historical narrative written from "within" (any position "in") economics. His concerns, therefore, are not with analytical issues per-se, but with questions of the constitution and meaning of economics in society at large, and his insights here are valuable and refreshing.

In his chapter on non-market societies, Düppe explains the fundamental difference between "economy" and "*oikovoμia*" (the former embedded in a structural order which sublates the political, cultural, and social to the "economic"; the latter deployed in a temporal order which preserves those distinctions) in a way which adds significantly to the work of, e.g., Karl Polanyi, or Keith Tribe (though Düppe does not cite them). In his chapter on the rise of "the economy" in the

science to forget the life-world: "Forgetting the life-world […], according to Husserl, is the fate of modern science" (p. 36). There is a porous line between "fate" and "telos."

seventeenth and eighteenth centuries, Düppe puts the accent not on the development of trade per se, as standard histories do, but on the nature of the relationships of merchants (that historically mistrusted, suspicion-arousing group) to the cultural and political channels in the societies of those times. What potentiated the idea of the economy in Britain was not the extensive nature of trade patterns per se, but the fact that there—unlike, e.g., the case of France—merchants were not under political authority and were thus subject to "suspicion" (as a special interest group). It was in that place therefore that a need could be shaped, or even invented, for some standard, some place (*ethos*) from whence the suspicion could be contained. The budding anonymity of the economy offered the conditions for the erasure of that environment of suspicion. Thus, what gave the *Wealth of nations* its rhetorical power (and began the analytical project of "economics") was not Smith's philosophical/ analytical bent—important as that might be in other respects—but the cover of "disinterestedness" Smith's academic garb could provide.

After Smith the fortunes of the likes of Ricardo and Malthus, or Marx (or, for a yet later horizon, even Keynes) do not play much of a role in Düppe's narrative. I will return to this omission later; for now the point is that, for Düppe, the analytical work of these figures did not contribute to the separation of economic "science" from politics, but rather worked against this separation. What Düppe's phenomenological scan highlights instead, as the challenge for the ethos of modernist science over the course of the short century from 1846 to 1932,[4] was the new wave of suspicion about economics created by the popularization of political economy by the likes of Harriet Martineau and by the passionate political and philosophical pronouncements of various socialist traditions.

By 1932 such suspicion could again be muted by reference to Lionel Robbins's *Essay on the nature and significance of economic science*. The power of the *Essay* lay not so much in its definition of economics per se ("Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses"), but in the turn toward the de-substantiation of economics that it confirmed: "In hindsight, Robbins's essay was successful not because

[4] With the repeal of the Corn Laws in the UK, 1846 inaugurated the official doctrine of free trade. 1932 saw the publication of Lionel Robbins's *Essay on the nature and significance of economic science*.

economists came to an agreement about the scope of their discipline, but because this question began to lose relevance" (p. 137).[5]

Of the various skirmishes that have punctuated the history of economics, the one Düppe focuses on is the socialist calculation debate. This particular debate, according to him, encapsulates both the instinct of economic *discourse* to be politically relevant (by speaking about the nature of different economic systems) and the contrary instinct of "science" to run away from any substantive concept of economic processes. It was the latter instinct that was eventually to prevail. The fact that, throughout the debate, both socialism and capitalism were essentially conceived as two modes of a (single) structured economic rationality (the anarchic market or the controlled market—but an economy conceived as a structure of markets in either case) was not conducive to a discourse of difference.

The debate did present opportunities for addressing substantive matters of economic organization nonetheless; and a certain Friedrich Hayek even seized them with his conception of the market as *process* (not structure). But, alas, under the pressures of the Cold War, and the need to allay the suspicions that war again fomented, economics presented itself increasingly as a-political. The debate, Düppe argues, therefore eventually ceased to attract "economists". What instead became the vogue in economic science was a turn to formalism, presenting the market as a pure, mathematically formal system of prices, without references to actual agencies, market processes, and the like.

Gerald Debreu is the crucial figure in Düppe's narrative of the inclination toward discreetness which compelled economics first to invent "the economy" and then to empty it of all concreteness. The book presents a deep personality sketch of Debreu (drawn from the recollections of his daughter, Chantal Debreu, and from accounts of Debreu's stay at the Cowles Foundation after WWII) as a figure of extreme discreetness. Debreu, Düppe writes,

> did not share the hopes regarding *explanatory purposes* of general equilibrium theory that his work with Arrow had caused. An equilibrium, for him, had no referential meaning but was a condition of a consistent theory […]. "In proving existence one is not trying

[5] For Düppe, the key step toward this liberation of "science" from politics, which Robbins's *Essay* sanctioned, was John Stuart Mill's attempt to divorce the "science" of production from the "art" of distribution.

> to make a statement about the real world, one is trying to evaluate the model", he explained later in his life (p. 171, emphasis added).

Düppe also quotes Debreu on the reactions to general equilibrium (G.E.) of, on the one hand, liberal economists who saw in it proof of the invisible hand, and, on the other hand, Marxist economists who saw in it the impossible conditions G.E. required: "I simply took the following stance: You can derive whatever conclusions you want from the assumptions. If it satisfies liberal economists and Marxists, too: Perfect!" (p. 175). It would indeed seem that, for Debreu at least, the question of capitalism or socialism had passed! And Düppe sees this as symbolic *in toto* of the discreetness embedded in the genetic code of economic science.

And so, after the 1970s, we come to our own times, in which economics, having gained its rigor and lost its soul, limps along. Arguing against the positions of some, e.g., John Davis and David Colander, Düppe does not see in the turn to complexity, behavioralism, experimentalism, game theory, and so on, a resurgence of the possibilities for real science in economics. For him, referring to an orthodox/neo-classical tradition which no one (supposedly)[6] practices anymore is not, as it is for Davis and Colander, inappropriate, or anachronistic. Düppe agrees that the "orthodox/neo-classical" center is empty, but for him the emptiness is meaningful in itself, it being exactly what the turn to formalism accomplished, the moment of high theory in economics; a turn which, though not preordained, was always already waiting to happen.

What to make of this very interesting book? As I have already indicated, the phenomenological approach is very productive of insights, and the narrative both complements and engages critically with other histories of economics. That in itself makes the book a great occasion for reflection, learning, and teaching. I do have my doubts, however, regarding Düppe's conclusion about the end of economics (both real and desired). It is indeed desirable to look at economics from a certain phenomenological angle (questioning critically the *ethos* of the economist as scientist), and from that angle it might very well be that

[6] I say "supposedly" because I do not myself agree with this characterization. The turn to these research programs seems to me to be a rather domesticated turn, with the neoclassical postulates of maximizing agency (and resulting equilibrating market tendencies) still setting the parameters for the asking of questions and the interpretation of answers.

economic science has ended. But it is quite possible to look at matters from other angles as well (that of the policy maker, that of the public intellectual, that of the political agent, and so on, even through the phenomenological prisms particular to each of these), and to weave a tapestry of economics (even of economic science, I would suppose, if "science" is defined broadly enough) from an intersection of angles. From such an intersection, it might seem less certain that we are seeing, or should be calling for, either the end of "the economy" or the recourse to languages of science.

In suggesting a multiplicity of phenomenological angles and a less certain stance on where we are today, I am not, I think, far from Düppe's own horizons. Düppe opens the book with a description of a 16th century painting (*The tax collector*) by the Flemish painter Marinus van Reyemerswaele. He focuses on the role of a scribe who, recording entries in a book of accounts, finds himself uncomfortably close to the human drama playing itself out at the intersection of two gazes, that of a powerful tax collector on one side, and that of a supplicant debtor on the other (the one figure inspiring antipathy, the other sympathy, perhaps!).

The scribe's function is to write down, and thus to *represent*, the transaction. The scribe's own gaze is on his ledger: he is looking down. His "down-gaze", however, Düppe convincingly argues, is not a sign of self-absorption in a taxing task (pun intended); it is, rather, a sign of discreetness, as the scribe eschews a discomforting encounter with the gazes of the economic actors. For Düppe, the scribe is the symbol of what the economist is going to become: the *representer* of a structured chain of transactions and also, like the scribe of the 16th century, a down-gazer, hiding from the pains, passions, and interests of economic life, hiding behind the curtain of science—and getting authority from that stance.

All well and good! But the story of beginnings does not have to, and should not, end there. As it happens, in 1966 Michel Foucault also opened his own book on the origins of modernity, *Les mots et les choses* (*The order of things*), with a description of a painting, a 17th century painting in his case: *Las meninas* by Diego Velasquez. Like Düppe, Foucault was also concerned with the problem of representation. But Foucault's formulation of the problem was more open ended, more open to multiplicity, than Düppe's. In *Las meninas*, the field of representation, as Foucault explained laboriously, is not given by (the relations between) the represented and the representer. It is given, rather, by the very absence of the representer.

This difference in the field of *representation* is significant. The difference itself might be explained by the lapsing of a century between the work of van Reyemerswaele and that of Velasquez: in that lapse, one could surmise, art became interested less in the relationship between the *subject* representing and the *object* represented, and more in the activity of representation in itself. And, to return to Düppe, we could further surmise that the down-gaze of the scribe upon which he puts so much stake already represents the possibility, if not the inevitability, of the exit of the representer from the scene; the limit to infinity of the "discreetness" Düppe finds in the genome of the economic scientist. We can thus explain the exit of the representer from the scene that Foucault considered so central to modernity. But having explained this exit, we should also consider whether it has consequences. It does, I think. The absence of the representer marks the scene with meaning. It marks it, specifically, with possibility, as an empty space which is not yet occupied by one single point of representation and is thus open to multiple sources (and inspirations and methods) of representation.

The sources of economic science (and in parallel fashion, the sources of the idea of "the economy"), can be presumed to be many. There are imperatives other than, and in addition to, the phenomenological one of the "scientist" for the practice of economics and for the power-conferring concept of "the economy". I will mention, as an example, the work of Timothy Mitchell, who traces the idea of "the economy", in the 20th century (though not earlier) to the colonial relationship between the European and other peoples, and to projects of "development" (Mitchell 2000; 2002).

We could also talk about the imperative of the Keynesian project, or of the Marxian one (in any of their forms) to construct "the economy" as an object of policy (in this respect, Düppe's light treatment of Marx and Keynes is problematic).[7] Thus, even if we grant that a phenomenological reflection could call for the erasure of the concept of the economy, there are still other sources of meaning to take into

[7] Also problematic, perhaps even more so than the light treatment of Keynes, is Düppe's neglect of the Cambridge/capital controversy, and of the alternative theoretical space that controversy kept open for a conception of "the economy", e.g., Marxian (or Ricardian—the reader can pick) in which extra-economic forces are determinants of prices, and which is not structuralist and not conducive to formalism.

account before any settling of accounts could be declared. And this, without even taking into account the effects of the Great Recession on what popular movements and policy makers might, could, would (will?) ask of “the economy” and of economists. To vacate Economics Departments without taking the multiplicity of those latter imperatives into account seems unwise (though, admittedly, they made themselves felt after Düppe’s thoughts were formed and it might be unfair to have expected him to foresee them).

I am in full sympathy with the idea that economics is an ideology, and that humanity would benefit from the clipping of its wings. But the struggle for an end of economics and of economic science will play itself out outside of academia as well as within. As powerful as science and philosophy are, the powers they have—the former to propose and the latter to dispose—are not exclusive to them.

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Review of *Economics for real: Uskali Mäki and the place of truth in economics*, edited by Aki Lehtinen, Jaakko Kuorikoski, and Petri Ylikoski. Routledge, 2012, 282 pp.

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Uskali Mäki has been one of the major thinkers in economic methodology during the last decades and devoting a book to his philosophy of economics is a natural and appropriate step. Let me already at this stage admit that I am not really neutral about Mäki's work. On several occasions I have myself applied it to contemporary issues in economics. From my own experience I know that Mäki's ideas about realism, realisticness, isolation, commonsensibles, ontology, and so forth, have the potential to be successfully communicated to practicing economists, other social scientists and even politicians. This kind of potential is noteworthy and not common in economic methodology.

The editors have managed to put together a very interesting book (and, according to the preface, within a short time frame). The book is organized around the following four parts intended to cover Mäki's extensive philosophy of economics: isolating truth in economic models; the commonsensical basis of economics; the proper domain of economics; and rethinking realism(s). I will briefly discuss each of these parts and then conclude with some reflections on the contents and scope of the book.

Since no contribution by Mäki is included in the book, the introductory chapter plays a vital role. Aki Lehtinen provides a great overview of Mäki's work that succeeds in capturing and analyzing its dynamics and is worth reading on its own. He goes back to the early writings of Mäki in the beginning of the 1980s; covers the well-known work on isolation, commonsensibles, realisticness, and so on; and also addresses recent work such as MISS (Models as Isolations and Surrogate Systems). Lehtinen also claims that the book "should be of interest not only to philosophers of economics but also to social scientists and economists reflecting on the nature of their science" (p. 3). Obviously I agree that Mäki's work has this kind of potential. However to what extent the book fulfills this aim is something I will return to at the end of this review.

Part I of the book, "Isolating truth in economic models", contains five essays. Frank Hindriks analyzes how Mäki's 'significant-truth' strategy (a theory can express a significant truth even if it involves falsehoods) compromises the realist ideal of true theories. From the latter perspective, Hindriks instead favors either the 'future-truth' strategy (falsehoods are temporary and therefore unproblematic) or the 'truth-of-the-counterfactual' strategy (idealizations are not to be viewed as falsehoods but as antecedents of counterfactuals). Ilkka Niiniluoto examines the relation between Mäki's MISS, Sugden's "credible worlds" and the concept of verisimilitude. Daniel Hausman focuses on MISS, viewing it as "clearly a hit—maybe not a home run—but definitely not a strikeout or a foul ball" (p. 81).

The final two chapters are the most fascinating. Till Grüne-Yanoff distinguishes three kinds of isolation used by Mäki over the years—essential, formal and minimal—and convincingly connects them to different phases of Mäki's work. Jack Vromen investigates to what extent we can benefit from applying de-isolation and re-isolation when dealing with the dynamics of (theoretical) dispute and provides an intriguing case study of crowding-out versus non-standard price effects. Probably a matter of taste, but the analysis could perhaps gain by more fully adopting the formal framework proposed by Mäki (2004).

Part II, "The commonsensical basis of economics", contains two essays. Francesco Guala examines the realism of commonsensibles, focusing on rational choice theory as well as developments in behavioral economics. Mäki's commonsense realism is criticized as a philosophical position that tends towards behaviorism. Wade Hands looks specifically at the relation between the theory of revealed preferences and Mäki's views on commonsensibles. Interestingly, although the case study shows that the relation is problematic, in the end Hands's analysis offers support for Mäki's general work on realism in economics.

Part III, "The proper domain of economics", contains two essays. Don Ross criticizes Mäki's philosophy of economics, focusing on the interaction between economics, psychology, and neuroscience. He illuminates the differences between commonsensibles and the economic conception of choice. According to Ross, applying Mäki's philosophy of economics leads to a misidentification of the scope of economics. It is a provocative and interesting chapter. However, the argumentation is sometimes unnecessarily complicated and I am not convinced by its analysis and conclusions.

I agree with Ross that Mäki's views on neuroeconomics can be further developed, especially by developing relations with his earlier work. But Ross also argues that "the economic conception of choice, unlike the psychological conception, is an intrinsically abstract one that is not derived from a Mäkian commonsensible" (p. 191). This view of economic choice puzzles me. For me a typical economic choice involves strategic interaction in one way or another (although there are other kinds of choices as well). But cannot theories of strategic interaction be connected to commonsensibles? In the early 1980s more or less the whole population of Sweden was made aware of the basic issues in the development of cartels by watching the soap opera *Dallas*. In fact, I am quite confident that the theory of cartels in microeconomics is just rearrangements and modifications of those commonsense views. Regarding how choice is understood in economics compared to psychology, I also wonder how bridging approaches such as work on dual processes by Stanovich and West (2000), combining analytical and intuitive reasoning, would affect the analysis.

John Davis investigates the results of applying Mäki's ideas about economic expansionism and imperialism to recent developments in economics. Basically we get a blurry picture with different economics research programs drawing on other fields and disciplines. Some research programs are rivals, others are just parallel. We also have expansionism and imperialism within the discipline of economics. This is a rewarding chapter, and given the popularity of interdisciplinarity today this kind of analysis will be important in the future development of the philosophy of economics.

Part IV, "Rethinking realism(s)", contains three essays. Kevin Hoover's point of departure is the methodological views of econometricians. In order to understand this, Hoover suggests applying Giere's perspective realism (complemented with Pierce's pragmatism). Taking the methodological views of practicing economists as a starting point for analysis is an approach I find fascinating. Jesús Zamora-Bonilla takes on the rhetoric versus realism debate between Mäki and Deirdre McCloskey through a formal approach that represents it as a persuasion game. The last chapter is a favorite of mine. Jaakko Kuorikoski and Petri Ylikoski compare how Tony Lawson and Mäki approach realism. This is an exciting chapter, revealing the difficulties in applying critical realism but also clarifying its advantages. A part of me thinks it is too bad that the book could not include contributions by McCloskey and Lawson themselves. At the same time, one must be realistic (!), and these chapters do a good job.

Let me end with two reflections. The first concerns the absence of any contribution by Mäki himself. I think it is a good choice not to incorporate comments or any end chapter by the subject of a book since it can change the focus too much. But some interview or career reflecting chapter would have been nice, and we have some recent examples of that (e.g., Mäki 2008; 2009).

The second remark is that I would prefer to see more on the application of Mäki's work. With the exception of the chapters by Davis, Hands, and Vromen, there is a clear emphasis on philosophical perspectives in the book. Work like "Theoretical isolation in contract theory" by Kirsten Foss and Nicolai Foss (2000), the best example of applying Mäki's work in economic analysis that I know of, is invaluable in reaching out to reflecting economists (in that case, those interested in contract theory and entrepreneurship). Thus, although I believe that this book is very interesting and important, I do not believe it will succeed in reaching reflecting economists.

It would also have been interesting to address how Mäki's views about applying his work have developed over the years. In the realism debate with Hausman, Mäki once said: "my experience has been that not all the concepts and ideas needed can be found in philosophy; one has to make up a few" (Mäki 2000, 110). This quote was important to me when I started doing applied economic methodology. I interpreted it as saying that economic methodology was not just applying contemporary philosophy of science to economics. And that if you want to reach practicing economists some flexibility is preferable. But I am not sure if that quote and this interpretation still apply today.

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A review of Valeria Mosini's *Reassessing the paradigm of economics: bringing positive economics back into the normative framework*. London: Routledge, 2012, 164 pp.

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In this book, Valeria Mosini explores an interesting and daring thesis. She examines whether Milton Friedman's famous 1953 essay might not have had a solely methodological significance but may also have served a political purpose. The central claim of the book is that the methodological essay played a significant part in backing up Friedman's neoliberal policy prescriptions. His instrumentalist argument promoting the un-realisticness of assumptions and emphasizing prediction for testing theories, Mosini contends, effectively served to make his theoretical views immune to prior empirical scrutiny, and thus allowed Friedman to present what are essentially normative or political claims (his neo-liberal policy prescriptions) as a positive scientific paradigm for economics: "Friedman's methodological argument provided the neoliberal doctrine with the extra-bit that was required to turn it into a fully fledged paradigm awaiting implementation" (p. 4).

Mosini claims for instance that a number of Friedman's neoliberal prescriptions are insufficiently backed up by evidence that meets his own standards of scientificness. Even when Friedman's 'neoliberal' policy prescriptions did not live up to his own methodological standards, his methodological statements nonetheless served to lend them scientific credibility. Let me first briefly summarize how Mosini builds towards this claim in her book, before I note some of the major problems with her thesis.

In the first chapter, Mosini explores whether Friedman's neoliberal policy prescriptions actually abide by his own methodological standards. She does so by examining the logical consistency and validity of Friedman's *Essays in positive economics* (1953), which besides the famous essay on "The methodology of positive economics" also contains essays on economic policy. She first looks at whether the 'neoliberal' policy prescriptions contained in such essays as "The case for a flexible exchange rate" or "A monetary and fiscal framework for economic stability" were in fact arrived at in accordance with Friedman's own stated methodological views. Were Friedman's policy prescriptions based on theories whose predictions were anywhere tested, if they could be tested at all? Mosini shows that they were not.

Mosini further considers the validity of Friedman's claims and argues, for instance, that the essay "The effect of a full-employment policy on economic stability: a formal analysis", in which Friedman argues on the basis of a formal analysis that stabilization policy is in principle possible but likely not to be right in terms of timing or size, leaves much wanting in terms of both empirical evidence and even 'logical compellingness'.

She goes on to refer to the philosophical work by some of Friedman's contemporaries, such as Henry Margenau, Percy Bridgman and Stephen Toulmin, to suggest—not unsurprisingly—that Friedman also went against the view of the time which maintained that prediction cannot be a sufficient ground for evaluating theories. From these considerations about consistency and validity, Mosini then concludes that Friedman's policy prescriptions fail to live up to the methodological standards of positive science to make the bold claim that "given that Friedman must have been aware of the logical inconsistency that burdened *Essays*, his attribution of objectivity to the core theses of the neoliberal doctrine was based on false pretenses [sic]" (p. 34).

In the second chapter, Mosini moves to criticizing Friedman's positive-normative distinction by studying its historical origins. Even though Friedman opens the methodological essay with a quote from J. N. Keynes, Mosini claims that Friedman makes a mockery of Keynes' distinction. Mosini argues that whereas Friedman claims that positive economics could and should be pursued independently from normative economics, Keynes actually maintained that, especially with regard to policy advice, positive economics is always entangled in normative economics. This subversion of Keynes' distinction, Mosini contends, allowed Friedman to suggest that his positive economics is in fact free from normative or ideological considerations (p. 49).

Moreover, revisiting the work of contemporaries of Friedman, and also Alfred Marshall and especially Lionel Robbins, Mosini further highlights the singularity (and opportunism) of Friedman's use of the distinction. While for others the distinction mostly functions to argue that normative claims cannot be derived from positive statements alone, Friedman employs it to achieve exactly the opposite, which is to argue that it is indeed possible to offer policy advice based solely on positive economics which is supposedly purged of any normative consideration.

The third chapter is essentially concerned with a similar claim, but now Mosini draws on Léon Walras's account of the distinction between positive (or rather 'pure') economics, and his moral and applied economics in his *Etudes d'économie sociale* (1896) and *Etudes d'économie politique appliquée* (1898) respectively. In Mosini's reading, Walras's pure economics was inscribed in a larger normative framework. For Walras general equilibrium economics was not a natural order, but was a possible system for achieving the normative goal of social justice. Drawing on William Jaffé's translations and commentaries, she argues that Walras has been largely misconstrued, since the normative framework underlying his pure economics is generally disregarded. Mosini's interesting discussion of Walras is left hanging in the air, however, as she does not relate it back to the discussion of Friedman.

In the fourth chapter, Mosini returns to Friedman's methodology. She argues that part of the confusion arising from Friedman's essay can be attributed to a conflation of methodology as a branch of philosophy with methodology as being concerned with proper methods of scientific inquiry. Friedman has mostly been read in terms of the first, while according to Mosini he was concerned with the second. With regard to the question of methodology, Mosini asks whether Friedman should be described as a positivist, instrumentalist, pragmatist, Popperian, or Marshallian, and answers in the negative. Mosini goes on to consider whether Friedman's method, i.e., his way of scientific inquiry, conforms with the standards of scientific practice, by which she means the natural sciences. After a two-page comparison of Friedman's work on monetarism with the discovery of the chemical bond in chemistry, Mosini concludes that "Friedman's method as described and illustrated in his own theorizing did not reflect in any way the scientific practice" (p. 96). She ends by asking whether Friedman really believed in his own methodological practice or whether it mostly served to persuade economists and policy makers that his work was scientific.

In the fifth chapter, Mosini considers some of Friedman's substantive contributions to neoliberalism. Chapter 5 examines Friedman's claim in his popular book *Capitalism and freedom* (1962) that economic freedom promotes political freedom. She observes that Friedman nowhere in the book provides any evidence for this claim. Mosini reviews some research on the component questions of whether economic freedom results in economic growth, and whether economic growth promotes political freedom, to conclude that there are formidable issues with Friedman's claims. She also reviews two case studies, namely Pinochet's Chile and Mubarak's Egypt, and concludes that they present clear counter-evidence to Friedman's claims and that, certainly with regard to Chile, he should have taken notice of them. In her words:

> The account given in this chapter of the many challenges brought to Friedman's apodictic claim that economic freedom promotes political freedom, and the evidence from specific case studies that can easily be generalized, revealed the existence of at least one blatant contradiction to the claim, the Chilean experiment, to which Friedman turned a blind eye (p. 119).

In the final chapter, Mosini tries to make a connection between the causes of the current financial crisis and Friedman's work on risk (e.g., Friedman and Savage 1948). It argues, somewhat haphazardly, that the neoliberal call to step back from markets and only come to the rescue when things turn bad, "was the natural development of [...] Friedman's treatment of uncertainty" (p. 135). More generally, the crisis in her view is very much the result of the divorcing of positive economics from normative economics as exemplified by Friedman's work: "Friedman's (1953) methodological paper did just that, killing two birds with one stone: attributing positive economics scientific status and objectivity, and, on that basis, subordinating to it normative economics, it ensured that regulations were informed not on ethics as traditionally understood but on 'market-ethics'" (p. 138). The economist's answer to the crisis, in Mosini's view, should return to the tradition of J. N. Keynes and Walras and again bring positive economics under the aegis of normative economics.

My general assessment of the book is that Mosini has potentially a compelling story to offer, but tells it poorly. Mosini asks an important question—is there a political significance to Friedman's methodological work—but she fails to offer a satisfactory answer. First off, I am afraid I have to say the book is poorly written. Sentences are convoluted and some contain grammatical errors (as the quotations given here demonstrate). This makes for a generally cumbersome read as one needs to continuously decipher what Mosini is trying to convey.

More problematic is its composition. It is not always clear from the outset what the purpose of a chapter is, and chapters generally lack a clearly articulated conclusion. The order of the chapters is not really explained and it is unclear how the chapters build up towards a coherent conclusion. In fact, the book ends without a conclusion. And what, for instance, is the function of the third chapter on Walras for the larger argument of the book? This is too bad, because Mosini has great material for an important story.

Mosini's central concern is to expose Friedman for not practicing what he preached, and to show moreover that his preaching mostly served an ulterior, political purpose. Although I am quite convinced that Mosini is onto something important, her evidence is hardly convincing. From a cursory reading of two essays from his 1953 collection and a two page discussion of Friedman's work on monetarism, for example, she concludes that Friedman does not provide evidence for his theories in the manner prescribed by himself or the scientific community at large. Therefore, she claims, Friedman's theoretical prescriptions lack a sufficient scientific basis, and therefore his methodological essay primarily served a political function. These are strong claims, and in order to be convincing they require a much more thorough treatment of the evidence than Mosini offers.

In the fifth chapter for instance, Mosini rightly takes on Friedman's claim that economic freedom promotes political freedom, noting that Friedman offers no scientific evidence for this claim. But Mosini's counter-evidence consists of a few pages long empirical discussion of the question of whether economic freedom results in economic growth and, in the long-term, political freedom or democracy. This is sketchy at best. Citing the cases of Chile and Egypt (the choice of the latter is nowhere motivated) hardly constitutes compelling and sufficient counter-evidence to Friedman's thesis. If her aim is to disprove Friedman's claims about economic and political freedom, that requires a book-length refutation. Otherwise she falls prey to a similar charge of unscientificness. If you want to lecture Friedman about scientificness, your own work needs to have impeccable scientific standards.

As I understand it, the main point of Mosini's book, however, is to expose how Friedman's methodological views may unwittingly have lent scientific credibility to his neoliberal prescriptions and thus helped engender a 'fact-free' type of neoliberal politics in which the potential falsity of an economic theory's assumptions is irrelevant to discussions of its policy-applicability. I think this is a valid and important point. However, there is then no need to go all the way to disprove Friedman's economics. Rather than an all-out reckoning with Friedman's role in neoliberalism, Mosini should have stuck to meticulously charting and critiquing the political implications of his methodological arguments. Staying focused on what I take to be her real question could have saved Mosini from veering into gross overstatements of her case.

One of these overstatements concerns Mosini's account of Friedman's role in the history of neoliberalism. My concerns are firstly that Mosini nowhere explains what she means by neoliberalism, and secondly that she takes Friedman's role in it too much for granted. Neoliberalism is a highly elusive label (see Zuidhof 2012) and Mosini takes a little too easily for granted what it stands for and how Friedman is related to it. She presents neoliberalism as a strange amalgam of trade liberalization, monetarism, deregulation of finance, limited government, free markets, and a market-ethics, and makes it seem as if Friedman, in particular through his methodology, is singlehandedly responsible for all things neoliberal.

In other words, Mosini tends to equate neoliberalism with anything that came out of Friedman, turning neoliberalism into some sort of Friedmanism. That is a gross overstatement of the reach of neoliberalism, Friedman's involvement, and the purchase of his methodological argument. Not only is the history of neoliberalism much more complex and multi-faceted than Mosini makes it seem, the relevance of Friedman's methodological insights are much more subtle and intricate than her account allows. So Mosini unfortunately ends up grossly overstating the role of Friedman's methodology in the history of neoliberalism, while she could have a much more concise and credible point to make.

The really interesting question underlying Mosini's book therefore remains unfortunately by and large unanswered. Did Friedman's methodological essay serve a political purpose and how may it have been instrumental in fostering a neoliberal agenda? Why indeed did Friedman write this essay? Mosini is onto something when she relates Friedman's methodological views to his hawking of neoliberal precepts. The crucial point I learned from Mosini is that Friedman's methodological argument may have served to inoculate his economics from direct refutation by the facts, allowed him to present normative views as positive, and was thus conducive to supporting a kind of fact-free neoliberal politics. It is a missed opportunity however that Mosini did not provide the reader with a less overstated and more accurate account of how this came about.

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A review of Daniel M. Hausman's *Preference, value, choice, and welfare.* New York: Cambridge University Press, 2012, 168 pp.

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Daniel Hausman's latest book is about preferences in economics, and the way in which economists use the concept of preference to explain, predict and evaluate actions and institutions. The book is divided into three parts. In part one, Hausman provides an analytical clarification of the notion of preference and the view of choice that are implicit in the practice of mainstream positive economics. In part two, he turns to normative economics and discusses, among other things, the extent to which preference satisfaction can be used to measure welfare. In part three, Hausman reviews empirical investigations of choices and preferences carried out by psychologists and behavioral economists, and explores at a very general level some ways in which these investigations should spur mainstream economists to modify their conception of preference and choice. In this review, I will focus on part one of the book because in my opinion it contains the key and novel tenets of the work.

As already mentioned, in this first part Hausman looks at the ways in which economists use the concept of preference to explain and predict choices, and attempts to make explicit what this use implies about what preferences are and how choice is understood in positive economic theory. This is a typical clarification exercise in analytical philosophy, which leads Hausman to conclude that preferences in economics are "total subjective comparative evaluations" and that mainstream economists understand choice according to what he calls the "standard model of choice".

By defining preferences as "evaluations" Hausman sets himself in opposition to a characterization of them as primitive, barely changeable, and possibly unreasonable desires, a characterization that is usually associated with David Hume's view of human nature. For Hausman, on the contrary, preferences in economics are "more cognitive, more like judgments, than [are] desires" (p. x). They are the output of a cognitively demanding process in which agents take into account not only their desires, but also everything they regard as relevant to their choices, such as moral commitments, beliefs about the consequences of their actions, or the pursuit of consistent behavior. Like judgments, preferences are subject to rational criticism and can be modified as a consequence of this criticism.

Preference evaluations are "subjective" in the sense that they are mental attitudes that differ from subject to subject; and they are "comparative" because "to prefer something is always to prefer it to something else. If there are only two alternatives, one can desire both, but one cannot prefer both" (p. x).

Finally, preference evaluations are "total" in the sense that the agents of economic theory are assumed to compare alternatives with respect to everything that matters to them. This holistic character distinguishes the economic notion of preference from the everyday notion:

> In everyday usage, preferences are typically 'overall' comparative evaluations. In an overall evaluation, agents compare alternatives with respect to most of what matters to them rather than […] with respect to everything that matters to them (p. 3).

In ordinary usage, moral commitments and other cognitively sophisticated evaluative dimensions are regarded as factors competing with preferences in determining choices rather than, as Hausman claims is the case in economics, factors that contribute to the very formation of preferences.

According to Hausman, the characterization of economic preferences as total subjective comparative evaluations draws from the four axioms of ordinal utility theory that constitute the core of positive economics. The first two axioms are mentioned in every economic textbook and require, respectively, that preferences be complete and transitive. The other two axioms, by contrast, are rarely formulated explicitly but, according to Hausman, are implicit in the practices of economists.

Axiom three allows economists to focus on the set of available alternatives and to disregard the context in which these alternatives become available. Hausman calls this axiom "context independence" and formulates it as follows: "Whether an agent prefers *x* to *y* remains stable across contexts" (p. 16). Axiom four is called "choice determination" and links preferences to choices: "Among the alternatives they believe to be available, agents will choose one that is at the top of their preference ranking" (p. 15).

Although Hausman observes that "there seem to be counterexamples to all the axioms" (p. 17), the goal of this part of the book is not to assess the realism of these axioms but to make clear what they imply about what preferences are in economics. For Hausman, the only plausible way to have a complete, transitive, and context-independent ranking of alternatives is when such a ranking results from a total subjective comparative evaluation of the alternatives. In particular: completeness implies that agents evaluate alternatives in a comparative way; transitivity requires that the agents have carefully and thoughtfully evaluated all alternatives at stake; context-independence may emerge only if agents have evaluated everything that matters to them; while choice determination implies that preferences are not just judgments but motivate action.

While I agree with Hausman that preferences in economics are comparative and subjective, I am not convinced by his characterization of them as total evaluations, that is, as exhaustive and cognitively sophisticated assessments of alternatives. No economics paper characterizing preferences in this way comes to my mind, and the author does not cite any economic texts in which preferences are so defined. In their practice, or so it appears to me, economists seem to be closer to the Humean conception of preferences as desires.

One may reply that Hausman's point concerns what is implicit in the practice of economic theorizing, and that he therefore does not need extensive citations and long reference lists to make his case. It is enough to prove that the four axioms of ordinal utility theory imply that preferences are exhaustive and cognitively sophisticated evaluations. However, I find Hausman's "proof" of this thesis not only too quick—the issue is dealt with in just two pages of the book—but also too loose. It seems to me that if one adopts the loose standard of proof adopted in these two pages, one could draw from the four axioms of ordinal utility theory many different "implications" as to the nature of preferences in economic theory. For instance, in the spirit of the definition of economics as an "inexact and separate science" that Hausman put forward in a previous and much discussed book (1992a), one might be tempted to claim that the four axioms offer an inexact characterization of selfish desires, which in turn allows economists to provide a unified account of the separate domains of social phenomena where selfish desires predominate as causal factors.

In my opinion, if Hausman had referred more closely to the economics literature and provided more textual evidence in support of his characterization of economic preferences as total evaluations, then this characterization might have been more convincing. Moreover, since I am not sure whether preferences as total evaluations are inexact and separate in the sense of Hausman's 1992 book, I would have appreciated a discussion of the relationships between the theses expressed in that work and those put forward in the book under review. Unfortunately, such discussion is missing.

I also see problems in Hausman's "standard model of choice", that is, his characterization of the understanding of choice that mainstream economists supposedly hold. In presenting this model (see especially pp. 36-37, and Figure 4.1), Hausman introduces a distinction between "basic preferences" (also called "distal" or "underlying" preferences) and "final preferences". The difference between the two is that while basic preferences are independent of "beliefs about properties and consequences of alternative actions" (p. 37), final preferences obtain when the agents take into account these kind of beliefs and adjust basic preferences in light of them. (Actually, Hausman notices that final preferences may have feedback effects on basic preferences, but I will pass over this further complication.)

Final preferences do not determine choices directly and independently. Actual choices depend also on what can be actually chosen, i.e., on constraints, as well as on beliefs of a kind different from those mentioned above, namely beliefs about what alternatives in the preference ranking are available. In sum, at a first level, basic preferences and beliefs about alternative actions jointly determine final preferences; and, at a second level, final preferences, beliefs about what is available, and constraints jointly determine choice.

A first problem I see in Hausman's standard model is that his statement that economists refer to two different concepts of preference—basic and final—seems to contradict his claim that preferences in economics are univocally total evaluations. So far as I understand, it is the final preferences that are total evaluations. But if this is the case, what are basic preferences, and what is their role in economic theorizing? Could basic preferences be interpreted as desires? It seems that this is not the case, since basic preferences are also cognitively sophisticated evaluations that are limited only by their failure to take into account beliefs about the properties and consequences of alternative actions. Probably, basic preferences could be characterized as "almost-total subjective comparative evaluations" (although this is not the author's terminology). But still, Hausman does not clarify sufficiently the relationship between basic and final preferences, nor explain how to conciliate the distinction between them and the characterization of preferences in economics as total evaluations.

I also find the role of beliefs in the standard model problematic. In the first place, the very distinction between "beliefs about properties and consequences of alternative actions" and "beliefs about what is feasible" seems to me a muddy one. For instance, is my belief that I cannot fly, no matter how vigorously I flap my arms, a belief about properties and consequences of my actions or a belief about what is feasible? In addition, the distinction between the different functions that beliefs about alternative actions and beliefs about what is feasible perform—the former interacting with basic preferences to determine final preferences, the latter interacting with final preferences to determine choice—seems to me somewhat arbitrary. More generally, if we conceive of preferences as cognitively complex evaluations (be they "total" or "almost-total"), how can we really keep preferences and beliefs apart?

A final problem for the standard model as a characterization of how mainstream economists understand choice is that, as Hausman acknowledges, its causal explanatory structure is at odds with the as-if interpretations of economic theory that most mainstream economists embrace. Consider for instance the case of expected-utility theory, which is explicitly discussed by Hausman (pp. 37-45). If we frame expected-utility theory in the terms of the standard model, we can say that an agent's preferences over the outcomes of gambles are his basic preferences, while his beliefs about the probabilities of the outcomes are his beliefs about the alternative actions. These preferences and beliefs jointly determine the agent's preferences over gambles, which are his final preferences. Therefore, according to the standard model, expected-utility theory would explain the agent's preferences over gambles as causally derived from his preferences over outcomes and his beliefs about probabilities.

However, this is not the way expected-utility theory is presented and usually interpreted in mainstream economics. The axioms of expected-utility theory concern the agent's preferences over gambles, i.e., his final preferences. Therefore, in expected-utility theory final preferences come first, and are not causally determined by preferences over outcomes and beliefs about probabilities. On the contrary, these latter preferences and beliefs can be identified only on the basis of final preferences over gambles. In the mainstream interpretation, then, expected-utility theory only says that, under certain assumptions, preferences over gambles can be represented as if they derived from the maximization of a linear function combining preferences over outcomes and beliefs about probabilities.

Hausman criticizes this as-if interpretation of expected-utility theory. He argues that by taking preferences over gambles as given, the as-if interpretation prevents economists from investigating what determines these preferences, and offers no guide about how to modify expected-utility theory when it is violated. (This criticism is in fact an application of Hausman's famous "under the hood argument" against methodological instrumentalism; see Hausman 1992b.) In many respects, I agree with Hausman. However, if mainstream economists understand expected-utility theory according to an as-if interpretation that is in clear opposition with the causal structure of the standard model, how can we claim that this latter model characterizes the view of choice that mainstream economists hold? In fairness to Hausman, I add that his application of the standard model to game theory in chapter 5 is much more convincing and makes that chapter one of the most appealing of the book.

More generally, in discussing the standard model Hausman seems to pursue two different goals at the same time: an analytical clarification of the view of choice held by economists, and advising them in a prescriptive spirit about the view they should adopt. Although both goals are perfectly legitimate, I think that Hausman does not distinguish them with sufficient precision, and this generates some confusion for the reader of the book.

In conclusion, in his new book Hausman discusses preferences in economics with his usual philosophical sophistication and a wealth of acute insights. However, it seems to me that the book leaves many issues open.

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Review of Matthew Adler's *Well-being and fair distribution: beyond cost-benefit analysis*. Oxford: Oxford University Press, 2011, 656 pp.

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Matthew Adler develops and defends a particular view on how the appraisal of social policies should be performed and in doing so offers a new and refreshing take on the subject of fair distribution.

Adler argues for prioritarianism, which, broadly speaking, acknowledges that inequality in the distribution of well being across individuals constitutes an important factor in collective decision-making. Adler does not contribute a novel prioritarian social welfare function. He provides a more thorough justification for one among those the literature has already identified (see Moulin 1988). In pursuit of this agenda, Adler takes us through all the stages of the construction of his prioritarian argument, from the foundational underpinnings to its policy implications. With this book, Adler accomplishes the daunting task of merging both the philosopher's and the economist's perspectives on the issues at hand. His exposition, as well as his line of reasoning, is enhanced by the fact that each topic is addressed from multiple angles.

The argument of the book develops against the backdrop of the fundamental exercise that underlies welfare analysis. Let $N = \{1, 2, \ldots, n\}$ denote the set of individuals and $X = \{x, y, \ldots, z\}$ denote the set of outcomes. Moreover, for each individual $i \in N$, let u_i: $X \rightarrow R$ be an index of individual well being at each outcome $x \in X$. Adopting this premise, one is confronted with the question of what constitutes the appropriate criterion for ranking outcomes from a collective point of view.

The book puts forward a particular prioritarian view on the matter of how a measure of social welfare should be constructed. A prioritarian social choice function bears the following form. Let g: $R \rightarrow R$ be a non-decreasing and concave function. For each pair of outcomes $x, y \in X$, x is socially at least as good as y if and only if

$$\sum_{i \in N} g[u_i(x)] \geq \sum_{i \in N} g[u_i(y)]$$

This broad family of social welfare functions exhibits aversion to inequality in the distribution of well being that each outcome induces. The degree of aversion varies with the degree of concavity of the function g. At the two extremes lie utilitarianism, which exhibits no aversion to inequality, and lexi-min, which is infinitely averse to inequality. Adler rejects these two extremes and provides a rationale for intermediate solutions.

This simple framing of the problem reveals the set of issues that one needs to tackle in order to argue in favor of prioritarianism. Adler meticulously reviews each of them in an effort to build a sound and comprehensive argument. First, one needs to accept a welfarist premise: individual indices of well being are the appropriate basis on which to found a criterion of social choice. Adler discusses the doctrine of welfarism in chapter 1, where he juxtaposes its requisites against competing theories, while providing an account of its philosophical foundations and moral implications.

Second, one needs to take a stance on the issue of the informational basis of the aggregation exercise. What information is conveyed by the individual indices of well being that a social planner may use to construct a social welfare function? In chapter 3, Adler promotes the view that such indices convey cardinal information and are, moreover, amenable to interpersonal comparisons. Although the case for prioritarianism could be promoted under alternative hypotheses, Adler's particular proposal requires this much information to be conveyed by individual behavior. He offers his own take on what constitutes a foundational theory that lends credence to the assumptions of cardinality and interpersonal comparability.

We are left then with one final question. Departing from a welfarist premise and operating on the information that the measure of individual well being conveys, how does one settle on a particular class of g functions? In chapter 5, Adler analyses the properties that the exercise of aggregation of individual utilities should abide by. He puts forward a list of axioms. These are properties of the social welfare function.[1] They impose on it adherence to certain principles whose validity may be ascertained in non-suspect time, that is without knowledge of the actual realization of individual preferences.

[1] A nice complement to this chapter is Herve Moulin's (1988) review of the formal results on which Adler bases his axiomatic argument.

The Pigou-Dalton axiom stands out as a fundamental principle that underlies the prioritarian perspective. If a distribution of utilities induced by some outcome $x \in X$ can be derived by another distribution of utilities induced by an outcome $y \in X$ by means of a hypothetical transfer in utilities between two individuals that preserves the sum of utilities and does not alter the individuals' relative rank in the distribution, then any social welfare function that satisfies the axiom should deem outcome *x* as socially better than outcome *y*.

Along with Pigou-Dalton, Adler puts forward a list of axioms and justifies their importance. He makes a good case for each of these, both on moral as well as practical grounds. These axioms uniquely determine a parametric class of social welfare functions. Each member of this family corresponds to a different degree of inequality aversion. Utilitarianism is rejected because Adler's version of the Pigou-Dalton principle does not allow for it. The lexi-min is rejected because it violates continuity, a formal property to which Adler attributes ethical content. What remains is unambiguously prioritarian.

There remains the issue of the degree of inequality aversion, the intensity of prioritarianism. Adler finds that this cannot be resolved ex ante. Instead, the social planner needs to become aware of individual preferences before being in a position to employ a fully specified social welfare function. Adler proposes a series of thought experiments performed by individuals that aim at eliciting their degree of inequality aversion. Here too, cardinality and the interpersonal comparability of utilities are crucial for such an approach to work.

Although the preceding account captures what in my view constitutes the backbone of Adler's book, it should be noted that he also takes up issues that are peripheral to his principal concern. In chapter 4 he discusses the foundations of utility theory and surveys different views on the matter. In chapter 6 he defends the idea that the aggregation problem should be framed in terms of entire lifetimes (i.e., utilities are defined over outcomes that encompass the entire span of individual lives).

In chapter 7 he embarks on the task of defending expected utility theory. The goal being not merely to demonstrate that prioritarianism is compatible with uncertainty, but, more importantly, to defend the premise that a theory of utility can be derived from well-behaved individual preferences over uncertain outcomes. Finally, in chapter 8, which concludes the book, Adler contrasts the implications of the social-welfare approach with those of competing approaches such as cost-benefit analysis, and alludes to possible extensions of his findings.

The fundamental problem that lies at the core of the book is none other than the problem of ranking social outcomes on the basis of their consequences for affected individuals. The importance of the exercise cannot be overstated. The standard methodology underlying policy design involves optimizing an objective function subject to constraints. Hence, understanding what we seek to optimize is of paramount importance. The moral values this objective function embodies, the positive assumptions it incorporates, and the conception of the individual it relies on, are all crucial and influential parameters of policy design. Adler's principal contribution is to take the reader on a journey through all the aspects of the problem, while delivering soundly argued opinions along the way.

The exercise of ranking social alternatives for practical purposes is fraught with difficulties and challenges. Although Adler chooses one complete path among many, he is careful to be fair to the alternatives he discards along his way. He reviews a broad array of dissenting opinions on each subject he touches. Moreover, he treats each topic with formal and analytical rigor informed with philosophical insight. In that respect, one source of value for Adler's book is that it serves as a survey. It will acquaint the reader with many facets of the problem, independently of whether the reader chooses to endorse Adler's views.

Primarily, though, the book's main contribution is to bring together two separate schools of thought that have been developing in parallel, and seamlessly merge them into a single account. The book offers insights for the economist, in particular the social choice theorist, who seeks to inform his approach with philosophical perspective. It will help him broaden his understanding of the welfarist foundations of economic theory and to better appreciate the moral justification of the axioms he regularly appeals to.

For the philosopher, the book offers a formal methodology that expands the relevance of the discipline and enables it to arrive at sharper conclusions with policy implications. Adler performs a normative exercise that is valid in a variety of contexts. Consequently, the criterion he delivers can be readily applied to address a broad array of issues. Adler does not merely suggest that the study of normative ethics matters for policy. He identifies a channel through which ethical theories may influence policy.

There is also, of course, the matter of the particular view that Adler is advocating. Precisely because of the comprehensiveness of his argument and his effort to construct it from the bottom up, Adler is exposing himself to many sources of criticism. This is an inevitable trade-off. From my perspective there are two important components of Adler's argument that may be points of contention.

First, there is the issue of how an individual is modeled as a decision-maker. Here Adler offers his own account, which diverges somewhat from the mainstream, but in the end draws heavily from expected utility theory. The capacity of individuals to make rational and informed decisions when faced with uncertainty is of pivotal importance for Adler's thesis. This capacity is reflected in the individual utility functions and the information they convey. Nonetheless, it seems to me that much of what Adler argues for would carry through even under alternative assumptions about the informational basis of the aggregation exercise (see d'Aspermont and Gevers 2002).

Second, Adler adopts the premise that utilities are interpersonally comparable. There has been a resurgence of research into Arrovian social choice, which is based on purely ordinal and non-interpersonally comparable utilities. This has produced an interesting array of solutions (see Fleurbaey and Maniquet 2011, for a recent survey of this literature). This literature poses two difficulties for Adler's argument. To begin with, it offers a theory of social welfare functions which relies on weaker assumptions than the ones Adler makes. Even if the assumptions on which Adler establishes his argument are reasonable, it must be pointed out that there exists an alternative theory of the social welfare function that relies on weaker ones. Moreover, in this literature the lexi-min criterion features prominently. The axioms that endorse it are justified by principles analogous to the ones Adler adopts.

The crux of the matter is that, in the Arrovian framework, abiding by a mild prioritarian principle, along with other principles that take no stance on the issue of redistribution, produces egalitarianism. Without interpersonal comparability of utilities, and in the absence of any other axiom that encompasses redistributive concerns, a Pigou-Dalton principle appropriately construed to accommodate the Arrovian framework has enough bite to induce an infinite aversion to inequality. This illustrates the fact that much of Adler's defense of prioritarianism hinges on the assumption of the interpersonal comparability of utilities.

In spite of these remarks, Adler offers an authoritative argument in favor of prioritarianism. I believe it is fair to say that although he does not write the final word on many of the issues he discusses, he nonetheless offers an invaluable contribution to an ongoing dialogue that it is critically important to sustain and nourish.

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Review of Spencer J. Pack's *Aristotle, Adam Smith, and Karl Marx: on some fundamental issues in 21st century political economy*. Cheltenham: Edward Elgar, 2010, 288 pp.

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Spencer Pack's new book can be understood as an architectural guidebook to three great buildings: the intellectual systems of Aristotle, Adam Smith, and Karl Marx. There is also an outlook on the contemporary landscape, as announced in the subtitle. For the most part, however, Pack analyses the writings of these three seminal figures, and he looks at them through the lens of three pairs of concepts: exchange value and money, capital and character, change and government. Spencer does not provide a detailed justification for his focus on these concepts or on these three thinkers—except that they are important figures in our intellectual tradition and stand in a kind of "dialogue" (p. xi). So the proof of the pudding is in the eating; but, to anticipate, the overall result justifies this choice of focus, as it illuminates central dimensions of the thought of Aristotle, Smith, and Marx, and leads to insightful comparisons.

Starting out with Aristotle, Pack discusses his views on money and exchange and his rejection of chrematistics, the use of money for acquiring more money, as unnatural. He lays out Aristotle's view of human nature with its focus on habit, and shows how this relates to his views of choice and consumption. He emphasizes Aristotle's cyclical view of history, which also applies to his view of government, which tends to become corrupt and decline into unnatural forms.

Adam Smith, in contrast, saw commercial society, including what Aristotle called chrematistics, as perfectly natural. Pack discusses Smith's arguments about the positive influence that commercial society can have on the human mind, but also mentions the problems with the character of "merchants and manufacturers" and other socio-economic groups in commercial society. He rejects the misreading of Smith as neoliberal by providing a well-rounded discussion of Smith's view of the state, which, on the one hand, has important functions to fulfil, but on the other hand can become an instrument in the hand of the rich for preserving their privileges. In contrast to Aristotle's view of history as going in circles, Smith sees history as a more or less linear development and evolution, and, as Pack notes, did not seem to see species as eternal, which points in the direction taken up by Darwin.

In Part III of the book, Pack analyses Marx's writings with an eye to the same concepts he focused on in the first two chapters. Karl Marx knew and referred to the works of both Aristotle and Smith, which makes this analysis especially interesting. Pack emphasizes that Marx's attempts to find the "essence" of social relations is a genuinely Aristotelian move, and that large parts of Marx's early writings build on his adoption of the Aristotelian distinction between use-value and exchange-value, which he combines with his labour theory of value. He discusses Marx's analyses of the development of money and of the role of capital in the history of capitalist society, as well as the question of the character of the protagonists of this society. He notes, for example, how Marx, in comparison to Aristotle, shifts the focus to the working population, and especially the unskilled workers, who stand in an extreme contrast to the capitalists. Pack also discusses Marx's view of the state and of history, distinguishing his functional, instrumental and alienated power views of the state. Finally, he analyses Marx's view of history, focussing both on the relation between the "natural" and the "social" in Marx and on his developmental account of capitalism, which owes elements to both Aristotle and Smith, and which also relates to Marx's views on science as a secularizing force that emancipates individuals from the "opium" of religion.

The ideal reader of this part of the book already has some general knowledge of Aristotle, Smith, and Marx, for Pack does not start out with general introductions to their thought. But he presents their ideas in a very clear manner, reducing jargon to a minimum, and embedding the aspects he focuses on in their wider systems. One might disagree with some of his interpretations, such as whether Smith really had a labour theory of value, or whether he saw this as an adequate model only for the early stages of society (see Blaug 1996, 38, 51f.). On the whole, however, Pack does a good job in presenting the central architecture of the views of these authors on economic issues.

The changing view of what is "natural" or "unnatural" about commercial society is an important theme in Pack's comparative discussions, and it shows that to call something "natural" is almost never a value-free statement. Another point that Pack underlines is the degree to which Marx takes up Aristotelian ideas and concepts, which he shows in great detail, focussing, for example, on the four types of causality in Aristotle that Marx also covers in *Capital*. Speaking more generally, the great attention to details is certainly one of the strengths of the book. Hidden in the footnotes are interesting asides, which often shed additional light on aspects of modern economic theorizing. For example Pack's point that the idea that commerce might create friendship, which was present in Aristotle and Smith, has been completely lost in modern economic theory with its assumptions of anonymous markets (p. 39). Naturally, not everything in Pack's analyses is new—that is the price for writing about well-known, influential figures. But unless one is an expert on all three of them, it is likely that one will come across new and interesting insights, many of which are brought out by the comparative perspective.

Part IV is different from the first three parts in that Pack here focuses on contemporary issues, in combination with (sometimes slightly repetitive) summaries of his earlier analyses. The structure follows the same three pairs of concepts: exchange value and money; capital and character; government and change. Pack brings in additional thinkers, notably Piero Sraffa and Vladimir Dmitriev, who both contributed to a better understanding of economic value. He explains Sraffa's model of the production of commodities from commodities, which dethrones labour as the only factor that creates value. Using Smith's and Marx's reflections on the need for commensurability of goods in an exchange economy, he argues that the US dollar may have taken on the function of a world currency, similar to the gold standards of earlier days, and discusses the implications for the trade and current account deficits as well as the monetary and economic policies of the United States.

In the chapter on capital and character, Pack discusses the problem of how those without property might be able to earn a living in an economy that becomes more and more automated. He rejects the argument that the expansion of capitalist economies is held back by a lack of savings, which he sees as founded upon a problematic definition of savings that leaves out certain forms of savings that are not processed by the financial markets. He also cautions the reader not to forget the problem of character, especially with regard to managers in both capitalist and in communist enterprises, taking up arguments from the literature about the structural "convergence" of capitalist and communist systems as a result of the interests of managers from the 1980s. The communist system does not exist any more, but the question of "how do we control our managers?" (p. 202) remains relevant in the 21st century, Pack holds.

With regard to government, Pack is equally sceptical. Referring to a body of critical literature about the presidency of George W. Bush, he argues that the risk that the state becomes a tool in the hands of the rich and powerful is as much alive now as it was in Adam Smith's time. He argues that one of the ways in which the not-so-rich and not-so-powerful have reacted to the fall of communism and the loss of Marxist hopes for a betterment of their lives was to turn to religious movements that are more or less fanatical in character. At this point, the discussion becomes somewhat speculative, but the general question of what people can turn towards in order to imagine a better future is certainly a valid one.

All of the problems discussed in the fourth part of the book are interesting and important, and Pack does a good job of showing how arguments drawn from Aristotle, Smith, Marx, and other historical thinkers are relevant for reflecting about them. A problem with this kind of analysis, however, is that Pack pulls out individual threads from what are extremely complex issues, and although they are certainly important threads, one wonders whether it is sufficient to discuss them in isolation. For example, the question of whether the US dollar might function as a world currency in today's globalized economy, and what this means for US monetary policies, seems to be closely connected to Chinese trade policies, since, according to one common argument, the US trade deficit has at least as much to do with the decision of the Chinese government to keep their currency undervalued in order to strengthen the export sector as with the dollar's function as a world currency.

Pack's decision to focus on the key concepts that he has chosen as a common thread for the book, however, means that such aspects remain outside the scope of analysis. But one has to say in his favour that Pack does not try to provide easy solutions to these problems. His aim rather seems to be to point out which questions we should ask, and maybe also which questions we have forgotten to think about because the history of economic thought, in which they had been asked, is not widely researched and taught any more. The questions Pack shows us may not be the only ones, but they are certainly worth asking, and for this it is helpful to take into account our intellectual heritage and the shift in perspective that it makes possible by showing the historical contingency of many of the concepts and ideas that we take for granted.

An important meta-question implicitly raised by the last part of the book, however, is whether it is enough to answer these questions in a piece-meal fashion. Pack's method of picking out certain concepts and elaborating their implications stands in some contrast with the overarching systems that Aristotle, Smith, and Marx built, and which Pack so brilliantly describes. It would of course be unfair to charge Pack with not having come up with something as grand as the systems he writes about, and one might even argue that it is questionable whether we will ever have such a system again. On the other hand, it also seems problematic not to have *any* overarching system, precisely because the problems are so complex and intertwined that overarching principles are needed both in order to know what to look at in the confusing wealth of phenomena and in order to evaluate the salient issues in a unified way.

The philosopher who is often said to come closest, in the 20th century, to an overarching system in political philosophy, is, of course, John Rawls (especially, Rawls 1971), and arguably part of what made him so influential is precisely the fact that he approached the problem of how to construct a just society in a more principled and systematic way than many other writers. But this also comes at a cost: Rawls's system stays rather abstract, and he says relatively little about the concrete institutions he would want to see in a just society, and in particular its economic organization.

What seems to be needed, at the present, is a synthesis between the overarching theories of justice that Rawls and others have provided, and more detailed accounts of economic life that are sensitive to its many normative dimensions. Not least because of the financial crises of recent years there is indeed an increased interest in economic questions among political philosophers coming from various parts of the political spectrum (e.g., Tomasi 2012; Honneth 2011, part C.III.2). The call for more "non-ideal theory", despite meaning different things to different people (e.g., Sen 2006; Wiens 2012), also implies that one needs to take a closer look at economic questions when one wants to talk about justice. A deepened understanding of the seminal figures that shaped the history of economic thought is a good basis for such an endeavour. This, together with the food for thought it offers through its wealth of observations, recommends Pack's books to readers both in the history of economic and political thought and in political philosophy in general.

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Review of Bernard Walliser's *Comment raisonnent les économistes: les fonctions des modèles*. Paris: Odile Jacob, 2011, 278 pp.

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Bernard Walliser is an economist at Paris-Jourdan Sciences Économiques (PSE) whose main research interests are in game theory (epistemic and evolutionary) and the methodology of economic models. His book provides a broad overview of what scientists in general, and economists in particular, do with their models. The French language literature on models is at best sparse, and this book contributes to filling the linguistic blank. But beyond this value for French readers, the project is itself original and of interest to a broader audience. Walliser's systematic typology of the different functions of models improves our understanding of them by putting them into a comprehensive epistemological framework.[1] The first part of this review will describe this typology in more detail, before turning to some critical comments.

The core of the book consists of six chapters, each one focussing on a distinct function of economic models. 'Function' here is to be loosely understood as the purpose a model can fulfil. The functions identified by Walliser (and whose labels will be explained below) are: 1) iconic, 2) syllogistic, 3) empirical, 4) heuristic, 5) praxeological and 6) rhetorical. This typology is explained in the introductory chapter.

Walliser claims that every model can be studied using both internal and external points of view. From the external point of view, a model is seen as referring to a target system, which can be real or not, and linked to it by bridge principles, i.e., rules that connect theoretical statements to observational statements. The internal point of view conceives the model as a system in itself that can be used for various conceptual operations, like simulation.

[1] It should be noted that the book is an extension of previously published papers (e.g., Walliser 2007a; 2007b) that already developed the main ideas behind the typology. English-speaking readers interested in the typology's basic framework may turn to Walliser 2007b, albeit at the loss of the explanatory depth, examples and conceptual analysis found in the book.

In addition, Walliser claims that each model can be analysed along three dimensions: the syntactic, semantic, and pragmatic. The syntactic dimension concerns the model's form, the semantic its content and the pragmatic its use. By combining these—applying the two points of view on each dimension—we get Walliser's typology of the six functions fulfilled by every model. Walliser identifies each function with a central epistemological problem and an associated virtue. Figure 1 summarizes the typology in a table.

Figure 1: Walliser's typology of models summarised

DIMENSIONS	EXTERNAL	INTERNAL
SYNTACTIC	**Iconic** Problem: *Interpretation* Virtue: *Expressivity*	**Syllogistic** Problem: *Explanation* Virtue: *Tractability*
SEMANTIC	**Empirical** Problem: *Idealization* Virtue: *Plausibility*	**Heuristic** Problem: *Cumulativeness* Virtue: *Fecundity*
PRAGMATIC	**Praxeological** Problem: *Instrumentality* Virtue: *Operationality*	**Rhetorical** Problem: *Performativity* Virtue: *Intelligibility*

The *iconic* function may be summarized as "to know is to represent" (p. 15).[2] Here, models are used to formally represent systems of which we only have an intuitive grasp. Bridge principles link the model to its system of reference. The central epistemological problem here is therefore *interpretation*: assigning meaning to the model's variables and their relationships with the target system. The characteristic virtue for the iconic function is the model's *expressivity*: its capacity to express the relevant and essential properties of the target system.

The *syllogistic* function may be summarized as "to know is to calculate" (p. 57). Models are here used to derive conclusions deductively from a set of hypotheses. They are instruments of reasoning that allow us to draw and structure inferences. The central epistemological problem, explanation, concerns the nature of the processes from which a model's conclusions are derived. Its characteristic virtue is *tractability*.

[2] All quotes are freely translated from French by the reviewer.

The *empirical* function may be summarized as "to know is to test" (p. 99). It concerns the relation between the model and the empirical world. A model is always at some distance from reality, a distance that has to be evaluated. The central epistemological problem here concerns *idealization*. The characteristic virtue of a model here is *plausibility*—i.e., its ability to convincingly represent the target system.

The *heuristic* function may be summarized as "to know is to create" (p. 141). New models tend to borrow from previous models. The central epistemological problem here is *cumulativeness,* which concerns how models cohere with each other and what continuity the knowledge they bear has through time. A model will be *fecund*, the function's characteristic virtue, if it can serve as the basis for other, related models.

The *praxeological* function may be summarized as "to know is to intervene" (p. 183). Models are used to investigate certain practical questions by predicting what the consequences of a given intervention would be. The central epistemological problem here is of *instrumentality*. Models must here have the characteristic virtue of *operationality*, the capacity to be used to answer questions about possible interventions.

The *rhetorical* function may be summarized as "to know is to communicate" (p. 225). Models are used here as a communication device to allow modellers to share their thoughts and results explicitly and pedagogically, to express various ideas that can be grasped by an audience. The central epistemological problem here is *performativity*. Models influence the beliefs and cognitive representations of agents, be they individual or collective, by suggesting that they should be structured according to what the model claims. The characteristic virtue of a model here is *intelligibility*.

Walliser expresses some criticisms—especially in the conclusion—about the current state of economic modelling, but these criticisms do not follow directly from his typology. The typology rather serves as a tool of investigation by pointing out how things *can* go wrong. Walliser says that,

> models exhibit an over-obligingness that makes them lose a lot of their rigor and relevance. Modelling has become an exercise apparently too readily within the reach of any newcomer in the profession. It amounts to the translation into a formal language of

preconceived ideas, of imported conceptions or of trivial regularities (p. 271).

However, Walliser does not advocate a return to a more 'literary' economics. He suggests different ways by which we can resist the alleged trivialization of economic models. Greater attention should be paid to their interpretation, their empirical validation, and their vulgarization. They should also be subject to stricter norms of acceptance and selection. He is hopeful about recent empirical developments in experimental and behavioural economics, and the tighter links economics now has with other social sciences.

The book offers an interesting typology for people interested in models in general. Walliser's systematic approach also makes it easy to locate and understand the different issues he addresses. He also gives numerous examples, though mostly from economics, to illustrate his points. The structure of the book forces the reader to reflect on the relations between the different functions of models and specific epistemological problems.

An interesting feature is that each chapter finishes with a quick overview of how that function is instantiated, with some variation, in the formal, natural, and social sciences. This discussion, even though it only scratches the surface, is certainly welcome and should be carried further. Models are often studied either in a very abstract manner or in the context of their application within a specific discipline. Walliser avoids either extreme, in line with his intention that the typology serve as a general epistemological framework for thinking about models.

Walliser's book is primarily theoretical. That is, he describes the functions of models qua models, but he does not aim at assessing whether or not particular economic models attempt or successfully fulfil them. When expounding the different functions he does not merely present hypothetical suggestions, but rather claims categorically that models are this and that, and that modellers are motivated by very specific reasons. However, at least some of Walliser's positions are considered very differently in the literature.

For example, Walliser retains the deductive-nomological (D-N) model of explanation (Hempel and Oppenheim 1948) as the right account of explanation (p. 73). He claims that both causal and intentional types of explanation in economics obey the D-N model (pp. 77-78). While it is certainly true that these explanations *can* often be construed in terms of the D-N model, it is widely accepted today that the D-N model of explanation is deficient in general and especially for economics (Hausman 2009). The reader should be aware that there are potential controversies around some of Walliser's claims.

Despite the mainly descriptive character of the book, it is sometimes not clear whether some of Walliser's claims are meant to be taken as matters of fact or rather as appraisals of modelling practice—e.g., "a model is *over-cumulative* if it is tirelessly repeated without any real novelty" (p. 171, emphasis in the original). This is especially the case when he discusses the problems associated with the different functions. He claims that models can be *over* or *under* affected by them, but this is contentious. For instance, there is no accepted understanding of what it would mean for a model to be 'over-explanatory' or 'under-explanatory'. Some recent accounts of models even deny that they can be explanatory at all (see Reiss 2012). Whether or not economic models do, or even can, suffer from the problems Walliser identifies is not a settled matter but rather the object of on-going discussions. The fact that a model is "tirelessly repeated", for instance, could be a virtue if the model is empirically adequate, as is arguably the case with some models in the natural sciences.

Like any good typology, Walliser's helps us to think clearly and systematically about its object. One can probe a model using the typology's categories in order to evaluate how it fares with respect to the qualities and problems models can have. One can use it to see the similarities and differences between various models, as Walliser often illustrates. However, it is sometimes difficult to understand to what extent these functions (and the problems they are associated with) are really independent. For instance, Walliser identifies the problem of explanation as pertaining to the syllogistic function. However, whether or not a model aims at (iconic function) and succeeds in (empirical function) truthfully representing the relevant causal relations is considered central to the 'paradox of explanation' (Reiss 2012).

To causally explain, a model has to receive a realist interpretation and it must accurately represent the causal relations at work. Conversely, the problem of idealization that Walliser discusses in connection to the empirical function is generally conceived as being central to how we should interpret (iconic function) economic models (Mäki 2009). Indeed, the discussion about idealization has generated an important literature on the fictional status of scientific models (Suárez 2009). This raises questions about the general accuracy of the typology, at least with respect to how these issues are usually treated in the literature.

A discussion on the relations between the different functions would probably have helped to clarify these issues. It would have been especially interesting concerning the qualities linked to each function, since these raise important questions. For example, are there trade-offs between the different qualities, for instance between expressivity and fecundity? Game theory would on this account be considered quite fecund, but at the cost of expressivity, since interpreting it poses serious problems (see Grüne-Yanoff and Lehtinen 2012). Or, to what extent is a model's operationality helped by its tractability? A macroeconomic model that cannot give clear answers about the consequences of potential interventions would have limited usefulness for the policy-maker. Although Walliser does not address such questions, his book suggests them as a possible line of inquiry for future research on the functions of models.

A final critical point is that it is very difficult to relate this book to the current literature on the subjects it covers. The book contains almost no in-text references, and one finds at the end of the book only a reduced "Summary Bibliography" of less than a page. The reader is at a loss to understand why these twenty entries in the bibliography were selected, while many other papers and books that are part of the literature were left out (for a good overview of the literature involved see Hausman 2008; Frigg and Hartmann 2012; Knuuttila and Morgan 2012).

The philosophical discussion on economic models comprises many different positions and arguments with very little consensus, and it would have been interesting to understand where Walliser locates himself with respect to that ongoing conversation. When he does take a position on a contested issue, its context is never mentioned, and we do not learn who defends opposite views and what their objections to Walliser's claims might be. As a corollary, it is difficult to recognise when Walliser is advancing an original and perhaps controversial opinion of his own, and when he is merely relating a generally accepted understanding of models. This shortcoming makes the book less useful for students who wish to introduce themselves to the subject.

All in all, Walliser's book is best suited for people who already have some knowledge about the philosophical issues related to economic models, but want to better grasp the many functions models have.

Apart from the more than welcome addition to the French literature, Walliser's main and substantial contribution is a systematic and original typology that brings the various functions models can have under a single epistemological framework.

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PHD THESIS SUMMARY: Informal institutions and economic development.

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My PhD thesis looks at the relationship between informal institutions (e.g., morals, customs, traditions, norms, ideologies, and religion) and economic development. The perspective is conceptual as well as empirical. The conceptual part critically compares the theories of Aristotle, Montesquieu, Alexis de Tocqueville, Karl Marx, Max Weber, Friedrich A. von Hayek, Douglass North, James Coleman, Robert Putnam, and Francis Fukuyama. The empirical part relates and analyses three country-level case studies: Mexico, South Korea, and Morocco

The PhD thesis is motivated by the observation that institutions are getting more and more important in the debate about the determinants of long-term economic development. However, most economic studies still analyse the role of formal institutions exclusively (e.g., the rule of law, property rights, or patent law) while informal institutions are neglected. Daron Acemoglu (et al. 2004) and Hernando de Soto (2000) can be cited as two prominent examples of this tendency. The disregard of informal institutions in development economics is surprising as Nobel Prize winner Douglass North underlined the importance of formal *and* informal institutions and their interplay throughout his path-breaking book *Institutions, institutional change and economic performance* (1990).

As most, economists have traditionally neglected informal institutions, culture, and social capital,[1] the most innovative approaches in this field have come from non-economists, like the political scientist Robert Putnam (1993), the historian David Landes (1998) and, to go back even further in the history of ideas, from the sociologist Max Weber, such as in *The protestant ethic and the spirit of capitalism* ([1904-1905]). The problem with these works is that they take formal institutions as

[1] Informal institutions, culture, and social capital are slightly different concepts, but all of them have in common that they can be interpreted as complements to formal institutions.

given and just analyse the role of informal institutions, thereby overstating the role of religion, social capital, and culture in general. For example, Landes writes: "If we can learn anything from the history of economic development, it is that culture makes almost all the difference" (2000, 2).

In contrast to many other works dealing with culture and economic development, my PhD thesis does not analyse the role of culture for economic development per se. Rather its main claim is that only a dynamic analysis of the interplay of formal *and* informal institutions can help to understand the process of economic development. I conclude that informal institutions can slow down as well as accelerate the process of economic development. This result has an important implication for development policies: Formal institutions that have been well-proven in one country cannot be transferred easily to another country with different informal institutions. Therefore, it is essential that reforms of formal institutions be compatible with local informal institutions, which change more slowly.

This finding is illustrated and endorsed by the three case studies. Mexico is presented as a country where the imposition of formal institutions that have worked well in other countries—such as democracy, federalism, a market economy, and secularism—did not succeed because they were not sufficiently supported by domestic informal institutions. South Korea, on the other hand, profited from its informal institutions. Confucian ethics in South Korea and other East Asian countries (sometimes also labelled "Asian values") proved to be more flexible, pragmatic, secular, and adaptive than other non-Western ideologies, such as Buddhism, Hinduism, or Islam. Consequently, they facilitated rapid political and economic reforms, and especially human capital accumulation as Confucianism places a high value on education.

In contrast to South Korea, important political reforms such as the separation of the church and the state have been impeded in Morocco by its informal institutions, shaped by the Islamic religion. Moreover, these informal institutions hinder the socio-economic integration of women and girls (60 percent of all females in Morocco are still illiterate). Nevertheless, formal as well as informal institutions are dynamic entities that evolve in the course of years and decades. Changes of formal institutions can cause changes of informal institutions and vice versa. For example in Morocco, the reform of the family code (*Moudawana*) in 2004 and constitutional changes during the Arab Spring put into effect by King Mohammed VI show that at least slow institutional change is possible even in countries that are thought to be locked-in to a disadvantaged institutional framework.

The results of my empirical analysis confirm central arguments of Aristotle, Montesquieu, and Tocqueville. All of these, as social scientists, stressed the importance of formal *and* informal institutions and argued that importing institutions is always problematic. For example, in his *Politics* (book IV, section 5), Aristotle argues that a democratic constitution can become oligarchic by non-democratic customs and vice versa. So informal institutions (e.g., customs) can influence the functioning of formal institutions (e.g., the constitution). In *The spirit of the laws* (book XIX, chapter 14), Montesquieu develops the thesis that the state can change formal institutions but that informal institutions cannot and should not be changed by a central authority. In *Democracy in America* (book II, chapter 5), Tocqueville highlights and admires the contemporary (1830s) political culture of the USA because it generates grassroots movements and voluntary associations which can solve social problems more efficiently than the state. On the other hand, he is realistic enough not to advise the introduction of American political institutions in his home country, France, which has a completely different political culture. These three authors are rarely considered in the current debate about institutions and development. So my thesis provides some new (and at the same time classical) perspectives to contemporary debates in development economics and institutional economics.

Apart from their specific arguments, I think that we can also learn methodological lessons from philosophically orientated social scientists like Aristotle, Montesquieu, Tocqueville, and Weber. While today's statistical studies try to reduce the concept of informal institutions, social capital or culture to a minimal definition in order to measure it, these scholars keep a broader concept in their mind. For example, in *Economy and society* ([1922]), Weber coined the term 'comprehensive social science' (verstehende) meaning that we should look more at the interaction of social variables and try to understand the underlying mechanisms. As I argue in this thesis, contemporary econometric studies (e.g., Knack and Keefer 1997) do indeed deliver partial results about the relationship between informal institutions (narrowly defined) and development (also narrowly defined as long-term growth of GDP). But these studies have to be complemented by a more dynamic and evolutionary perspective that takes into account the complexity of social values.

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PHD THESIS SUMMARY:
Ethics before God and markets: a theory of moral action in conversation with Adam Smith and Ernst Troeltsch.

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PhD in religious ethics, June 2011
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The dissertation contends that in our religiously charged and economically unstable world the disciplines of ethics, theology, and economics must be involved in a conversation about human moral action and well-being. We live in a world of increasingly fragmented social spheres and a plurality of values that has seen the conversation between theology and economics become acrimonious or simply non-existent. The dissertation uses the field of ethics to begin a rapprochement between theology and economics by following the deeply interdisciplinary works of Adam Smith and Ernst Troeltsch, who each use the insights of all three fields to build their visions of moral action in the world.

The dissertation develops a view of moral action that helps the conversation between theology and economics avoid two well trod traps: theological reflection on the economy that does not take seriously the real constraints and drives of human behavior and the reduction of theology to economic rational choice without remainder. The thesis of the dissertation is necessarily two-tiered. It first shows the structural similarities in the works of Smith and Troeltsch that enable the construction of a tensive, multidisciplinary view of moral action that arises in the face of history and the divine. Then it argues that such moral action brings theology and economics back into conversation, because it can bridge their divergent views on freedom, through the mediating power of ethical freedom, and it can bridge their divergent views of morality, through the symbol of responsibility. It places a Smithian virtue theory and a Troeltschian theory of compromise within a responsibility ethic to describe the individual quest for personality in a fruitful relationship with God and one's many social spheres.

An adequate ethical response to our world must be a relational one that holds us responsible for forming compromises for how we should live out the different values in our moral spheres. We are formed to build such compromises through our sympathy with others, our understanding of history and other peoples, our ethical freedom and imagination, and the virtues that our communities teach. Such compromises disclose—if even only in part—something about real moral values. The lure of God's personality—the unity of value that pervades the diversity of the world—draws us to the hard work of inventing moral compromises in relationship to others, and through such moral labor we form responsible personalities. The responsibility ethic developed in the dissertation offers a way to talk about moral action that is responsive to both theological and economic methods, and it provides a necessary critique of both. It enables us to express what it means to be an ethical personality *before* God and the market.

Joseph D. Blosser obtained his PhD from the University of Chicago. His dissertation was supervised by William Schweiker, Edward L. Ryerson distinguished service professor of theological ethics at the University of Chicago; and his two readers were Kathryn Tanner, professor of systematic theology at Yale University, and Deirdre McCloskey, distinguished professor of economics, history, English, and communication at the University of Illinois at Chicago. Dr. Blosser now serves as the Robert G. Culp Jr. director of service learning and assistant professor of religion and philosophy at High Point University in North Carolina. He continues to research at the intersections of ethics, theology, and economics.
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PHD THESIS SUMMARY: Introductory economics courses and the university's commitments to sustainability.

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In 1990, fearing that unsustainable production and consumption patterns were aggravating poverty and undermining prospects for future generations, university leaders issued the Talloires Declaration, committing their institutions to fostering sustainability, which the Declaration describes as "an equitable and sustainable future for all humankind in harmony with nature".[1] They pledged to work toward population stabilization, eco-friendly technologies, and ecological restoration, implicitly acknowledging ecological limits to economic activity.

My dissertation focuses on the implications of universities' sustainability commitments—such as the Talloires Declaration—for introductory economics courses (Econ101) at Canadian universities. Sustainability commitments and the sustainability in higher education literature point to the desirability of integrating sustainability across the curriculum. Universities have made considerable progress in the greening of campus infrastructure, yet there has been little progress in updating curriculum.

I proceeded on the basis that if students are to have the knowledge to improve prospects for sustainability, they need an understanding of environment-economy linkages. These linkages include: how the economy depends upon ecosystem services; how resources are extracted from the environment and returned as waste products; and how habitats are converted in the process of economic development.

I had three main reasons for selecting the way sustainability commitments play out in introductory economics courses as the research topic. First, environmental change is largely the result of economic drivers. Second, a growing body of literature suggests that if prospects for sustainability are to be improved, new economic models

[1] The declaration is available at: http://www.ulsf.org/programs_talloires_td.html

are needed; Ricardian-like presumptions that nature has inexhaustible properties are no longer tenable (see Daly 1992; Speth 2008). Third, in North America, students from across the academy are required or elect to take these courses—by one estimate, about 40% of undergrads (see Salemi and Siegfried 1999). The content of Econ101 courses is highly standardized. These courses influence the economic beliefs that circulate through society and the types of economic policies and outcomes that are considered desirable.

The theoretical framework I utilized to analyze Econ101 textbooks and assess student and faculty perspectives on the course was constructed from the transdisciplinary field of ecological economics and focused on theoretical contributions useful in understanding deficiencies in how Econ101 conceptualizes the environment. This literature also helped identify theory students should be introduced to if they are to understand the economy's relation to the environment and sustainability. For example, ecological economists posit that human demands on the biosphere should be kept within precautionary ecological thresholds (see Meadows, et al. 1972; Rockstrom, et al. 2009), calling into question the viability of further economic growth and consumerism.

I conducted content analysis of nine introductory economics textbooks, focusing on representations of the environment-economy nexus and implicit values related to sustainability (published in part in Green 2012). I found that the textbooks privilege economic growth and consumerism and offer little to prepare students to understand sustainability issues or potential limits to growth.

Three populations linked to Econ101 at British Columbia's three largest public universities were interviewed. The first group comprised eleven economists who deliver the course. The second involved nine professors who teach undergraduates in programs that explicitly focus on sustainability and require that students take Econ101. The third consisted of 54 students who had recently completed an Econ101 course. I utilized qualitative research methods since they are considered appropriate in instances where the topic being studied has yet to be delineated through previous research and researchers are seeking to generate data about perspectives, experiences, and opinions.

Sustainability is not salient to Econ101 lecturers and they give little attention to the environment, public goods, or externalities, instead focusing on what they deem to be core theory (though two of the lecturers were troubled at how their colleagues downplayed the theory of externalities). They are concerned that incorporating sustainability into curriculum entails enlisting them in changing student values. There is limited willingness to revisit curriculum to better address the environment-economy nexus.

Professors from sustainability-oriented programs are dissatisfied with what their students get out of Econ101 and see it as steeped in ideology and problematic values like consumerism. They are seeking to offer alternatives to Econ101 that focus on the environment-economy interface, adopt a normative position of presuming that sustainability is desirable, and scrutinize the value positions of contemporary economic policies (e.g., at one of the universities such students can now take an ecological economics course instead of Econ101).

There was near unanimity amongst students that the environment is downplayed in Econ101, taking up at most two lectures per semester. Those from sustainability-oriented programs tended to be dissatisfied with the course and saw it as dismissing ecological limits to growth; they felt dissuaded from raising concerns about standard theory's limitations or philosophical questions about the purpose of economic activity. Student performance on a brief exercise designed to see if Econ101 helps prepare students to offer informed opinions on a public policy of contemporary significance (carbon taxes) was underwhelming.

My overarching findings indicate that universities' sustainability commitments have yet to influence Econ101 curriculum and that the curriculum does not support these commitments.

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