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REASSESSING CONTEMPORARY  
MACROECONOMICS ON METHODOLOGICAL  
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A WEBERIAN APPROACH

BY

PETER GALBÁCS

CHOPE WORKING PAPER NO. 2016-32

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CENTER FOR THE  
HISTORY OF POLITICAL ECONOMY  
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# REASSESSING CONTEMPORARY MACROECONOMICS ON METHODOLOGICAL GROUNDS

## A Weberian Approach

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**Abstract:** Through this paper I try to join the lively debate around mainstream economics with a view to calling attention to some methodological aspects. It is aimed at outlining an interpretation based on Max Weber's traditional neoclassical methodology that can help us to find the adequate territory of abstract-idealized mainstream models. Special attention is paid to an assessment of the relationship between models and reality. It is argued here that scrutinizing this relationship should reach beyond the oversimplifying approach of empirical tests. Simultaneously, this assessment gives us a refined set of aspects by the application of which to contemporary macroeconomics some methodological breaks can also be highlighted. These breaches would not have been revealed by a linear narrative.

**Keywords:** assumptions, realism, instrumentalism, abstraction, idealisation, mainstream economics

**JEL codes:** B22, B41, E32

Recently both professionals and the public have witnessed the continuous strengthening of sweeping and powerful critiques against mainstream economics. All these remarks reflect the dissatisfaction with the Americanized high theory which is alleged to have got lost once and for all and is claimed to require a fundamental revision. Blaug (2002) looking back upon the heritage of this dissatisfaction mainly points his critique at general equilibrium economics. In his view, this family of theories should primarily be blamed for the long process of sacrificing practical relevance on the altar of mathematical precision.

Critical voices against the main stream are basically of methodological nature, since these remarks are often formulated to underline the inadequacy of the relationship between models and reality. However, studying contemporary macroeconomics on grounds of philosophy of economics has remained a toy in the hands of a narrow circle of researchers. Only some of the results seem to be utilized by either modern macroeconomics or even its critique. Attacks pointed at the pureness of the theory (Mihályi 2013) naturally doom the critique to failure. Kornai (1971) is an excellent example for an ill-positioned and, consequently, fruitless critique (see Gedeon 2013 for more on this). So, there remain only the episodes of the misuse of mainstream models to rely on from which hardly does the fact of misconstruction of the theory follow. By the way, the disagreement mentioned above rarely takes on the form of a *debate* in a strict sense. Critics keep on repeating their well-known arguments which are left unanswered by the members of the mainstream movement. For the latter both the benefits and the purport of the science they do are obvious, so everyday practice in mainstream economics does really

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\* I am grateful to Mrs. Edit Koncz for proofreading the text. Errors remain my own. Any opinions expressed are those of the author and not those of the Center for the History of Political Economy, Duke University.

make sense for them. As a result, mainstream theorists tend to keep away from the allegedly nonsense confrontation.<sup>1</sup> However, this attitude can hardly bolster the position of mainstream economics including modern macro, since in the end the objections remain up in the air.

This paper is aimed at providing some methodological aspects by the consideration of which we can get closer to an in-depth understanding of the adequate use of mainstream models. Scrutinizing the usefulness of the existing paradigm must take precedence of the urge to form a new one. Without denying the extremely abstract nature of modern macro, here I make the case for *changing* relevance of these models—relevance that should occasionally be taken as existing. The nature of this relevance is also highlighted. *My surmise is that mainstream economics cannot be regarded as uniform in terms of the subtle relationship with reality.* Through these arguments I call attention to the need for an interpretation that is more differentiated than our current narratives. My key term below is *understanding*. It is demonstrated that as far as the understanding of some facets of socio-economic reality is concerned certain chapters of mainstream economics have particularly been successful and adequate, while others can really and evidently be regarded as failures. Moving along the Friedman–Lucas–RBC line I highlight the details how Lucas has followed and how RBC-theorists have abandoned the traditional neoclassical principles. As a by-product, a set of methodological aspects emerges that is handy for a careful assessment of the relevance of modern macro. The most important task here is to clarify *when* and *why* we should argue for the usefulness of a theory when it has only slight direct practical relevance. All this is necessary in order to prevent the imperative critical re-evaluation of the main stream from sliding into an uncritical refusal. *It is mistaken to infer the failure of the theory alleged to be a uniform scientific enterprise from the evident unsuitability of mainstream economics for certain purposes.* Only by keeping this in mind can we avoid the missteps of identifying the (possibly) correct mainstream economics with the (occasionally clearly) erroneous use of the theory and the refusal of the whole body of the system as useless. Otherwise the emerging theoretical proliferation would become hopelessly devoid of sense. Contradicting views may be expected to show up and in the debate neither of the parties would have ultimate grounds for reasoning.

In the case of mainstream economic thought its ideal-typical character seems to be so important a feature<sup>2</sup> (Weber 1917: 43–44) that it is highlighted even by some latest commentaries not critical of the theory (e.g. Angner 2015). It is not wise to form opinions on the use of ideal-typical concepts in economics without explicit references to the relevant works of Max Weber having given clear-cut methodological recommendations as to the topic. Putting the blame on mainstream economics for its ideal-typical character, however, sets limits even on the critique itself. If the main stream is identified as a discipline that operates with ideal-types, the lack of real existence of these concepts cannot justly be taken as a shortcoming. *This feature belongs to the essence of such concepts.* By citing Weber's methodology, it becomes clear, first, that the lack of real existence in the case of ideal-types is not a failure but an intended characteristic

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<sup>1</sup> Tom Sargent is an outstanding example of this mind-set. In an interview (Rolnick 2010) he was obviously not cognizant of the details of the critique against mainstream economics, and, after the interviewer's outlining those remarks, he put them down one by one to the account of the lack of understanding. He was particularly reluctant to bother to go over these comments.

<sup>2</sup> However, the relationship between the methodology of ideal-types and modern macroeconomics is often debated. Hartley (2001: 117) clearly disbelieves this connection, while Madra (2016: 98) is more willing to admit that. Thus on this conceptual ground the review of the methodological evolution of modern macro could not have been carried out yet.

and, second, that *understanding* in social sciences is an extremely complex notion that implies multiple strategies. It is true that ideal-types abundantly used in mainstream economics are not suitable for direct description. However, it must also be noticed that along a different epistemological strategy such concepts may prove to be handy or even indispensable. Contradicting the voices that criticize the main stream for using ideal-types can be completed in two interrelated steps. First, ideal-type as an instrument of comprehending (socio-economic) laws must be introduced and, second, the theoretical importance of the comparison between model and reality must be highlighted. Going along these lines some mainstream efforts that can really be regarded as failures in looking for some fundamental laws naturally become conspicuous.

### **The theoretical connection**

In the literature it has become a special heritage to judge the relationship between Weber and economics on the basis of Tribe's (2006) tiny paper. Accordingly, today we talk about a lost connection—while Weber's relevance for the broader social science community has never been questioned (Galbács 2016). As it is pointed out below, the methodological linkage is still strong in economics, even if explicit references are lacking. Actually, they become unimportant after outlining the directions of reception. Even if one can find some sporadic references to Weber in modern methodological questions, these works rather discuss his relevance outside the mainstream camp (Milonakis – Fine 2009), so the relationship between the main stream and Weber seems to be finished. For the sociologists (e.g. Trigilia 2002: 123; Bratton et al. 2009: 216–218), the connection between Weber and 19<sup>th</sup> century neoclassical economics is clear-cut, of course. It is probably due to the fact that Weber's works are still regarded as of primary importance. What is more, Gary S. Becker, who is often ridiculed by economists for his relation to the presumption of complete rationality, is judged here in a completely different way. He is referred to as a relevant contributor to the complex act of social scientific understanding (Alexander et al. 2004: 84–89). Simply put, the benefits for social sciences of assuming complete rationality seem to be better comprehended by sociologists thanks to Weber, as it is pointed out below. As far as his ultimate purpose is considered, Becker (1996: 155–156) himself is unambiguous when highlighting the reconstruction of the rational core of human actions as his main originality. At the same time, he supposes himself to be far from postulating complete rationality as an unaffected and visible feature of actual actors (Becker 1992). In his review, Jon Elster (1997: 763) brightly casts light both upon this peculiarity and the need I want to clarify here. For an in-depth understanding the reconstruction of this rational core is obviously not enough<sup>3</sup> on its own but required. A well-balanced reception is hindered further by the circumstance that Elster mainly features on the reading lists of departments of sociology.

True as it is that an institutional aspect is mostly missing from the repertoire of the main stream, the world view of the formalized main stream is quite impressed by Weber's methodology. Referring to Weber may seem a convenient defence strategy for mainstream economics in the course of which something is discovered *ex post* in order that it could be applied to mainstream methodology with hindsight. The big picture, however, is far more complicated. Compared to present circumstances, Weber was a more prominent constituent of the economic thought in at the time when “Chicago Economics” emerged which as an approach was both a fundament of

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<sup>3</sup> The review on Becker has an ambivalent standpoint due to pointing out both the shortcomings and the evident advantages. It is clearly expressed by the fact that right on the first page Elster uses the expressive adjectives “crude” and “jeune” to describe Becker's method.

the subsequent main stream and an academic educational method. The direction of influence must also be taken into account. Economics did *not* react to Weber's methodology. By contrast, it was Weber's ideal-type-methodology influenced by Menger that became the brief summary of conceptualizing in economics (Ringer 1997: 110). Thus referring to Weber's methodology is not an *ex post* apology for mainstream theory but rather a precise clarification of how theorizing is completed in economics or an explication of a careful and recommended usage.

During the interwar period economic thought in the United States was characterized by a colourful heterodoxy. For example, the institutional interest often missed today was quite strong and characteristic (see e.g. Rutherford 1997 for more on this). Neoclassical orthodoxy could only rise to predominance after World War II, marginalizing even the great figures of the preceding heterodoxy. Weber's methodology and comparative historical sociology (for the details see Weber 1906: 165–166) emerged in the debates of the 1920–1930s between neoclassical economics and institutionalism. It was Frank H. Knight who recognized the possibility of setting up a complex social scientific discipline on the basis of Weber's system by which the debate seemed to be transcended once and for all. Knight hoped to call attention to the fact that it made no sense to have quarrels about the primacy of the neoclassical and institutional approaches since both methods have their adequate scope in the analyses as complementary parts. In 1927 Knight translated Weber's general economic history (Weber 1927) and he himself planned to publish some further works of Weber, while in the meantime Talcott Parsons brought out Weber's most influential work, i.e. the protestant ethic (Weber 1930). After these first pieces the English editions of Weber's works came out straight off the reel (see Scaff 2011: 201–206 and Emmett 2006: 108–109 for great summaries). In those days Knight became a serious expert at Weber's theory. He is likely to have got acquainted with these works as early as in 1913 during his travels in Europe. The intellectual relationship with Weber remained in existence even after his homecoming due to his studies and professors. This relationship can also be scrutinized in a broader context, of course. Wagner (1976) gives a detailed description what a strong influence Germany had on US social sciences at the beginning of the 20<sup>th</sup> century<sup>4</sup> and what a serious part the sociologists of the University of Chicago bore in disseminating the ideas of Weber and other social scientists from Germany.

Knight is interesting for his being regarded today as an early founder of modern Chicago Economics. Even though assessing his relationship to the developments after the 1950s (i.e. after the foundation of the present-day school) is highly problematic and debated,<sup>5</sup> it is still undoubted that all the determinant figures of the approach maturing in the 1950s were his former students (Milton Friedman, Allen Wallis or Aaron Director<sup>6</sup>) and they all are likely to

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<sup>4</sup> Surprising as it is, economics and history were the first to come under this influence, followed by sociology later.

<sup>5</sup> For the details of this debate see Emmett 2009: 145–155. The neoclassicization of the 1950s made even Knight himself peripheral. Even though it is obvious today that his comparative approach is indispensable to the complex understanding of socio-economic reality, he could hardly gain adherents among his colleagues. In those days his system was not even regarded as a constituent of economics, rather it was pigeon-holed under the eclectic adjective of "social philosophy". Keeping the careers of Oliver Williamson or Goffrey Hodges in mind (see Csaba 2014: 63 for more on this) it is not surprising at all that at the end of his active years (i.e. in the post-war years) Knight partly fortified the department of philosophy, playing a role in public life as a founder of the Committee on Social Thought, accompanied by others disappointed in the aspectual turn in economics of that time (Emmett 2006: 118).

<sup>6</sup> As a statistician, *Wallis* greatly contributed to making the application of theoretical economics to practical problems widely used. *Director* carried out a fruitful combination of economic and legal education. The circle of students and colleagues around Knight was far broader (see Emmett 2015). For example, *George Stigler* and *Rose Director*, *Friedman's* later wife, were some of them. Interestingly, it was not the only marriage in the circle (*Homer Jones* and *Alice Hanson*), which excellently bolsters the view that the protagonists of the mainstream movement

have been profoundly familiar with Weber's theory due to Knight's interpretation. In Friedman's case, for example, the commentaries report quite a strong influence from Knight (Reder 1982: 6). Occasionally the relationship is clearly visible. The way Friedman (1953: 14) writes about the unrealisticness of the underlying assumptions almost directly echoes the pertinent parts of the famous objectivity study of Weber (1904: 80). The strength of this parallelism is not at all weakened by the fact that Friedman advancing Weber's methodology to be the cornerstone of his theory gave a reinterpretation of that from an instrumentalist direction. A strong influence is also apparent in Lucas' case. In one place Lucas, for example, regards models in theoretical economics as of *utopian* character (Typed Notes. Lucas. Various. Box 13, Directions of Macroeconomics 1979 folder) which was, however, the trademark Weber used in his comments on neoclassical economics (Weber 1904: 90; 1917: 42).

Over and above being an expert at the theory of neoclassical economics and its applications (cf. Stigler 1985: 3–4), Knight due to the Weberian principles also recognized the fact that the scientific scope of the theory is extremely limited (Knight 1999: 1–2). He realized that because of the consistent application of both the mechanical analogies and the physical concept of equilibrium economists cancelled some important facts and processes of socio-economic reality from their field of view. The relevance of this thought can hardly be overestimated as for present day economics. These details labelled as exogenous variables (e.g. state, law, norms and values) are swept under the rug for the sake of simplicity and mathematical tractability. They are rubbed out of the questions posed to the theory. These social facts cannot be comprehended through the preferred physical approach, while their importance in terms of understanding socio-economic reality can hardly be denied. Treading Weber's footsteps Knight early realized that neoclassical economics has been a mere ideal-type as an instrument in understanding but not as its ultimate purpose. That is, theoretical economics remains relevant as for comprehending reality without the real existence of the entities it applies. To be more specific, an in-depth understanding of our socio-economic reality *requires* us to *complement* the picture formed on the fundamental economic laws with the study of institutions. For Knight, theoretical economics gradually became a constituent in a broader approach or context in which analysing either social institutions or historical processes is equal in rank. Knight's relation to the Weberian methodology and, simultaneously, to neoclassical economics he himself also *defended* was ready at the beginning of his Chicago career. So in spite of his critical attitude Knight did not refute neoclassical economics (see Emmett 2006: 102 for more on this), but called attention to the need for a more careful interpretation and a more tinged application. First in German in 1930, then in English in 1935 Knight published his paper in which he clarifies the relationship between socio-economic reality and neoclassical economics in a way that is valid even to this day. By the time Friedman (graduated as a master in 1933) and his fellows became Knight's students, he had been able to convey clear-cut insights to them. Stigler (1985: 2) reported a dedicated Weber seminar from the mid-1930s which was focused on studying the original German edition of Weber's *Society and economy*, an extremely important text as for methodology (Scaff 2011: 209; 2014: 275). *So Knight's students were familiar with Weber*, and

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have formed a closed party. The strength of Knight's intellectual influence can mainly be attributed to the closeness of the formation around him (cf. Reder 1982: 7). From a shade later period (1941–1947) Don Patinkin (1973) can be mentioned here, whose book (Patinkin 1965) was eulogized by Lucas (1980: 701) as a work of fundamental importance in after-Keynes monetary macroeconomics. It is difficult to imagine that Knight would have wanted to hide his deep respect for Weber's social scientific approach from his most beloved and closely known students and colleagues.

not only could they get acquainted with Weber's views on social institutions or the emergence of capitalism but *they also had a considerable knowledge of both the idea of the division of labour between different scientific approaches and the methodology of constructing and using ideal-types*. For our current purposes the latter detail is of crucial importance. And although Knight's role in the rise of Chicago Economics and modern mainstream theory is likely to remain debated forever, putting emphasis on the conveyance of the Weberian methodological foundations may contribute to this debate with a new aspect. This contribution may be valuable especially due to the fact that in methodological terms the overseas commentaries tend to highlight rather the *break* between Knight and the moderns than a *continuity* to be carefully evaluated.

Instead of using the theory as an indirect instrument in understanding, Friedman (1953) made that a tool for direct use.<sup>7</sup> However, as it is pointed out below, the relationship between reality and mainstream theory is so subtle that the complexity of the parallel methodological approaches could hardly be summarized with the label of 'Chicago Economics' without serious distortions or oversimplifications. The fact that empirical performance became an important aspect, on the contrary to Knight's original ideas, makes the big picture more convoluted.<sup>8</sup>

### **Postulating socio-economic laws: the ideal-type**

Max Weber (1904: 88–89), who formulated the methodological recommendations as for the use of ideal-types in a way that is still valid, was critical of the burgeoning neoclassical theory of the time especially on the ground that theorists through the laws intended to grab the directly experienced reality. It is a similar problem when one directly advances his conclusions drawn from a pure theory to be real social requirements (Weber 1917: 44). By the way, the relation of neoclassical economics to reality is extremely problematic. The founding masters were precise to separate models from reality (see Galbács 2015: 24–25 for some details on this). John Hicks (1932: 42–56) was similarly correct to emphasize the fact that over and above pure theory plenty

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<sup>7</sup> The distinction, which is widely used in the North American works on the history of economic thought and methodology, between 'pure science' and 'applied science' also has to be mentioned here. The former refers to the interpretation Knight gave on neoclassical economics (i.e. theoretical economics is a basic framework built on the rationality postulate), while the latter refers to the tradition triggered by Friedman (1953) that advanced the application of the theory to real-world problems to be a requirement of primary importance. This differentiation, however, fails to reveal the fact that the organizing principle of the theory (i.e. theorems deduced from a set of axioms) remained the same all along. On this showing, an *applied theory/science* equals to an applied *pure theory/science*. That is, the real difference consists, first, in the limits one sets on the theory when it is applied and, second, in the principles of establishing the set of presumptions.

<sup>8</sup> In some modern American accounts on the history of economic thought this problem is solved by simply rubbing Lucas out of "Chicago Economics". Setting aside both the problem of *applied vs. pure theory* and the relation of the set of axioms to reality, this decision is grounded by the fact that in methodological terms Friedman followed in Marshall's footsteps, while Lucas has been a pioneer of neo-Walrasian economics. The latter tradition having started in the 1940s, however, is simply swept into the competence of the Cowles Commission and neo-Walrasians are placed into the intellectual sphere of Harvard/MIT (Emmett 2009: 151). On other occasions the significance of Lucas or Fama is admitted, while it is still Friedman's generation that is regarded as of high importance (Horn et al. 2011: xvii–xviii). Of course, the results of the Cowles Commission can hardly be separated from the success story of Chicago Economics. It seems to be a smart explanation that the pre-Lucasian neo-Walrasian economists regarded the empirical confirmation Lucas emphasized as inessential (they were quite right), while Lucas' empirical interest was a characteristic of the Cowles Commission (De Vroey 2016: 193–194). Whereas Lucas was determined to reformulate the problems Friedman had studied (Letter to Axel Leijonhufvud, October 28, 1982. Lucas. Various. Box 5, 1 of 2 1982 folders), modern narratives emphasizing new classical's being tied to multiple institutions do not refer to them as real constituents in Chicago Economics (Emmett 2010: 78). As far as the methodological breaks analysed here are considered it is important because this peculiarity can call attention to the methodological discontinuity between Friedman and Lucas.



of factors must be taken into account in case one wants to give account of the functioning of actual macro-systems. Hicks (1946) clearly distinguishes theoretical economics and institutional analysis and in terms of understanding reality the former is also regarded as relevant. However, De Vroey (2016: 4) suggests that Austrian economics having given the face of mainstream theory in the time of Keynes responded to the Crisis of 1929–1933 with completely mistaken recommendations, this development being the result of directly applying the same pure theory to reality. This seems to have been the momentum that forced Keynes to come up with a more de-idealized theory. This is the reason why it is to be emphasized that Weber’s methodology is *not* the methodology of contemporary mainstream economics – at least not in the case of all the chapters of the main stream. Instead, we had better take Weber’s system as an interpretative framework that can, first, serve as a manual to the correct way of use and that can, second, help us to clear mainstream economics of the charge of some alleged failures. At a theoretical level the distinction is not that complicated. *Mathematical formalism is not appropriate for us to solve every problem, so it should not be used where it does not seem to be useable.* However, it does not follow that mathematical formalism is completely useless for everything.

For economics, constructing ideal-types is the instrument of seeking laws. By means of using such concepts we try to understand the mechanisms of socio-economic reality and the underlying causal structure. An ideal-type in economics *per definitionem* summarizes the forms of human behaviour that *would* show up as the elements of strictly instrumentally rational actions under hypothetic conditions. Ideal-types for social science, interpreted in a broad sense to include both economics and sociology (Weber 1978: 18–19), should serve as bases of comparison *and nothing more*. By making comparisons, all the actions, events and forms of human behaviour emerging in real socio-economic environment can be interpreted as deviations from the well-defined ideal-types<sup>9</sup> (Weber 1978: 6–30; 1917: 43–45). The factors that deform ideal-typical actions and that can be taken into account are legion. As for understanding, Weber put the main emphasis on irrational mental factors, while some modern economists who pay attention to the aspects involved by institutional economics or transitology would be willing to extend their interests to cover the distorting elements implied in legal and political environment, culture and even history as well (Knight 1999: 167–168). In the meantime, Weberian irrationality occurring due to, say, incomplete information is still relevant.

*As far as rationality-based ideal-types are considered, it is to be highlighted in the first place that they are only methodological instruments but not manifestations of a world view.* Using the *homo oeconomicus* built on the elements of rational actions does not imply the assumption of complete rationality of real actors. Ideal-types do not directly describe reality, since their construction does not reveal anything about the extent to which processes of socio-economic reality follow complete rationality and other assumptions. However, and this is crucial, one can only shed light on the degree of accordance or divergence by contrasting reality with an ideal-type<sup>10</sup> (Knight 1999: 393). Declaredly one-sided ideal-types cannot comprehend or exhaust the

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<sup>9</sup> Such a comparison comprises, first, the registration of the *differences* between model and reality and, second, giving a causal explanation for these differences. It is far from confirming an abstract model with empirical data. Contrasting an abstract model with reality (that is, when the researcher is interested in the *similarities* and not in the differences) is particularly dangerous, because this may lead to one’s distorting reality in order that the validity of a theoretical construction could be confirmed.

<sup>10</sup> *Real economic actors do not display complete rationality and real economic processes are radically different from mainstream theorems.* It seems to be the most powerful argument for ideal-type-based models that such a

infinite abundance of reality. Without them, however, this abundance becomes impenetrable and meaningless or uninterpretable. Laws and theorems both in “pure” neoclassical economics and in its intellectual inheritor mainstream economics are no more than the rational core of a mass phenomenon. Within this rational core economists using economic-rationality-based concepts and formalized tendency laws try to analyse what the economic dimension of our society would be like if the theoretically set presumptions came true. These are the theorems of neoclassical-rooted economics that emerge as the completely rational limiting concepts of the market actions performed by the selfish agents taking their fellows’ expected behaviour as conditions into account. Unambiguity of our results is ensured by the clear-cut definitions and presumptions and the consistent application of mathematical deductions. To this extent, using ideal-types is a necessary methodological step, *while the complex act of understanding should obviously amount to much more than constructing ideal-types*. According to this view the effects of the social-historical-institutional factors that modify or distort the rational core framework can only be comprehended by contrasting reality with the theoretically generated rational outcomes. Thus this methodological approach is built on an explicit distinction between *hypothetic* and *actual* actions. The ultimate purpose is to make contrast and the more sharply the hypothetic outcomes differ from real situations, the easier it is to complete this comparison. *So it is a definite advantage of an ideal-type if it does not look like the real entities*.

Weberian-style ideal-types are the instruments of scientific realism after all (Knight 1999: 375–376), since either the rational core or the distorting factors are considered (Weber 1967: 52–53), the purpose in both cases is to reveal the causal structure underlying *real* social processes. Mechanisms highlighted by ideal-typical constructions somehow correspond to actual processes. Weber (1978: 10–11) interpreted Gresham’s law in a similar way. Some of the causes lying behind the phenomena are summarized in ideal-typical theories and models, while other causes are comprehended as the origins of the differences between actual processes and ideal-typical outcomes (Knight 1999: 392). Actually, this is the essence of *understanding*.<sup>11</sup> This is problematic indeed. How does rationality manifest itself in actual market actions and how does the *lack* of this rationality show up? As the rational core framework has to be adequate as for the causal structure, the differences from the hypothetic outcomes also require causal explanations. Ideal-typical concepts avowedly do not have real existence, since they all are built on the intended distortion of the entities experienced in reality (Weber 1978: 9). Consequently, their adequacy cannot empirically be proven or refuted either. The same is true of the consequences derived from ideal-types and postulated economic laws. Validity of pure economic models is not a question of empirical performance. To be more specific, confirming a theory that means verifying the causal structure we have highlighted is not necessarily possible on the basis of confronting theory with the data. Careful formulations are needed in order that presumptions of a theory could sensibly be sought in reality. This is the case of *rational plausibility* mentioned by Weber (1978: 11). Properly speaking, a researcher even *before* formulating his theory or model clearly knows the causal structure that is supposed to be underlie the situation he analyses. So without empirical evidence one’s best option is to be

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simple sentence often used for discrediting the main stream cannot even be formulized without having in-depth knowledge of economic rationality and its consequences. Of course, this knowledge comes from ideal-type-based theories. A historical-institutional approach does *not* make pure mainstream theorizing nonsense and useless but uses it as a *prerequisite* (cf. Weber 1949: 41–42).

<sup>11</sup> It is Knight (1917: 293–312) who gave economic thinking the idea that through abstract concepts we can have access to reality, i.e. to a hidden causal structure that is muddled up by contingencies.

satisfied with the belief that the description of a social action is *causally adequate*. Our last resort is the *adequacy in respect to meaning* between *surmised* causes and their effects.

It must be noted that Lucas (1990) himself uses economic models according to these principles. Whereas the adequacy of mechanisms highlighted in the simplest neoclassical growth models or the appropriateness of the assumptions are not questioned, it is not possible to provide a direct empirical support for the theorems, since macroeconomic processes are most likely to differ from the forecast patterns. Lucas takes some circumstances into account in respect to human capital or the general conditions of capital markets that may be blamed for triggering such differences. So an economist should constantly be cognizant of the relationship of his model with reality, of the differences in-between and of the way he has to give account of these discrepancies by modifying the early step assumptions or by building new determinants in. Mäki (1994: 245) gives an excellent description of the process of approaching reality step by step by changing the presumptions. Referring to such early step assumptions we can draw a sophisticated distinction between certain applications of the methodology well-known since the neoclassical era, as the discrepancies between model and reality can be treated in multiple ways. According to the Mengerian-Weberian methodology, the differences revealed by pure economic theory can be subject to further sociological-historical-political analyses, while Mill suggested modifying the set of assumptions *within economics* in order that models would get closer to reality (Blaug 1997: 54–56). No matter which of these non-conflicting procedures is chosen, it should go without saying: both the fact of the differences between model and reality and the need for treating these differences in scientific terms have been constituents in our intellectual traditions since the dawn of modern economics.

### **Applying ideal-types in economics**

So it does matter what we think about the laws described in our theories. Even though neoclassical economics itself came into the Platonic tradition of Newtonian mechanics that set itself the task of grasping the *essence* underlying chaotic phenomena,<sup>12</sup> we have no access to this essence. In spite of our desire to believe in the existence of eternal and never-changing laws, in the existence of an essence to reveal, we cannot exceed our own concepts that we put into reality. This is the direct consequence of the epistemological limits uncovered by Kant and accepted even by modern philosophy of science (Kant 2004: 37–72; Cassirer 1981: 166–167). These limits, of course, build solid walls around economic realism and actually around scientific realism as such. Consequently, the task of social sciences cannot be other than the *analytical ordering of empirical reality* (Weber 1904: 58–63). In the case of economics this task emerges as identifying formalized laws and tendencies. However, such laws in a strict sense cannot be regarded as *objectively existing rigid* laws—they may be, but due to the epistemological limits on us one cannot make a judgement. Thus a realist social scientist has no genuine options beyond constructing concepts. Through these concepts he makes efforts to grab the laws that are believed to be “out there” deep in chaotic social reality. So there is no contradiction between the Platonic traditions penetrating mainstream economics and ideal-type-based understanding. The ideal-type-grounded strategy of understanding clearly signs where our epistemological boundaries run.

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<sup>12</sup> Without explicitly referring to this tradition, Knight (1999: 386–388) picks the idea of *circle* often cited in Platonic literature as an example for the relationship between economic thinking and reality. Empirical circles more or less *share in* the attributes of circle as an *ideal shape*.

Even though Weber strongly relied upon Menger's methodology of constructing ideal-types, they seem to have attributed different ontological statuses to such types. Actually both of them discussed the same thing. For Menger the entities of economics are really "out there" in socio-economic reality. By contrast, Weber emphasizes that through ideal-types one can select some *existing* elements of socio-economic reality from their actual context. These types, however, cannot directly be found in reality, as Menger also suggests it. It is difficult if not impossible to imagine how such an ideal-type as *capitalism* often mentioned by Weber can have real and objective existence that is independent of the constructing mind, but this would follow from taking Menger's standpoint literally. For Weber the rational core selected by neoclassical main stream is not the essence of human behaviour (cf. Weber 1904: 72–73), but a facet of real human actions that is given central significance by economics—that is *important* for economics. On the basis of the superficial difference, Mäki (1997) brands Weber as an instrumentalist, whereas both Menger and Weber insist on connecting ideal-types and, consequently, theories to reality. The conceptual pair of *abstraction* and *idealisation* highlighted in contemporary philosophy of economics as a guarantee for realism equally belongs to both Menger and Weber. The above confusion is likely to come from the fact that Weber accentuates the Kantian epistemological limits as well that were neglected by Menger. Weber having placed the methodological principles in a broader context provided a more precise re-interpretation of the technical details that Menger clarified for economics (Caldwell 2004: 91), but their realism is indisputable. Economics is the science of understanding reality and our discipline serves this purpose by selecting and highlighting certain laws and tendencies—and these laws and tendencies can be recycled by other social sciences.

Giving a comprehensive and complete description of reality is impossible. Its complexity *would* make it nonsense (Weber 1906: 169–170), and because of the epistemological boundaries it is naturally a hopeless ambition. So economists are forced to make selections, that is, we need to decide what hidden mechanisms we want to reveal. Without this discretion we would be compelled to give immense descriptions of reality that would not be clearer and better-organized than reality itself. Selection and simplification are necessary, and for this purpose it is constructing ideal-types that is our most effective option in a twofold sense. First, ideal-types can be used for highlighting laws and, second, for comparisons supporting causal explanations. Bringing certain laws to the fore is dependent on the researcher himself (Weber 1904: 82; 1917: 21–22), and when making this decision he is under the influence of some social factors. First of all, he is influenced by the scientific environment in which he has become socialised, however it is clear that the "big questions" bothering the researcher's broader social environment also have an impact on him. This is the reason why we can talk about schools of economic thought at all. Even though it is the individual who serves as a direct articulator, the directions of interest and the ways how a researcher formulates his questions are governed by the ultimate foundations rooted in his scientific-societal environment.

By being built on the postulated law-like tendencies mainstream models based on ideal-types can only highlight some mechanisms from the infinity of causal relations<sup>13</sup> of real socio-

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<sup>13</sup> The same way of thinking appears when Weber made efforts to look into the formation of capitalism. His explanations are not inconsistent, even if Weber sometimes emphasized the significance of religious ethic (Weber 1930) and sometimes the material-technical prerequisites (Weber 1927). Weber in these works of his tried to reveal different *but simultaneous* factors of the emergence of capitalism.

economic systems, that is, of *the region of economic events and institutions*.<sup>14</sup> Strictly speaking, these mechanisms are also of postulated nature. However, we should not attribute *directly experienceable existence* to these concepts or postulated mechanisms. As Weber (1904: 81) formulates: all knowledge of cultural reality is always knowledge from *particular points of view*. This sociologically established point of view is expressed by the researcher through his unique interest, that is, through the way he approaches reality by using his concepts.<sup>15</sup> There is only *one* reality, no matter how variable it is, but there is an infinite number of possible approaches—and this is the ultimate reason for the researchers to conceive new theories or models to understand the *one and only* reality. *It is a mistaken and oversimplifying interpretation in which the findings of some economists approaching reality through general equilibrium models are regarded as irrelevant because of the obvious disequilibrium situations experienced in real macro-systems*. This severe judgement is mainly passed on new classical macro for its modernity. However, it has *not* been the real issue whether we should regard the natural equilibrium stages of real macro-economic systems as axiomatic. Equilibrium in models is of axiomatic character indeed, however, from this no inferences can be drawn with regard to either the conditions of real macro-systems or the existence of built-in equilibrium mechanisms or even the desirability of such mechanisms (cf. Weber 1904: 80–81). According to the Weberian principles rational economic calculation is not the most important driving force of real-world human behaviour at all (Weber 1917: 38). Thus general equilibrium is not a real phenomenon but a principle or rather *a way of looking at things* by which we try to understand reality and the consequences of the mechanisms working there (Snowdon – Vane 2005: 281). So, and this is particularly uncomfortable for the critics, keeping the underlying methodology in mind these models cannot validly be rejected on the basis of the mere unrealisticness of the presumptions. A valid rejection requires a far more careful analysis of the relationship between model and reality (suggested below). Over and beyond discovering some parts of the causal structure, the most we can declare as to reality is that some consequences *would* be experienced *if* the assumptions we applied in theory were directly and unaffectedly experienced in real macro-systems as well (Weber 1917: 37–38). For Americanized modern macroeconomics a model and reality are two distinct things. As to the latter, we apply some simplifying assumptions for the sake of tractability. In this way, we can study the logical consequences of some mechanisms which are important for us in terms of understanding reality. However, these artificial economies must not be mixed up with real macro-systems (Lucas 1980: 696). The significance of the postulated mechanisms makes a difference, of course. But anyway, the presumptions underlying mainstream models (e.g. the rationality of agents) apply to models, so it makes no sense to call for their *direct* real existence (Lucas. Various. Box 13, Barro folder).

Whereas the neoclassical founding masters could considerably extend our knowledge of the necessary and sufficient conditions of macroeconomic equilibrium, Keynes reacting to the acute

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<sup>14</sup> For Weber (1904: 64–89) this is the territory of mainstream economics (1). Here he applies a threefold terminology in order to give a precise distinction of the tasks of social sciences. In these terms we also have to distinguish (2) phenomena and institutions that are *economically relevant* but not of economic nature and (3) the phenomenon and institutions that are not economic but *economically conditioned*. The latter may include even arts, so the scope is extremely wide. It is evident that *economic interest* used in a broad sense could be taken in the social institutions that can modify the functioning of our fundamental economic laws. However, it does not belong to the field of mainstream economics.

<sup>15</sup> This point of view can also be extended to cover the consideration of mainstream economics. Following classical mechanics mainstream economics set the task of seeking laws, which is nothing else than a specific interest. Applying this line of reasoning we can easily clear the main stream of the charge of narrowing down.

unemployment difficulties of his age simply relocated the problem by changing the way of addressing the issue. On the basis of a fairly de-idealized theory Keynes was curious which institutional factors have effects on the equilibrium tendencies of macro-systems.<sup>16</sup> New classicals extended the then twofold approach (*neoclassical static equilibrium* vs. *Keynesian disequilibrium economics*) to be a threefold system by studying whether there are *some* elements among the structural factors scrutinized also by neoclassicals that can trigger large-scale fluctuations. Lucas suspected that such economic fluctuations may have some of their origins right in the dynamic economic system supposed to be perfectly working (cf. Plosser 1989: 52–53). In other words, business cycles seemed to be the *consequences* but not the failures of optimizing. On this showing, Keynes and new classicals scrutinized the same thing. They all were interested in looking into the cyclical fluctuations experienced in macro-systems. Keynes studied the role played by institutional factors, which were *admittedly and intentionally* neglected by neoclassicals. By contrast, Lucas and his followers highlighted the fact that it is not necessary for us to regard business cycles as disequilibrium situations, since such fluctuations may occur as the consequence of optimizing behaviour while the majority of the classical presumptions are kept intact (Sargent 1977). *Following their own interest*, new classicals supposed the factors triggering business cycles to be somewhere else—but this is the only difference.<sup>17</sup> Without considering the epistemological driving forces behind concept-construction we would inevitably suppose the evolution of the economic thought to be linear and the successive theoretical constructs to be overwriting one another, to be hypotheses aimed at looking for the *only* truth. Actually, the relationship is *complementary*, that is, only together can these models deepen our understanding of real-world macro-systems without casting inconsistencies. This is the reason why the rivalling theories that actually highlight different facets of reality merge into one another to form a coherent big picture in the end (De Vroey 2016: 142; Blanchard 2008: 5). A causal analysis of the phenomena experienced in reality is endless, consequently we have to be satisfied with partial truths. This is the reason why we are always in need of new approaches that can uncover some new aspects of the same phenomena. The way we construct concepts is dependent on the way the problems are addressed, but these problems always vary with the content of the cultural-scientific environment itself (Weber 1904: 105). Actually, it is the abundance of scientific approaches that is indispensable for us to understand new aspects of reality. It is impossible to overemphasize the fact that assuming involuntary unemployment away in models does not imply the rejection of regarding involuntary unemployment as one of the real and acute social problems (cf. Lucas 1987: 48–53). This text also sheds light on the way the interest a researcher takes guides the process of concept-construction and model-building. For Lucas the problem of unemployment was unimportant, so he started from general equilibrium to study how the analysis of equilibrium can enhance our knowledge of the business cycle. So the change of theories and their abundance

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<sup>16</sup> There is a tendency to draw a sharp distinction between Keynes and neoclassicals. One of the briefest and clearest exposition of this idea is from Hicks (1946: 4–7). According to him, in a strict (neoclassical) sense Keynes was not an economist, since he was engaged in taking the effects of the institutional environment into account, whereas he did not make any efforts to build macroeconomics on a choice theoretic framework (cf. Plosser 1989: 52). It is an interesting detail that in the extinction of the interwar pluralism and consequently in the triumph of neoclassical mainstream Keynes himself also played a role. With a single blow he could uncover the modifying mechanisms the study of which could have given institutional economics a great deal to do for a long time (Rutherford 1997: 188). Just for the sake of emphasis: we are far from declaring an institutional approach excrescent or mistaken. It is only something *different*, that is, it helps us to answer other questions.

<sup>17</sup> As Kydland and Prescott (1996: 73) put this idea forward: “A model environment must be selected based on the question being addressed.” This is a clear, Weberian message.

are beneficial—and it is exactly this strategy that can considerably deepen our understanding of reality. The ambition to broaden to totality an aspect that is believed to be the only justified point of view is a failure.

## Assessment

*Selection* or isolation (i.e. abstraction and idealisation) is highly important as to constructing theories. Even though Weber could not have been cognizant of this fact, contemporary debates in philosophy of economics have been around this strategy, that is, the ways of setting up the underlying presumptions. Theories and models built on ideal-types have an empirical-logical core. To be more specific, researchers depict such mechanisms and relationships in abstract-idealised forms that are *either* visibly *or* evidently<sup>18</sup> constituents in the course of experienced social facts (Weber 1904: 90). This is obviously not the case when an economist following instrumentalist principles is allowed to set up the underlying presumptions in one way or another. *Only* characteristics and mechanisms that actually work in reality can be used in the construction and this is the criterion of substance in clarifying the relationship of economic models to reality<sup>19</sup> (Weber 1906: 173). *Understanding reality is only possible by using models connected to reality.* This abundance of aspects obviously contributes the possibility of providing multiple approaches to socio-economic reality. Artificial systems resulting from model-construction are simpler than real macro-economies. However, in the relevant aspects<sup>20</sup> they bear resemblance to their real counterparts by omitting the features that are regarded as unimportant and that are supposed to prevent the characteristics and mechanisms of central importance from clear manifestation (Typed Notes. Lucas. Various. Box 13, Directions of Macroeconomics 1979 folder). Lucas by emphasizing the requirement according to which models should be aimed at selecting some *important-relevant* features of *real* societies directly echoes Weber's methodology of building theories on actual attributes and at the same time places his own models under the label of *surrogate systems* (Mäki 2009). Models are only useful if to a considerable extent they are like macro-systems we want to study and understand (Typed notes. Lucas. Various. Box 27, 1 of 2 Adaptive Behaviour folders, 1985–86). That is, the purpose of creating simple-simplifying systems is always to support our understanding of more complex structures. *How to achieve realist purposes by using isolation-based models of which distorting real features is an essential character?* It is exactly the central problem of contemporary macroeconomics. In other words, it is about how to construct models that cannot directly describe our actuality, whereas they can still convey meaningful messages as to reality due to their being built from that. On multiple occasions Lucas explicitly clarifies the

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<sup>18</sup> Here *visibly* refers to such mechanisms and relationships that are empirically experienceable, while *evidently* to ones as to which it is logically obvious that they are working (cf. Knight 1999: 378–381).

<sup>19</sup> The clinching argument (Weber 1904: 90) is worth quoting at length. “[The] relationship [of abstract economic theory] to the empirical data consists solely in the fact that where market-conditioned relationships of the type referred to by the abstract construct are discovered or suspected to exist in reality to some extent, we can make the *characteristic* features of this relationship pragmatically *clear* and *understandable* by reference to an *ideal-type*.” The relative pronoun “where” above is of crucial importance, since this provides the selective criterion for drawing up the set of presumptions. Weber's further examples support this impression. Either city economy or handicraft is considered (Weber picked these concepts for demonstration), when constructing ideal-types the purpose is always to accentuate certain “existing” and “retraceable” features of reality. As Weber (1904: 91) formulates: “[Each utopia] has really taken certain traits, meaningful in their essential features, from the empirical reality of our culture [...]” As far as economic realism is considered, this hint is of crucial importance.

<sup>20</sup> Here *relevant* refers to the aspects which we are interested in and which are important for us.

requirement of connecting to reality, which is far more than accentuating mere empirical performance.

However, we had better avoid suggesting the idea that every chapter of mainstream economics is built around this principle. *The way concepts are constructed is actually the way we connect to socio-economic reality.* As we have seen, ideal-types constructed to highlight economic laws are aimed at uncovering the hidden causal structure. At the same time, analysing the differences between empirical reality and models is also a constituent of the causal analysis. This task, however, becomes nonsense when our concepts do not stem from reality. In such a case the mechanisms they underlie cannot highlight laws supposed to be actually working. Differences of empirical reality from such mechanisms can hardly deepen our understanding. It is exactly this recognition that gives us the criteria for evaluation. According to the Weberian methodology, the most profound requirement on theories is to bring an *actually* working mechanism to the fore. As long as a theory fulfils this precondition, it is completely mistaken to question its adequacy. *Evaluating each school of mainstream economics must be carried out on methodological grounds where the real issue is whether the concepts and the postulated mechanisms are connected to reality in a Weberian sense.* Empirical performance and the lack of some direct realism are only of secondary importance.

Not only are the effects of Friedmanian instrumentalism on the origins of new classical theorizing documented in detail (Galbács 2015) but the sharp demarcation of new classicals from these principles are also analysed (Galbács 2017), so it is enough to call attention to some additional considerations. As there is a methodological break separating Friedmanian orthodox monetarism and new classicals, a similarly radical methodological turn makes a hiatus between Lucas' circle and RBC-theory. After Hume's time, the cyclical effects of money on real economy have become one of the fundamental problems for theoretical economics and an idea has emerged that growth theories and theories of large-scale fluctuations should be regarded as distinct areas. RBC-theorists changed this setting with a single blow. Even though integrating growth theories with theories of the business-cycle (see Kydland – Prescott 1982 for more on this), that is, tracing back large-scale fluctuations to shocks affecting growth factors is not without precursors (see Kydland – Prescott 1996: 76 for some details on the antecedents), the real problem is that the shocks to tastes or technology/productivity accentuated by RBC-theorists can doubtfully be regarded as the triggers of short-run cyclical fluctuations (Blanchard 2008: 6). Even the authors themselves were uncertain of the reliability of the time-series data on technological shocks. Theorizing in an instrumentalist fashion (De Vroey 2016: 274–275 introduces RBC-theory as the culmination of Friedmanian instrumentalist tendencies) and realizing how well their systems perform in empirical terms, they easily left these concerns behind (Plosser 1989: 62–63). In spite of all their objectionable methodological decisions, RBC-theorists were still careful and wise *not* to regard these real shocks as the *only* triggers of the business cycle (Long – Plosser 1983: 68). They were right for sure. So it is nothing more than scrutinizing whether there are further possible causes of large-scale fluctuations over and above the monetary shocks the main stream has already analysed however far they were from a complete account. It is exactly the ground on which Lucas (1993: 13) stands up for RBC-theory. Economists have still admitted that business cycles have monetary causes. According to this comment of Lucas RBC-theory emerged under such macroeconomic conditions that shock-free monetary policy made it possible to reveal some further triggers of large-scale fluctuations. By contrast, monetarists could scrutinize the real effects of money in times when the impacts of the Solow-residuals were negligible, so these impacts did not interfere with the



mechanisms being at the core of monetarism. The fact that the causal structure underlying experienced macroeconomic phenomena is far more complex than *that is* (or *that can be*) accentuated in economic models is clarified even in Lucas' island models (Lucas 1973: 331–332). Thus it is no wonder that in this context good empirical performance of models means that a considerable relationship was detected between price dynamics and the cyclical component of quantity supplied. In statistical terms, however, this relationship was not that strong, making room for the effects of the omitted explanatory variables. Going over the econometric results it becomes obvious that Lucas' purpose was to provide empirical tests of the mechanisms he highlighted. No clues can be found as to his ambitions to sacrifice solid economic theorizing for mere empirical performance. Here a theory set up following a traditional methodology was econometrically tested.

## Conclusions

My conclusions are ready now. On the basis of Weber's economic methodology, I called attention to the idea that the traditional interpretations provided on mainstream economics can only be kept intact at the price of substantial intellectual distortions. The argumentation above was aimed at making the point that carefully interpreted and applied mainstream models are not inconsistent with the institutional approaches put on a different aspectual footing. Actually, mainstream economics should be regarded as a valid basis for institutional theorizing. Without having any conflicts, we are not forced to make any decisions. Whereas institutional or behavioural economics have succeeded in complementing the admittedly deficient way of mainstream thinking, neoclassical-rooted economic theory is still strong, no matter how many professionals dream of entombing that. Its fundamental purpose is nevertheless achieved. These models make up the valid rational core of real social phenomena.

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