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Table of Contents

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Spiethoff's Economic Styles: a Pluralistic Approach?

Sebastian Thieme, Schasching-Fellow 2015/2016 of the Catholic Social Academy of Austria, Vienna, Austria.
sebastian.thieme@mem-wirtschaftsethik.de

Abstract

The main scope of this article is to introduce Spiethoff's economic styles approach to economists outside the German-speaking scientific community. It also provides a contemporary (new) reading and interpretation of this approach, especially regarding the methodology Spiethoff used in his research. Essentially, it will be shown that Spiethoff applies a kind of 'abductive' thinking that is usually ignored. Especially in the recent debate about pluralism in economics, the dichotomy of induction and deduction excludes the concept of abduction. Against this background, the article highlights what Spiethoff's economic styles approach could add to the current debate regarding pluralism in economics.

Key words: economic styles approach, methodology, abduction, German historical school

JEL codes: B250, B41, B520, Z130

1. Introduction

Criticism of established economic concepts is not new itself. What is new, however, is how criticism has been voiced since 2007. When the worldwide financial crisis began, the criticism of 'the mainstream'[1] was no longer restricted to the academic discourse, but moulded into a public debate about the artificial, unworldly or non-realistic character of economics and the ignorance of how much the theory of economics diverged from 'economic reality'.[2] The marginalised specialisations or 'schools' of economics, often called 'heterodox economics', perceived and empowered this criticism in the aftermath of the crisis. For instance, Frederic S. Lee (2012, p. 342) stated that the current standard of economics would produce 'pseudo-knowledge'. Tony Lawson (2006) implied the non-realistic view of the current standard of economics by its ontology (closed economy, atomism and isolationism) before the financial meltdown. Other heterodox economists suggested a reorientation of economics on real (world) problems (e.g. Dequech, 2012; Lavoie, 2012), which also implies that the currently established economic approaches are abstract and unworldly.

[1] The terms 'mainstream' and 'standard' are synonymously used within this text and follow Dequech, who defined: 'Mainstream economics is that which is taught in the most prestigious universities and colleges, gets published in the most prestigious journals, receives funds from the most important research foundations, and wins the most distinguished awards.' (Dequech, 2012, p. 354) This definition is obviously coloured by the concepts of Thomas S. Kuhn (paradigm) and Imre Lakatos (research programme), but Dequech's definition seems to be sufficient for the appropriate understanding of 'mainstream' and 'standard' as it is used in this article.

[2] For the case of Germany, this criticism of public opinion is shown by the newspaper articles of Ahmia (2008), Storbeck (2009), Dullien (2012), Fischermann and Pinzler (2012). These German newspaper articles are listed in the references by their original German titles.

Surprisingly, the general criticism of economic theory did not consider sufficiently that the history of economic thought bears some approaches to deal with 'real-world' economics and the uses that could be made of them. One of these widely neglected approaches is Spiethoff's concept of *economic styles*. To put it in a few words, an economic style is the description of the entire economic life based on its characteristics at a certain time and within a certain culture and region. Any economic theory has to be based on a certain economic style; a theory's validity is limited to this certain style. Hardly any article links Spiethoff's economic styles approach to the current debate about pluralism in economics.[3] Therefore, one of the main purposes of this article is to show that Spiethoff's approach can add plenty to the debate about pluralism.

The concept of economic styles was mainly brought up by German economists such as Arthur Spiethoff, Alfred Müller-Armack, Heinrich Bechtl and Hans Ritschl. Among these economists, Spiethoff is the most important proponent of the economic styles concept (Schefold, 2015[1994], p. 65). His concept is also regarded as the most systematically and elaborated one (Klump, 1996, p. 15). Consequently, the article intends to introduce Spiethoff's economic styles approach to a non-German-speaking audience, putting the prime focus on its *methodological aspects* based on the original texts of Spiethoff.[4]

In order to restrain the expectations about what follows and to avoid disappointments – especially with respect to remarks about the history of economic thought – the following paragraphs shall clarify what this article does *not* address or deal with.

First, although there is a lot of secondary literature about economic styles in general (such as Kaufhold, 1996; Schefold, 2015[1994]; Rieter, 2002), a related literature review almost certainly leaves the readers with Gioia's (1997) impression that most authors do not focus on the methodological aspects of Spiethoff's economic styles approach. Consequently, secondary literature will only be considered if it contains aspects important for the purpose of this article.

Second, a comparison with other economic styles approaches (e.g. by Bechtel or Müller-Armack) as well as Werner Sombart's approach to economic systems is not intended. Note that this is already a topic within other articles (e.g. Rieter, 2002; Schefold, 2015[1994]) and especially within Spiethoff's own writings.

Third, today, Spiethoff is known for his research on economic crises and business cycles. However, exactly these subjects overshadow other contributions to economic theory, especially the methodology of his approach which shall be discussed here.

Fourth, researchers often apply the concept of economic styles to interpret 'real facts' but rarely use it as a systematically developed framework as provided by Spiethoff's categories, subcategories and features of economic styles (see subsection 4.4 and table 2). Consequently, a meta-study concerning the application of the economic styles concept is required, but that goes beyond the scope of this article.

As already mentioned, this article focusses on the *methodological aspects* of Spiethoff's economic styles approach and the meaning of this approach for the current debate about pluralism in economics. The following issues will be addressed in detail: some biographical notes about Spiethoff and the theoretical background of the economic styles approaches (section 2); the theoretical framework, i.e. Spiethoff's differentiation of theory (section 3); the description of the economic styles types (section 4); a discussion about which reasons exist for the small attention to economic styles today (section 5) and the meaning of

[3] This current debate about pluralism has been stimulated through the *International Student Initiative for Pluralism in Economics* (ISIPE, 2014).
[4] Spiethoff originally described his concept in 1932 and modified it around 1952 which was then partly published in several English papers (Spiethoff, 1952; 1953; Redlich, 1970).

the Spiethoff economic styles approach for the current discussion about pluralism in economics (section 6).

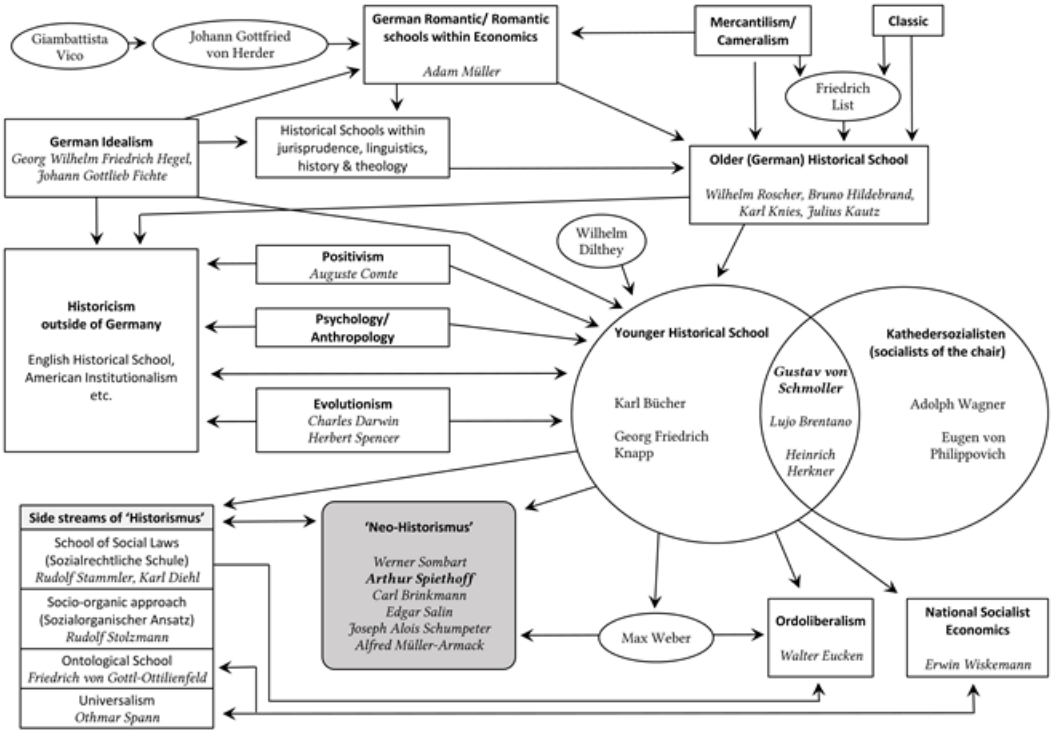
2. Biographical Notes and the Background of Economic Styles Approaches

Arthur August Caspar (Kaspar) Spiethoff was born on the 13th of May 1873 in Düsseldorf (Rhineland).[5] He attended grammar schools in Berlin and Mühlhausen (Thuringia). From 1893 to 1899, Spiethoff studied '*Staatswissenschaften'* (political and cameral science, i.e. economics) at the Friedrich Wilhelm University in Berlin (which was renamed 'Humboldt University' after 1945), where he attended lectures by Adolph Wagner (1835-1917) and Gustav von Schmoller (1838-1917).[6] Later, Spiethoff became assistant to Schmoller and participated in editing the journal *Schmollers Jahrbuch*. In 1905, Spiethoff finished his doctorate on business cycles. After he received the '*Habilitation*' (i.e. the qualification as a university lecturer) in 1907, Spiethoff was appointed to the chair of political economy at the German Karls University in Prague. He rejected appointments to both the chairs at the Universtiy of Gießen (1913) and the University of Göttingen (1918). Finally, in 1918 he accepted a position at the University of Bonn. In 1925 Spiethoff successfully supported Joseph Alois Schumpeter (1883-1950) to become a professor of economics at the University of Bonn.[7] Some chroniclers of the history of economics at the University of Bonn admiringly described the era of Spiethoff, Schumpeter and Herbert von Beckerath (1886-1966) as the 'triple star' (Kamp, 1970, p. 55) and 'the great time of Bonn' (Krelle, 1985, p. 12). However, this era declined with the emigration of Schumpeter (1932) and Beckerath (1934). Spiethoff worked at the University of Bonn until his retirement in 1939 and died in Tübingen on the 4th of April 1957.

These biographical notes and the mention of personalities like Schmoller, Wagner and Schumpeter may lead readers to question the place of the economic styles approaches and Spiethoff in the history of economic thought. Firstly, there is no 'school' of economic styles in the same way the term 'school' is usually used, for instance, the Austrian School of economics, the old institutionalism or Neoclassicism. 'Economic styles' is rather a generic term linking several economists who worked on economic styles approaches. Spiethoff clearly belongs to the 'economic styles' scene of economists which can be categorised as a part of '*Neo-Historismus'* within '*Historismus*' (German Historicism).[8]

[5] For biographical details see *The History of Economic Thought Website* (2018) and Schefold (2010).
[6] Both economists (Schmoller and Wagner) were important representatives of the German Historical School and (co-) founders of the professional organisation of German-speaking economists called '*Verein für Socialpolitik*' (Association for Social Policy).
[7] Readers interested in the relationship between Spiethoff and Schumpeter are referred to Kurz (2010).
[8] The German term '*Historismus*' is often translated in English as 'historicism'. However, some authors are hesitant to accept this translation because '[t]he term "historicism" in English has acquired a large number of meanings which the term "Historismus" did not posess' (Iggers, 2011, p. xiii) To avoid misunderstandings the following text explains how German experts of the history of economic thought use the term and, therefore, the text refers to the original German terms '*Historismus*' and '*Neo-Historismus*'.

Figure 1 *Historismus*, including German Historical School and '*Neo-Historismus'*



Source: Author's picture based on Rieter (2002)

Figure 1 shows how Spiethoff is categorised in the German *Historismus* which shall be explained in the following paragraphs. First of all, *Historismus* (German Historicism) is a generic term which includes different 'schools', movements and economists in the German-speaking area (such as Friedrich List, the Romantic School of economics and the German Historical School of economics). Rieter (2002, pp. 136-147) defined the German 'Historismus' by a certain 'style of thinking' (Denkstil): this way of thinking is *relativistic*, *holistic*, *organismic and anti-mechanical* (e.g. analogies to biological phenomena are preferred), *evolutionary* (interest in developments/dynamics), *ethical/normative* (i.e. economic and social developments are not only described, but also judged), *empirical* (because inductive research is preferred to deductive theorising) and *oriented to humanities and cultural sciences*. Consequently, these attributes are inherent in the economic styles approaches and Spiethoff's approach.

Furthermore, Spiethoff can be set in relation to the term 'Neo-Historismus' under which Rieter (2002, p.133) mainly subsumed personalities representing descendants of the (younger) German Historical School such as Werner Sombart and Joseph Alois Schumpeter.[9] Spiethoff's aforementioned working relationship with Schmoller proves a particular influence of the German Historical School of economics.

Unfortunately, Rieter (2002, p.133) does not distinguish between the various economic styles approaches and thus lets them fade into one of many sub-branches within the German '*Neo-Historismus*'. Moreover, Rieter (2002) only mentions Spiethoff, Alfred Müller-Armack (1901-1978) and Werner Sombart (1863-1941) as economists who were

[9] As Backhaus and Hansen (2000) explained, the attribute 'younger' in 'younger German Historical School' was pejoratively used by its opponents, for instance, Schmoller, in order to devalue and discredit the German Historical School.

working on economic styles approaches. As indicated above, a few more economic styles concepts exist. For instance, Spiethoff himself discussed the styles concepts of Heinrich Bechtl, Hans Ritschl and Alfred Müller-Armack. Spiethoff labelled these approaches as 'cultural styles' in order to distinguish those ideas from his concept of 'economic styles' (Redlich, 1970, p. 651). He also mentioned Sombart's concept of an 'economic system' which is similar to the economic styles approach (e.g. Spiethoff, 1932; Redlich, 1970, p. 652).

In addition to the aforementioned 'style of thinking' which characterised the German '*Historismus*', other elements are mentioned in the secondary literature to specify the general character of economic styles concepts. According to Schefold (2015[1994], p. 25) and Kaufhold (1996, p. 27), both the term 'style' and the economic styles mode of thought, intend to consider and describe the *Zeitgeist*, i.e. the common expressions and attitudes which characterise an epoch. Additionally, the 'styles' were supposed to be a promising base for an economic theory specific to the *Zeitgeist*/epoch (Kaufhold, 1996, p. 27). According to Rieter (2002, p. 159) and Kaufhold (1996, p. 34) economic styles approaches try to reconcile theoretical analysis (associated with 'deduction') and historical analysis (associated with 'induction').[10] In general, the description and discussion of economic styles in the literature (e.g. Rieter, 2002; Kaufhold, 1996; Schefold, 2015[1994]) give the impression that the research on economic styles was a more systematic approach than the research of the German Historical School. For instance, Spiethoff developed categories to determine how a certain economy is embedded in a certain time, culture, region and the like. Finally, in contrast to the prevalent stereotype about the German Historical School, i.e. hostility against modelling, Spiethoff's economic styles approach allows for theorising and modelling.

Although Spiethoff's economic styles approach has been categorised as belonging to the German history of economic thought, the discrepancy between his work and the general hostility of the German '*Historismus*' to modelling, affords some further remarks. This hostility is a very popular attribute of the German Historical School and seems to be very appropriate for the prevalent *narrative* about the German *Methodenstreit, i.e. the well-known dispute over the method (induction vs deduction) between Gustav Schmoller and Carl Menger*. To date, the German Historical School (represented by Schmoller) is usually associated with reactionary insistence on inductive inference against the 'modern' and 'exact' deductive reasoning (presented by the 'Austrian' Carl Menger).[11] Even Rieter (2002) attributes the hostile attitude on economic modelling to Geman '*Historismus*'.[12] However, such a judgement is exaggerated, misleading, sometimes deliberately pejorative and, therefore, causes serious problems.

For instance, the stereotyped opposition of 'modelling vs non-modelling' ignores the intention of economists such as Spiethoff. He tried to synthesise 'induction' and 'deduction'. Spiethoff did not reject modelling in general, which is verified by his remark about different kinds of theory containing the historical pure theory (see section 3.2). Consequently, a profound review of the German 'Historicism' would result in a more detailed and complex judgement than a simple assertion of the (supposed) German Historical School's hostility to modelling.

[10] In short, 'deduction' means a conclusion based on generally accepted facts or premises, while 'induction' stands for a generalisation based on observations. More details can be found in table 1 of subsection 3.2.

[11] A critical review of this 'narrative' about the *Methodenstreit* is provided by Backhaus and Hansen (2000).

[12] Rieter (2002, p.136) explained that this hostility against modelling results from the fact that the economic analysis within economic styles research would not refer to *homo economicus* or 'strong cases' in terms of Ricardo. This reasoning obviously ignores the possibilities of modelling with actors different from the *homo economicus*.

Although Rieter (2002) stated that the 'hostility against modelling' characterises the aforementioned 'style of thinking', he eventually turns to a revised, more adequate and positive assessment of the (younger) German Historical School of economics and its descendants (*Abkömmlinge*) in terms of '*Neo-Historismus'*. He acknowledges that recent articles oppose the caricature of the German Historical School and its related economists to work without any theory and to solely engage in an empiricism hostile to (economic) theorising. In contrast to the traditional view, these articles rather show that economists of the (younger) German Historical School of economics and '*Neo-Historismus'* did not reject deductive reasoning in general, but that they wanted to substantiate the deduction as a final step of reasoning with a profound, inductive exploration of empirical facts (Rieter, 2002, p. 163).

3. The Theoretical Framework: Spiethoff on Theorising

3.1 Spiethoff's Differentiation of Theories

The basic premise of Spiethoff's economic styles approach is that each theory of economics requires a certain economic style. To demonstrate the meaning and dimension of this premise, it is important to know that Spiethoff distinguishes between pure theory and economic Gestalt theory.[13] Starting with *pure theory* (*reine Theorie*), Spiethoff wrote:

> 'Pure theory emphasizes the isolation of specific phenomena and their relations to other isolated and specific phenomena; other relations which may also exist are disregarded. It is interested in isolated phenomena, not in the innumerable concatenations that in reality link them together. Attention is focused upon specific phenomena and relations, selected with the aid of a given frame of reference and manipulated for research purposes without regard for their location in a 'total' situation. […] Pure theory starts from data which have an axiomatic character, and conclusions are reached by a process of logical deduction: the student draws conclusions about effects by taking given data as causes' (Spiethoff, 1953, p. 445).[14]

Pure theory is what current students of economics are taught and for what Tony Lawson (1997; 2013; 2006) often criticised standard economics (called 'mainstream'). To put it bluntly, pure theory stands for inferences through *deduction*, it is based on isolation and abstraction, it deals with abstract phenomena (i.e. isolated and atomised subjects), and is therefore artificial. However, there is an important difference between a) being completely artificial and ahistorical and b) isolating the phenomena on the basis of 'reality'. This point will be discussed in more detail later in this article.

Spiethoff explained that there is another kind of theory which he called 'economic Gestalt theory' (*anschauliche Theorie*; in the following just called: 'Gestalt theory'):

[13] For this purpose, the aforementioned *Methodenstreit* has to be kept in mind. According to Schachtschnabel (1971, pp. 10−11), the *Methodenstreit* stipulated various attempts to synthesise *induction* and *deduction methods*. Spiethoff's approach is such an attempt where terms like 'pure theory' and 'timeless economy' (Spiethoff, 1932) clearly breathe the spirit of Carl Menger's 'exact method'.
[14] Redlich (1970, p. 642) also referred to an alternative translation, the 'isolating theory'. Spiethoff (1932) synonymously used the term 'abstract theory' and 'pure theory'.

> 'This theory aims at the closest possible approximation to the observable reality. […] [E]conomic Gestalt theory considers the maximum number of relations in which the phenomenon to be investigated actually occurs, provided that those relationships are uniform in character. By a process of induction, economic Gestalt theory arrives at discrete species of phenomena whose characteristics are the data from which it starts. It does not propose to deal purely and simply with relations between rigorously specified phenomena; on the contrary, its purpose is to consider all phenomena that actually and *uniformly* impinge on the one which is the center of attention. Consequently, the selection of phenomena is not determined by looking only at relationships that have been defined in advance, but by the goal of embracing *all uniform* and *essential* relations that occur in a given situation of economic reality. Essential are those phenomena which appear to be causes or conditions of the one under investigation or indicative of those causes and conditions. The ultimate goal of economic Gestalt theory is a replica of reality' (Spiethoff, 1953, pp. 445-446).

For Spiethoff, economic Gestalt theory was firstly characterised by the inference through *induction*. In addition, Gestalt theory is also based on abstraction and isolation (being typically associated with deduction). However, the abstraction here is 'formed in such a way as to leave out only the irregular and the inessential' (Spiethoff, 1953, p. 446). Conflicting phenomena, which would disturb a 'harmonic' ideal of economic life, but are thought to be essential, *must* be incorporated as well (Spiethoff, 1953, p. 458; Spiethoff, 1932, pp. 133-134). Consequently Spiethoff states that '[i]t does *not* matter whether or not the elements, put together, form a *logically consistent* body of knowledge' (Spiethoff, 1953, p. 458).

Therefore, it is almost impossible to formulate economic Gestalt theory in terms of a logical deductive construction (where consequences are clearly defined and determined by – known or assumed – causes and/or circumstances).

Most importantly, economic Gestalt theory 'deals not only with economic phenomena that have a material substratum but, in addition, it also incorporates economic ideas, motives, and goals' (Spiethoff, 1953, p. 447). As a result, and in contrast to pure theory, Gestalt theory was thought to provide a broader applicability (Spiethoff, 1953, p. 447).

Finally, Spiethoff (1953, p. 446) stated that pure theory and economic Gestalt theory are different in terms of *deduction* and *induction* as well as in the related 'spirit' (Spiethoff, 1953, p. 446). For this reason, Spiethoff explicitly rejected the consideration that pure theory and Gestalt theory might 'shade into the other' (Spiethoff, 1953, p. 446). However, while Spiethoff's pure theory and Gestalt theory obviously stipulate the popular polarity between *either* the (pure) deduction *or* the induction method, Spiethoff abandoned this polarity in some other passages of his texts. This will be addressed in the next subsection.

3.2 Deductive Historical Theory, Qualitative Induction and Abduction

Spiethoff's texts about economic styles often suggest that he was caught within the duality of the induction and the deduction method. However, some passages of his writings (which will be referenced in the following) allow for a different conclusion, and deserve to be mentioned in the context of the current debate about 'mainstream' economics and pluralism in economics. Therefore, the following discussion starts with the heterodox economists' criticism of (what they call) the 'economic mainstream' which is then linked to the perspective of Spiethoff's approach.

First of all, according to Tony Lawson (1997; 2006; 2013), the current standard of economics is criticised for its deductivism. Lawson linked this deductivsm to formalism and especially mathematical techniques. This is in line with the popular (or rather stereotypic) criticism of the so-called mainstream economists' insistence on mathematics, often criticised as 'mathematisation' or over-mathematisation.[15] The line of reasoning is that the 'mainstream' is deductive, uses mathematics and mathematical modelling extensively, and therefore is purely artificial – or, in terms of Spiethoff, produces only 'pure figures of thought' (*reine Gedankengebilde*).

This line of reasoning consequently implies that mathematical techniques should be avoided (or rejected) because they are insufficient for analysing the 'real world' (e.g. Lawson, 2006). This raises a fundamental problem for *all* economists – and especially for those 'heterodox' economists – who do use mathematics. To put it bluntly, deduction and mathematics would not be useful for a 'real-world' theory.[16] Consequently, an economist can decide *either* to employ mathematics and, therefore, produce artificial constructs *or* to use another scientific technique for the sake of 'real-world' economics.[17]

While this kind of argument follows is very black and white, Spiethoff broke this duality in his extended theoretical framework through a differentiation of ahistorical and historical theory. Ahistorical theory aims at what 'all species of economic life have in common' (Spiethoff, 1952, p. 135). It incorporates all phenomena of the economies which are unaffected by time and, therefore, show a uniform or an invariant character. Consequently, ahistorical theory 'is by necessity pure theory' (Spiethoff, 1952, p. 135).

In contrast to ahistorical theory, *historical theory* deals with the *time-conditioned* phenomena of economic life.[18] While Gestalt theory is typically a historical theory, Spiethoff emphasises that deductive pure theory can also be useful for historical theory:

> 'Pure theory deals with models resulting from the isolation of phenomena, and it is "historical" when it builds its models by isolating phenomena that have existed only at a particular time or a particular place, phenomena that have significance for a specific economic style' (Spiethoff, 1952, p. 136).

As a result, deductive pure theory can be useful to what critics of the current standard of economics would call 'real-world economics'. Therefore, a further differentiation can be added to the current debate about economics: on the one hand, the 'mainstream' is typically criticised for being an artificially pure theory; on the other hand, there is a more 'real-world' oriented pure theory that could be associated with mathematical modelling in heterodox

[15] I refer to the prominent criticism of mathematical modelling and deduction by Lawson (2003; 2006; 2013) because it is more elaborated than the heterodox criticism of mathematical modelling that normally appears in the aforementioned pluralism debate.

[16] This problem is more or less anticipated in the current debate. However, the discussion seems to make no progress. Instead, it persists in the duality of pro and contra about using mathematics.

[17] To avoid misunderstandings especially with respect to Tony Lawson, he is aware of the use of mathematical modelling in heterodox economics (Lawson, 2013). In his eyes, the main difference between mathematical modelling within mainstream economics and heterodox economics is that mainstream economists insist on mathematical modelling while heterodox economists are open-minded to non-mathematical economics (Lawson, 2013, p. 957). However, although I sympathise with Lawson, who accepts mathematical modelling in heterodox economics and tries to explain the difference between mathematical modelling in heterodox economics and mainstream economics, I think there is an inconsistency in Lawson's original criticism of the deductivism within mainstream economics (associated with mathematical modelling).

[18] Redlich (1970) clarified that Spiethoff used the term *historical* 'in a broader sense than is common in the United States'; the term would rather address 'uniqueness as the characteristic element in historical phenomena' (Redlich, 1970, p. 641).

economics. Spiethoff, however, overcame the duality of induction and deduction far more when he discussed the determination of causes and the development of economic styles:

> 'The original determination of causes depends, methodologically speaking, on deduction; the step-by-step verification of the argument, by means of empirical research, is induction. Actually, theoretical deduction and empirical induction go hand in hand in the task of explanation. A subject of investigation can be approached with a hypothesis; with its help an over-all picture of the possible relations among the phenomena under consideration is constructed. It is also possible that factual research rather than intuition leads to the initial discovery of interrelations and the formulation of imputed causes. Speculative deduction is indispensable; but no logical method exists which by itself would be sufficient to result in an intuitive hypothesis. In later stages of the investigation, empirical research and theoretical considerations mutually influence each other' (Spiethoff, 1953, p. 450).

Obviously, Spiethoff (1953, p. 450) holds that induction and deduction *mingle* in the *real process of research* by intending that economic styles describe 'real' economic life. Already in an early article about economic styles (1932), Spiethoff had claimed that the (empirical) identification of the conditions should, to its own advantage, be accompanied by a theory in which both – theory and empirical identification (*Wesensfeststellung*) – would control and correct each other (Spiethoff, 1932, p. 133). Spiethoff is thus getting very close to what is called 'qualitative induction' and 'abduction' (see table 1).[19]

Table 1 Summary of the Kinds of Inference

Kind of inference	*Description*	*Point of reference*	*Character of inferred results*
Deduction	A single case is subordinated to a known/ (well-) defined set of rules, axioms and assumptions. If the rules are valid, the results are also valid.	Rules/ pure thought (logic)	Tautological & truth-conveying
Induction (quantitative)	A general rule is inferred from observed cases, i.e. some cases are transferred into a rule.	Observed 'reality'	Tautological & probable
Qualitative Induction	Certain observed characteristics of cases (sample) are combined in a way that infers another currently not observed combination of characteristics that – or its possibility of existence – is nonetheless part of the knowledge within the society. Qualitative induction also refers to known rules/ experiences within the society.	Observed 'reality' & known 'rules'	Probable & extending the observed cases
Abduction	Certain observed characteristics of cases (sample) are combined to infer a new combination of characteristics for which no rules or experiences currently exist, so a new rule has to be created.	Observed 'reality' & thought (indirectly: the existent rules/ knowledge)	Creating new rules and knowledge

Source: Author's table based on Reichertz (2013; 2010; 1999).

[19] The concept of abduction goes back to the works of Charles Sanders Peirce (1839-1914). However, I refer to Jo Reichertz (2013; 2010; 1999) who analysed the work of Peirce and found that Peirce's concept changed: what the early Peirce described as 'hypothesis' and called 'abduction' was mistaken with qualitative induction; later, Peirce stated his concept of abduction more precisely in the same terms as Reichertz (2013).

The meaning of *qualitative induction* and *abduction* is shown in Table 1, which summarises the different kinds of inference on the basis of Reichertz (2013; 2010; 1999). While both pure induction and pure deduction are tautological and fail to produce any new ideas, *qualitative induction* would extend the existing knowledge about the observed 'reality' (although in a limited way). *Abduction,* however, would produce new knowledge (Reichertz, 2010). Obviously, qualitative induction and abduction seem to be of use to the endeavour of developing and creating economic styles, as will be shown in the following subsections. Especially in the case where a new style is required, abduction would provide the proper method of inference. From the perspective of the history of economic thought, Spiethoff's economic styles associated with 'abduction' show a wasted historical opportunity to introduce alternative methods such as *grounded theory* (belonging to the standard toolkit of research within humanities/social sciences) to economics.

4. The Economic Styles

4.1 Overview

The economic styles were first defined as patterns or examples of economic life (*wirtschaftliches Zusammenleben*) which – as a synopsis of all styles – would depict the heterogeneity of the entire social and economic life (Spiethoff, 1932, p. 126; Spiethoff, 1952, p. 132). There is a theory possible for each economic style (type), but each theory's validity is limited to its attendant style. Consequently, a comprehensive general theory would contain different partial theories with limited validity. Spiethoff emphasised that the economic styles can aim at different purposes, so he differentiated the following *types*:

> 'Type 1. The model which mirrors a real institutional situation, and is arrived at by economic Gestalt theory.
> Type 2. The model in pure theory arrived at by abstraction from reality.
> Type 3. The model in pure theory which has no counterpart in reality.
> Type 4. The model envisaged by statesmen or utopians to be realized in the future' (Spiethoff, 1953, p. 451).

4.2 Economic Style Type 1 (Based on Gestalt Theory)

The *style model of type 1* aims at the analysis of the real specifics of economic life (Spiethoff, 1953, p. 357; Spiethoff, 1932, pp. 128-134). Spiethoff clarified:

> 'The specific characteristics selected for the determination of a style model of type 1 serve the function of elucidating why that specific pattern of economic life came into existence and persists; they are meant to explain causally the working of concrete patterns of economic life' (Spiethoff, 1953, p. 458).

Obviously, this seems to be the prime motive for the use of the style concept that Spiethoff had in mind and which is evident in his focus on the recreation of the *real* economic life.

Although economic styles are *figures of thoughts* (*Denkgebilde*), they try to recreate the characteristic phenomena of reality (Spiethoff, 1932, p. 128). Especially for theorising, the scientist requires styles that show the 'lived reality in its essential differences' (Spiethoff, 1932, p. 129; author's translation). Therefore, economic styles were thought to be constructed

as an *image* (*Abbild*) of reality (Spiethoff, 1932, p. 131). This indicates that 'the economic style is not a descriptive concept, it is a model' (Spiethoff 1953, p. 451).

Spiethoff already provided an illustrative explanation of this fact in his article from 1932. There, he insisted that this image is no photocopy, which means an exact replica of 'reality', but it is much more a painting (Spiethoff, 1932, p. 133). Just like a painter, it is in the eye of the scientist to determine the important characteristics which are (then) shown by the created economic style. Therefore, the scientist is not devoid of interest, but the creation of economic styles is influenced by the scientist's interpretation about the *entire* economic life. The latter includes the characteristics which are thought to be important. Although the 'important' and 'essential' characteristics are determined by the scientist's decision, the scientist's aim is clear: as mentioned above, he or she should show the differences of economic life by its characteristic arrangements (Spiethoff, 1932, p. 129).

According to Spiethoff's analogy to painting, there is an important difference between a painter and a scientist: while the painter might ignore some characteristics of reality in favour of the harmonic atmosphere of his or her painting, the scientist *must* 'paint' *all* the characteristics including those which disturb a harmonic or consistent *ideal* of reality, but are thought to be essential for real economic life (Spiethoff, 1953, p. 456; Spiethoff, 1932, pp. 133-134). In addition, Spiethoff explained:

> 'It does *not* matter whether or not the elements, put together, form a *logically consistent* body of knowledge. The ideal of consistency plays no role in the selection of what appear to be the characteristics of a style' (Spiethoff, 1953, p. 458).

Therefore, economic styles of type 1 have to contain the essential similarities and uniformities as well as the typically paradoxical phenomena within the economic life (Spiethoff, 1953, p. 451).

As a result, economic styles depend on the scientist's point of view and experience, so the styles of type 1 are determined by interpretation, i.e. the styles' 'objectivity' is limited. However, the creation of economic styles is not completely free in terms of an arbitrary arrangement of assumptions, *if* he or she wants to create an economic style type 1 in order to analyse real economic life. This means that the creation of economic styles is limited through the demand for addressing the entire *reality* of economic life – including the paradoxical phenomena.

4.3 Remaining Economic Styles

The *style type 2* is based on pure theory, is 'obtained by the quite permissible procedure of abstracting from concrete reality' and 'may be designed for heuristic purposes' (Spiethoff, 1953, p. 461). Therefore, these styles represent what Spiethoff (1932) once called *heuristic styles*, which can be useful for the explanation of the Gestalt theory. According to Spiethoff:

> 'From a style model reflecting a historical reality the student derives a more abstract model representing a constellation of data which represents "historical" pure theory, in contrast to style models of type 1, which belong to the realm of "historical" economic Gestalt theory.' (Spiethoff, 1953, p. 461)

The *style type 3* shows what Spiethoff once called the creation of *interesting styles* (Spiethoff, 1932, p. 134) which could represent an arbitrary collection or arrangement of elements of

economic life (Spiethoff, 1953, p. 461). Such styles are constructions – like those known in geometry, i.e. logical constructions free of any relation to reality. They are pure creations of thought (*reine Denkgebilde*). Spiethoff (1953, p. 461) stated that this kind of style is for 'the sake of pure mental experiment'.

Finally, the aim of *style type 4* is 'to elucidate the possibilities and also the dangers of a desired set of economic institutions' (Spiethoff, 1953, p. 461); they represent the created or desired ideals of how economic life should or could be (Spiethoff, 1932, p. 134). Spiethoff obviously alluded to the debate about a *command economy* and a *free market society* (e.g. Spiethoff 1932).

4.4 Categories and Further Implications of the Economic Styles

Table 2 Categories, Subcategories and Features Suggested by Arthur Spiethoff

I	*Economic spirit (Wirtschaftsgeist)*
1	Ethical attitude (*Sittliche Zweckeinstellung*): Kingdom of God, economic success as a symbol of divine predestination, common welfare, the individual's maximum happiness on earth
2	Mental incentives (*Seelische Antriebe*): fear of punishment, religious-ethical motivations (charity, sense of duty etc.), partly ethical motivations (sense of honour, drive for activity, pleasure of work etc.), selfish motivation (own benefits), drive of personality and striving for power, nutrition or purchasing
3	Mental attitude (*Geistige Einstellung*): habit, modernisation
II	*Natural and technological basics (Natürliche und technische Grundlagen)*
4	Population density
5	Natural population change: deadlock, slow, moderate, fast-increasing
6	Production of goods: with division of labour, without division of labour
7	Brain work and handicraft: combined or divided
8	Technical procedure: organic or artificial-mechanical
III	*Social condition (Gesellschaftsvertrag)*
9	Size of economic society
10	Social conjointness (*Gesellschaftliches Verbundensein*): family, force, contract
11	Social division of labour and social composition: e.g. self-employed people and wage
IV	*Economic condition (Wirtschaftsverfassung)*
12	Property rights: free property, governmental property or social property
13	Condition of Production: subsistence economy, managed economy (by social or political institutions) or free market production
14	Condition of Distribution: general money consideration, specially arranged money consideration, specially free money consideration, charity
15	Condition of Labour: cooperative, by force or contractually manorial
V	*State of economic development (Wirtschaftslauf)*
16	State of Economic Development: steady, progressive, between boom and stagnation

Source: Author's table based on Spiethoff (1932, p. 146-147).

The main characteristic of Spiethoff's economic styles approach is the system of categories and sub-categories, and the latter's specification. Each style type (type 1, political style, etc.) should include a set of features like these. Unfortunately, they were only presented in

Spiethoff's article from 1932. There, Spiethoff described five categories with 16 sub-categories including different specifications presented in table 2.[20]

These categories, features or characteristics of economic life (table 2) are not definitive. As Spiethoff (1932, p. 148) emphasised, modification may be necessary. Consequently, the economic styles approach is understood as an *open* and *changeable* tool. According to Spiethoff (1953), this openness can also be understood in terms of method (deduction, induction and abduction): 'Characteristics are found through observation, through deduction, and through the search for possible causes' (Spiethoff, 1953, p. 459).

While Spiethoff focussed here on the many kinds of inference, today this methodological perspective should be extended to different ontological concepts (critical rationalism, constructivism, etc.) and different scientific techniques (mathematics, hermeneutics, qualitative research methods, etc.).

Additionally, economic styles are accompanied by a kind of theorising where theory and empirical identification (*Wesensfeststellung*) would control and correct each other (Spiethoff, 1932, p. 133). This means: theory, reality and 'empirical identification' are mingled in the process of research. Consequently, there is space for interpretation to break the still-dominant duality of *either* deduction *or* induction – including the limitations of the scientist's choice to use a certain ontology and scientific technique.

Furthermore, economic life is understood as holistic or emergent and also determined by an interacting network of elements: 'The reality must be measured as a whole, and every train of thought about the combination and explanation of causes must be embedded in this link of reality' (Spiethoff, 1932, p. 151; author's translation).

In addition, and with respect to the economic style type 1, the historical economies are thought to be singular in time (time-conditioned), i.e. determined by the certain historical and social context. As a result (again), each Gestalt theory is limited to the time and its circumstances which are described by an historical economic style type 1.

However, this basic premise is not limited to only the economic style type 1. As Gioia wrote:

> 'the *Wirtschaftsstil* seems to me to present an undeniable advantage on the epistemological plane since it defines a sort of "mental experiment" aimed at determining, for every explanation, a context of reference which explicitly fixes the selection criteria that stake out the area of validity of the explanation and at the same time establishes the network of semantic terms for its control' (Gioia, 1997, p. 184)

Basically, Spiethoff's approach implies, in more general terms, that any economic style type is a description of the framework that limits the respective economic theory. *Each* economic style type contains (or rather should contain) all of the criteria (features such as ethical attitude or property rights) for proving the related theory's application, validity and power of explanation. This is the general principle of Spiethoff's economic styles approach, unfortunately not explicitly articulated in his articles: each theory requires a certain style and each style is the basis for a certain theory.

In the case of analysing real economic life in terms of style type 1 and Gestalt theory, this *limiting function* is obviously determined by the demand for 'realism'. However, this limiting function also works in favour of creating interesting styles (type 3) or desired styles (type 4). Economic styles would then describe the more or less abstract framework of conditions in which (for type 3 and 4) a normative or 'speculative' theory would work.

[20] Table 2 is based on Spiethoff (1932, pp. 146-147), which was translated by the author. Terms which are difficult to translate and may cause confusion are additionally shown untranslated in brackets.

As a result, this limiting function of styles addresses the validity of its respective theories. This implies different *instances of proof* for a certain style (type) and its related theory. The following table 3 tries to show this context by relating the types of styles and theories mentioned by Spiethoff to the theory's character of time and the respective instances of proof. Note, the social context is added here as a fourth instance of proof which is not explicitly mentioned within Spiethoff's texts, but seems to be a useful feature of heuristic styles in terms of historical pure theory (contrary to ahistorical pure theory) and political styles. 'Social context' simply means that, for instance, even in the case where the results of a theory are in accordance with reality, they have to be checked against the entire social context where these 'real' results can be assessed as acceptable, 'unjust' and the like. Obviously this is an important instance of proof for a Gestalt theory aiming at the entirety of economic life embedded into society, but also for political styles aiming at desired situations in the future.

Table 3 Proofing styles and theories

Style	*Theory*	*Time Character*	*Instances of Proof*			
			1	2	3	4
			Theory's Consistency	'Realistic' Assumptions/ 'Axioms'	'Realistic' results	Social context
Type 1	Gestalt theory	Historical	-	+	+	+
Type 2a: heuristic	Pure theory	Ahistorical	+	+	+	-
Type 2b: heuristic	Pure theory	Historical	+	+	+	+
Type 3: interesting	Pure theory	Ahistorical	+	-	-	-
Type 4: political	Pure theory	Ahistorical	+	~	~	+

Key:
'+' = instance of proof exists for the style and its related theory
'-' = does not exist
'~' open for the incorporation of the respective instance of proof

Source: Author's own table.

In addition, *each* economic style (irrespective of its type) can be associated with a concept of how the described economic 'life' is *entirely* characterised. Therefore, the economic styles approach demands that economists explain their concepts of the entirety of economic life that form the basis of their theorising. As a result, the economic styles approach could finally help to keep economic theorising transparent, comprehensive and well-grounded in terms of theory, normative considerations (or rather ethics) and 'reality'.

5. Why Are Economic Styles Mostly Forgotten Today?

Bertram Schefold (2015[1994]), one of the famous experts in the history of economic thought in Germany, started his book about economic styles by describing his astonishment that most economists in Germany feel unfamiliar with 'economic styles'. His view results from the fact that the very popular and omnipresent basic principle for German economic policy called *Soziale Marktwirtschaft* (Social Market System) was designed in the mode of economic styles

by its 'great originator' Alfred Müller-Armack (Schefold 2015[1994], p. 7).[21] Of course, several articles and books concerning economic styles have been published in recent years such as Schefold (2011), Quaas (2009), Rieter and Zweynert (2009) and Rossi (2006). However, the total number of publications remains very low. In addition, there seems to be hardly any current research programme concerning economic styles, not to mention any current attempts to further develop the approach, or any past or present chair explicitly dedicated to economic styles.[22] Consequently, even if Spiethoff's economic styles concept were thought to be unimportant, readers can agree with Schefold's astonishment that the concept of economic styles, in general, fell into oblivion while the still very popular basic principle of the *Soziale Marktwirtschaft* was originally created as an economic style. Although the fading attention paid to the economic styles approaches is open for detailed analysis, the existing literature on the history of economic thought and the development of economics in Germany provides some explanations of the disappearance of economic styles approach.

Most importantly the Third Reich had a number of effects on the German Historical School and its descendants. Firstly, there were, of course, political effects. When the National Socialist Party came to power in 1933 a lot of scientists emigrated from Germany, while most of the proponents of the German *Neo-Historismus* and related academic movements stayed at home. Of course, there were some economists who stayed in Germany and showed a distant attitude towards the new regime (e.g. Spiethoff) or were against the Nazis (e.g. Edgar Salin and Karl Diehl), but other economists, such as Werner Sombart and Othmar Spann, sympathised with the Nazis (Rieter, 2002, p. 161). Altogether, whether or not individuals remained in Nazi Germany almost certainly became an important factor when social changes were adopted during the spirit of 1968. The movement of 1968 heavily criticised the fact that a lot of Nazis returned to their old position within the Third Reich (especially within German academics). As a result, scientists who made a career within the Third Reich were of no interest to people who wanted social (and academic) change in Germany. In the 1970s, when the spirit of 1968 successfully resulted in the foundation of the so-called 'reform universities', in which a lot of heterodox economists were appointed to chairs (Heise, Sander and Thieme, 2017), the concepts of the German Historical School and its descendants were naturally of no academic interest.

Secondly, after World War II, German economists were thought to be outdated. As a result, the USA as an occupying power of the defeated Germany organised re-education programmes and the exchange of young academics from West Germany to the USA. There, young academics were to learn about modern economics (Hesse, 2010, p.144), i.e. more formalistic, mathematically-oriented etc., economics. In additionally to this 'Americanisation' there was also what Hesse (2012) recently discussed under the term 'Self-Americanisation', i.e., German economists themselves felt outdated in economics and suffered from an inferiority complex, so they started to turn towards this modern economics.

However, this widespread 'mathematisation' of economics in Germany is not only a result of 'Americanisation' and 'Self-Americanisation', but much more a product of the entire spirit of this time. Note, even heterodox economists later enjoyed mathematically demanding

[21] Regardless of all re-interpretations, changes etc. of the term *Soziale Marktwirtschaft* it describes a situation where a) the markets are fenced in a regulatory framework avoiding market effects unwelcome to society, and b) a welfare state is required to reduce social and individual suffering from negative results of market competition (such as unemployment).

[22] Interestingly, the *Handbook of the History of Economic Thought*, edited by Jürgen G. Backhaus and published in 2012, mentioned 'styles' only once in the contribution of Helge Peukert about Werner Sombart (where you can find the only mention of Spiethoff's name within the entire handbook). The same applies to the textbook about the history of economic thought by Söllner, published in the third edition in 2012, where Spiethoff is also mentioned only once, but only with regard to his work on crises or (rather) business cycles; economic styles are not mentioned anywhere.

techniques, for instance, within Neo-Ricardian analyses. Therefore, regardless of the mathematisation's real causes, the (younger) German Historical School and its descendants such as the German *neo-Historismus* suddenly became uninteresting because of their supposedly 'outdated' methodology.

There is another reason pointing in the same direction. As Hesse (2010, pp. 275-277) reported, in the 1950s and 1960s, the academic community of economists in Germany deliberately decided to follow the way of 'modern' economics and left the path of economics-in-terms-of-humanities. Moreover, most German economists showed a disregard for methods or approaches used in other disciplines such as sociology. Therefore, many economists who worked in a more humanity-like way applied for chairs of sociology, etc., or changed faculties/departments (Hesse, 2010, p. 277). For instance, as Hesse (2010, p. 277) reported, Georg Weippert (1899-1965) was a representative of economics in terms of cultural sciences and was at one time at the centre of the discussion about the appointment to a chair of economic theory at the University of Frankfurt (Main); he stayed at the newly-founded chair of sociology and economics at the faculty of philosophy at the University of Erlangen. Another example is Werner Hofmann (1922-1969) who was a Marxian economist and originally worked at the University (Hochschule) Wilhelmshaven. He was then appointed to a chair of economics at the University of Göttingen in 1962 and finally moved to the University of Marburg where he was appointed to a chair of sociology in 1966. As a result, proponents of the economic styles approach diverged from most of their 'normal' colleagues.

In addition, although the economic styles approaches were well discussed in the 1930s and the 1940s, *at that time* a lot of economists showed skepticism (Kaufhold, 1996, pp. 31-32). Some of the skeptics claimed that economic reality was oversimplified by the categorisation of the real economy within the economic styles approach. However, as Kaufhold (1996, p. 33) wrote, this was not necessarily caused by the economic styles concept itself.

Another skeptics' counter-argument to economic styles approaches was the absence of an economic style theory determined by time, culture and the like (Kaufhold, 1996, p. 33; Rieter, 2002, p. 163). This also applies to the current impression of economic styles approaches and especially to the concept of Spiethoff: economic styles approaches are (still) simply thought to be an unfinished work that especially lacks a developed theory (Kaufhold, 1996, p. 31, 34).

Some experts in the history of economic thought suggest here a conceptual problem: as the categories of economic styles are defined more closely to a specific economic reality, the less useful they are for generating a general theory; on the contrary, if a general theory is wanted, it requires more general categories of economic reality – which run the risk of losing the explanatory power for the certain economic reality referred to by an economic style (Kaufhold 1996, p. 34). However, this suggestion about the opposed relation between general theory and concrete economic reality (determined by time/history, culture etc.) unfortunately failed to notice the deliberately relativistic character of the aforementioned 'style of thinking' as well as the possibility of 'abductive' theorising where, as Spiethoff indicated, empirical observation and the development of theories are interactively intertwined.

As a result, the supposed 'weakness' of economic styles approaches is caused by a number of unfortunate misunderstandings, even among experts in the history of economic thought, and the fact that an expected (Gestalt) theory based on economic styles is (still) missing.

Readers could additionally refer to the introductory remark of Schefold and ask why the economic styles approach has been forgotten even though the very popular basic principle of *Soziale Marktwirtschaft* is based on economic styles (of Müller-Armack). There is

no simple answer to this question, which remains open for detailed research in the future. However, there are some indications. For instance, the basic principle *Soziale Marktwirtschaft* was influenced by different intellectual movements such as *humanism in economy and society* (*Wirtschafts- und Sozialhumanismus*) represented by Wilhelm Röpke and Alexander Rüstow, and the *Ordoliberalism* prominently represented by Walter Eucken (Quaas, 2000, p. 28). Therefore, the economic styles approach's origin of the *Soziale Marktwirtschaft* may be simply overlooked. In addition, the term *Soziale Marktwirtschaft* seems to be monopolised by the proponents of the *Ordoliberalism*, so any thought of economic styles may eventually be driven out.[23]

Another reason for the disappearance of economic styles concepts in Germany could be attributed to the fact that the history of economic thought has vanished from the curricula of economic studies.[24] Even where a course in the history of economic thought is offered, it remains optional, mostly limited to just one semester and very often only limited to the 'standard' masterminds of economics such as Aristotle, Adam Smith and David Ricardo.

All things considered, the disappearance of the economic styles approaches can be attributed to a mixture of elements such as, the historical/political circumstances; the post-World War II *Zeitgeist* of economics, which was imprinted with more formal mathematical modelling; the dominance of the modern 'mainstream' economics; and the fact that economic styles approaches require further development and application.

6. The Relevance of Spiethoff's Economic Styles to the Current Discussion about Pluralism in Economics

The study of Spiethoff's economic styles approach is often treated as an area of special interest within the history of economic thought. However, Spiethoff's economic styles approach could also prove to be an attractive topic within discussions about pluralism in economics because it can be associated with an alternative approach and could contribute some insights.

To start with a more general aspect, Spiethoff's approach provides an alternative to the standard economic toolkit in terms of the heterodox demand for a 'real-world orientation' as usually articulated by heterodox economists.[25] As one of many examples, Frederic S. Lee wrote:

> 'The heterodox explanation involves human agency embedded in a cultural context and social processes in historical time affecting resources, consumption patterns, production and reproduction, and the meaning (or ideology) of market, state and non-market/state activities engaged in social provisioning' (Lee, 2012, p. 340).

Obviously, the aforementioned features of Spiethoff's economic styles and especially Spiethoff's economic style type 1 can aim at 'real-world economics' such as implied by the above quotations of Lee (2012).

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[23] Note that *Ordoliberalism* established a fundamentalist belief in the pleasant effects of markets (commonly called 'neoliberalism' in the German-speaking area) which was the mainstream of economics and economic policy in Germany for a long time (Ötsch, Hirte and Pühringer, 2018).

[24] For instance, the German Network for Plural Economics (*Netzwerk Plurale Ökonomik*) published its own study of the Bachelor education at Universities in Germany where they found that only 1.3 percent of the provided courses in economics were dedicated to economic ethics, history of economic thought etc. (*Netzwerk Plurale Ökonomik*, 2016).

[25] Student movements also often demand a 'real-world' economics which can be shown, for instance, by the open letter of the Post-Crash Economic Society at Manchester University (Chick et al., 2013).

A further apparent feature of the economic styles approach is the extensive match with the trinity of pluralism within the open letter of the *International Student Initiative for Pluralism in Economics* (ISIPE, 2014), i.e. theoretical pluralism, methodological pluralism and interdisciplinarity.

Theoretical pluralism is already provided by the different kinds of theories within the economic styles approach (i.e. pure theory, Gestalt theory, historical and ahistorical theory). Methodological pluralism in terms of different kinds of inference (induction and deduction) is included within the different theories and styles (Spiethoff, 1953, p.459). Additionally, Spiethoff (1932, p. 148-149) stated that economists have to be trained in theoretical *and* historical means of research. The latter obviously addressed the work of the German Historical School, which can be associated with, for instance, observation, statistics and work with public records (archive work). Today, these methods would surely be extended to include techniques from other disciplines such as those used within psychology, social and cultural anthropology, sociology, political science and the like. This is reflected by the fact that current experts in the history of economic thought deliberately associate the economic styles approaches with *interdisciplinarity* (e.g. Schefold, 2015[1994], pp. 69-70; Rieter, 2002, p. 163; Kaufhold, 1996, p.35).

In principle, the different kinds of theories and styles within Spiethoff's approach can be extended to different kinds of economic 'schools' and the respective variety of theories. That means that Spiethoff's approach can cover heterodox approaches such as Post-Keynesian economics, evolutionary economics, institutional economics, feminist economics and the like. For instance, a certain feminist style of economics (e.g. a Marxian feminist style) could be developed which, in turn, could form the basis for a related feminist economic theory. Similarly, the integration of 'mainstream' approaches and theories is possible by ascribing them to a certain style with a certain theory. As a result, Spiethoff's economic styles approach could then constitute *a meta-theoretical framework* providing theoretical and (implicitly) methodological pluralism as well as interdisciplinarity.

This meta-theoretical framework would also imply pluralism in terms of ontology and epistemology. The latter represents the different concepts of, for example, how 'the real world' works (including the related assumptions, 'axioms' etc.) and the question of whether they can perceive the 'real world' and, if so, in which way and to what extent. In this context, Spiethoff's approach can cover different bases of theorizing, such as critical rationalism (still very popular within economics), pan-critical rationalism, constructivism or pragmaticism in terms of Peirce, as well as radical feminist ontology (referring to an androcentric ontology) and the social ontology as advocated by Tony Lawson (1997; 2006).[26]

With respect to the methodology results from Spiethoff's aforementioned consideration of the determination of causes and the 'interplay' of theory and empirical identification (*Wesensfeststellung*), his explanations are close to the 'abduction' mode of inference and *grounded theory* as used within other disciplines. Therefore, Spiethoff's 'abductive' thinking allows us a glimpse of the methodological diversity within economics that would be possible today, if economics had not forgotten about Spiethoff's approach. However, 'abduction' is rarely mentioned, even within the current German debate about pluralism in economics. Consequently, Spiethoff's 'abductive' thinking provides the possibility, not only to extend the methodology of economics, but also to break the dichotomy of induction and deduction still dominating the discourse of pluralism in economics.

Against this methodological background, Spiethoff's explanations of theories and styles give good reason to think about categorising heterodox approaches in a more

[26] For the relevance of *critical rationalism* and the tradition of 'Popperism' in economics, see Hands (2001).

differentiated way. Based on his discussion on pure theory and Gestalt theory, *fundamental* differences seem to exist between a) a more formalistic heterodoxy of economics in terms of ahistorical, pure theory and b) heterodox approaches in terms of what heterodox economists and students have normally in mind when they demand an orientation towards the study of the humanities. The latter aims at scientific techniques (especially qualitative research methods, hermeneutics etc.), ontological assumptions, the variety of theories and the proper dealing with this variety which are altogether thought to characterise the work of disciplines within the humanities, and are fundamentally different from the everyday business of mainstream economics.[27] The heterodox approaches most associated with the humanities can be found within, for instance, social economics, feminist economics, the French economics of conventions (*économie des conventions*) as well as the previously discussed German 'neo-Historismus' and economic styles approaches.

In contrast to these approaches, the term 'formalistic heterodoxy' aims at heterodox approaches simply using mathematics and modelling that can be observed within, for instance, Neo-Ricardianism, parts of Post-Keynesianism, parts of evolutionary economics and complexity economics.[28] To avoid misunderstandings, in accordance with Lawson (2013, p. 957), 'formalistic' here does *not* mean that 'formalistic' heterodox economists necessarily show an *insistence* on mathematical modelling, as it is typically expected from mainstream economists.[29] Additionally, heterodox mathematical modelling can clearly differ from mathematical modelling within mainstream economics, e.g. in terms of scientific techniques (matrices vs differential and integral calculus) or the degree of openness and determinism within modelling (agent-based modelling vs the neoclassical model of the labour market). Therefore, the category 'formalistic heterodoxy' rather aims at special problems with mainstream economics arising from the use of mathematical modelling. For instance, 'formalistic' approaches probably run the risk of becoming (methodically) monopolised by mainstream economics.[30] Additionally, people not familiar with economics and heterodox economics may face difficulties spotting the differences of mathematical modelling between heterodox and mainstream economics. This problem can be illustrated by complexity economics which was recently discussed within Heise (2017) who differentiates between heterodox and mainstream approaches of complexity economics. As a result, heterodox economists have a good reason to pay more attention to the 'formalistic' heterodoxy and its special problems with mainstream economics.

There is another reason for the suggested differentiation between 'formalistic' heterodox approaches and heterodox approaches in terms of humanities. The fundamental

[27] Of course, methods similar to that normally used within mainstream economics (mathematics, statistics and the like) could also be found within the disciplines normally associated with the humanities (e.g. sociology, anthropology, linguistics and philosophy). Especially the well-known and criticised 'economic imperialism' or rather economic 'colonisation' demonstrates the methodological and intellectual influence of 'modern' economics on humanities. However, when heterodox economists and students who demand pluralism in economics insist on the idea that economics should be understood as a part of the humanities, they usually then associate humanities with all the methods, thinking etc. that are really different from the mathematical modelling typical for mainstream economics.

[28] An impression of 'Mathematics for pluralist economics' can be found within Keen (2009).

[29] As already mentioned (footnote 17), while an *insistence* on methods of mathematical modelling is clearly a defining feature of mainstream economics (Lawson, 2013, p. 957), the existence of heterodox 'paradigm warriors' casts doubts about the pluralistic attitude of heterodox economists as a defining feature of heterodox economics. In addition, the discrimination of non-mathematical approaches is not limited to the manifest allegation that non-mathematical approaches of heterodox economics are not scientific or not 'proper' economics. The discrimination of non-mathematical approaches can also work in a more tacit way and is finally shown by the attention to these non-mathematical approaches mentioned within the debate about pluralism.

[30] In contrast to this, mainstream economics would face many more fundamental problems by trying to monopolise humanity-like heterodox approaches which are methodically different.

difference between a) the formalistic approaches of economic heterodoxy and b) the other heterodox approaches in terms of humanities is often overlooked within the discussion about pluralism in economics. Typically, the names of Marx and Keynes – as well as specialisations (or rather 'schools') such as Post-Keynesianism, evolutionary economics and complexity economics – are popular enough to be mentioned in the German debate about pluralism in economics. Contrary to this, heterodox approaches in terms of humanities seem to be very under-represented here. However, particularly the latter approaches are really different from the usual expectation about (mainstream) economics and provide examples for a completely different perspective for doing economics. In other words, they can provide an intuitive and catchy illustration of the demanded alternative approaches to mainstream economics.

Furthermore, the story around Spiethoff's economic styles approach gives another reason for the substantiation of the call for pluralism in economics. This is because some approaches, such as Spiethoff's economic styles, simply require the freedom of plurality to get updated and/or modified because they are currently underdeveloped. In other words, the often supposed 'weakness' of such approaches mainly results from a lack of opportunity to become 'competitive' with other approaches of economics. Consequently, for the sake of scientific freedom and fairness, mainstream economists must allow heterodox approaches to become 'strong', 'competitive' and up-to-date.

7. Conclusion

This article tries to give plausible reasons why it would be worthwhile to remember and rethink Spiethoff's economic styles approach. As it was argued, Spiethoff's economic styles approach is especially attractive for the recent discussion about pluralism in economics because it is in itself pluralistic in terms of, among other things,

- the ISIPE's (2014) trinity of pluralism (i.e. pluralism in theory, pluralism in methodology and interdisciplinarity),
- an economic theorising considering the social context (including cultural and moral aspects of life) which is typically demanded by heterodox economists, and, therefore,
- the ability to provide a 'real-world economics' (especially via the style type 1 and Gestalt theory).

Additionally, Spiethoff's economic styles approach illustrates the importance of the history of economic thought in the discussion about pluralism in economics. For instance, the economic styles concept of Spiethoff is just one example of an approach both buried and forgotten today. Therefore, the research into the history of economic thought can again bring up economic approaches interesting for pluralism in economics today. By the way, the economic styles approach also illustrates that the research on the history of economic thought can help to substantiate the current call for pluralism by insights, reasons etc. using (very) old discussions about similar topics.

However, many questions about economic styles still remain. For instance, a detailed analysis of the economic styles approaches' disappearance would be required. Additionally, a comparison of Spiethoff's economic styles with other heterodox approaches such as evolutionary economics or the old institutionalism could specify the heterodox character of the economic styles approach. Perhaps other researchers would like to continue along the lines of Hesse (2010) who reports the academic emigration of economists who think that economics is a part of humanities. This leads to the question about the remaining

academically emigrated economists. Consequently, as these few questions illustrate, the economic styles debate indicates that there are many issues still open for future research.

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The Backward Induction Controversy as a Metaphorical Problem

Ramzi Mabsout, American University of Beirut
rm95@aub.edu.lb

Abstract

The backward induction controversy in game theory flared up and then practically ended within a decade – the 1990s. The protagonists, however, did not converge on an agreement about the source of the controversy. Why was this the case, if opposing sides had access to the same modelling techniques and empirical facts? In this paper I offer an explanation for this controversy and its unsettled end. The answer is not to be found in the modelling claims made by the opposing protagonists, but in the tacit metaphors they operate under. Aristotle defined metaphor as giving a 'thing a name that belongs to something else' (Poetica, 1457b). The meaning of metaphors has not changed much since then – in contrast to models which are comparatively new, and still not well-understood, scientific tools. The controversy of backward induction in game theory provides a test bed for the explanatory power of metaphors. This paper frames the controversy in terms of metaphor choice to provide a common framework for the protagonists. This results in the identification of three different domains – mathematical logic, game theory and the world – each connected to the other via different metaphors. The controversy around backward induction is placed in, and tentatively explained by, this framework.

Keywords: backward induction, common knowledge of rationality, game theory, metaphors, models, rationality

JEL Codes: B4, B16, C72

1. Introduction

> 'How we think about such questions depends on the predilections we bring to an inquiry, on our suppositions about what will count as an answer, on our explanatory preferences' (Keller, 2002).

If metaphors are as important as their advocates argue they are, their function in science is for cognitive purposes – not merely ornamental. Metaphor, as a cognitive concept, should contribute to our knowledge of the world (Boyd, 1979 [1993]) and function 'whenever some phenomenon of cognition is conceptualised or explained through the use of metaphor' (Hoffman, et al., 1990, p. 177). The process by which metaphors work is increasingly well-understood in both natural[1] and social[2] sciences. The advocates of metaphors claim that metaphors can be helpful, across a range of disciplines, to form theory, posit novel

[1] See Emmeche and Hoffmeyer (1991), Hesse (1966), Keller (2002), Pulaczewska (1999), and Jeppson et al. (2012).
[2] See Bicchieri (1989), Boumans (1999), Bronk (2008), Brown (1977), Clarke et al. (2014), Fernandez-Duqe and Jonhson (1999), Hodgson (1995; 2005), King (2012), Klamer and Leonard (1994), Leary (1990), McCloskey (1983; 1994), and Mirowski (1988; 1989; 1994).

hypotheses, construct models, explain phenomena, solve problems, and interpret empirical results.

Metaphors are qualitative and procedural – not exacting logically, 'never complete, precise, or literal mappings'[3], nor even fixed in time and across contexts. Even if not currently acknowledged in economics, metaphors are formal conceptual tools too, in the sense that they can be used in both teaching and research. They operate on various levels. First, economists themselves can be under the spell of a meta-metaphor, also known as a root, constitutive, or theme metaphor, which explains the origins of the other metaphors in use; second, the model itself can be seen as a metaphor; third, metaphors can be found within the model connecting its various inputs to bridge the gap between the world and the model. One implication is that metaphors will not vanish once an idea has been mathematically modelled, 'they are constitutive of scientific discourse' (Bicchieri, 1988a, p. 104).

Metaphors enable mapping between two distinct domains. In science, their primary function is establishing links between scientific language and the world (Kuhn, 1979 [1993, p. 539]). In economics, this mapping connects the (mathematical) model, or syntactic structure, to the economic world (which, it may be noted, economic theory could have helped create performatively). Without the metaphor, the structure consists of un-interpreted syntactical relationships. There is no need to think further than the supply and demand cross to illustrate the working of metaphors. Within this textbook example, the metaphor maps the geometric or algebraic properties (specifically, the intercepts and slopes coefficients) to features of the world, namely, the relationship between quantities and prices of some goods. We could, in metaphoric terms, conceive of the metaphor as 'the engine of the model' since it selects the properties that are mapped from one domain to another, abstracting from all other features kept constant with *ceteris paribus*. Without such a mapping, the model is a set of equations and lines silent about the world. The mapping generated by the metaphor permits empirically meaningful explanations, possibly even predictions (Hesse, 1966).

This paper explores a game theoretic controversy on the meaning and use of backward induction (BI) to compute equilibria. The controversy ended in an apparent unexplained deadlock. The back and forth between the protagonists ends abruptly, so to speak, as neither side gives ground or agrees on what was actually the subject of the dispute. The controversy has also spawned a micro industry that feeds on what axioms justify BI. I shall have more to say on this towards the end of the paper. The controversy is amenable to metaphorical interpretation as the protagonists (Aumann, Binmore, and L. Samuelson, among others) do not agree on the use or meaningfulness of BI. The BI equilibrium entails players who will not cooperate or retaliate, failing to reach mutually beneficial payoffs. BI can be considered the textbook game theoretic solution. A controversy arises since such outcomes are not deemed 'rational' especially in repeated games. Furthermore, empirical evidence (corroborative or not of BI) has had no apparent implications on the controversy.

This controversy can elucidate (or not) the workings of metaphors in economic modelling. Metaphors can offer at least a partial explanation of the controversy and why the protagonists do not converge on agreement. There may be other explanations, of course, but here we shall focus on the plausibility of one, namely, the choice of metaphor.

There are reasons to suppose that, when controversies such as this one arise, the source might be metaphorical. Thus, it was pointed out that, 'when theories run into problems, both the problem and their proposed solutions are consequences of the logic of the metaphors that are at work' (Fernandez-Duque and Johnson, 1999, p. 83) and 'many acrimonious debates in the history of economics would have been clarified tremendously if these [metaphorical] tenets had been kept in view' (Mirowski, 1988, p. 139) or that

[3] Hodgson (1995).

> 'controversies in economic discourse can be clarified, and identification slips avoided, if the user of metaphor specifies its type… controversies arising from misunderstanding can be resolved, saving intellectual energy to the defense of the type of resemblance which a particular metaphor is supposed to reveal' (Khalil, 2000, p. 7).

Although not acknowledged by the participants in the controversy, such views suggest that the BI dispute might have its origins in metaphor choice.

The problem pursued in this paper is whether the BI controversy is one that should involve, and would be solved by, the identification metaphor choice. If metaphor choice captures an essential part of the controversy the discussion can move to justifying the legitimacy of different metaphorical choices. To this aim, section 2 defines metaphors and discusses their relationships with models, narrative, and analogies. Section 3 offers an overview of metaphors and their use in economics. Section 4 introduces the game theoretic controversy on BI. In section 5 the controversy is cast and assessed in terms of metaphor choice. Section 6 concludes.

2. Metaphors, Analogies, Models, or Stories?

I argue in this section that metaphors are primitive but simpler and sturdier than models in their ability to explain controversies such as the game theoretic dispute on the relevance of backward induction (BI). A successful metaphorical explanation involves showing that a scientific disagreement is based on the choice of metaphor. It would provide understanding about why certain authors adopt and understand some models, arguments or findings while others – presented with the same models, arguments or findings – fail to be convinced. What is not explained in this paper is how metaphor choice is, or ought, to be made. This requires further historical epistemological analysis that lies beyond the scope of the paper, but that would nevertheless shed light on why certain individuals have come to adopt some metaphors and others not.

Metaphors have had a continuous consistent definition since (as far I know) Aristotle who argued that metaphor gives a 'thing a name that belongs to something else; the transference being either from genus to species, or from species to genus, or from species to species, or on grounds of analogy' (Poetica, 1457b). Models, however, unlike metaphors, are more recent and still not well-understood. According to Heyck (2015), before 1950, only 7% of articles in the social sciences used the word 'model' or its variants. By the 2000s the range is between 70% and 90%, depending on the social science. But Morgan and Morrison (1999) remark, 'there remains a significant lacuna in the understanding of exactly how models function to give us information about the world' (p. 7) and 'we have very little sense of what a model is in itself and how it is able to function in an autonomous way' (p. 8). Accordingly, instead of labelling the large number of heterogeneous practices that fall under modelling, including metaphorical explanation, as models – it is strategic to avoid this broad categorisation which hides rather than reveals heterogeneity in scientific practices. Why label metaphors 'models', if metaphors have a clear and distinct definition? Modelling imperialism – seeing models everywhere – can, I suggest, add confusion instead of clarity. Metaphors are more primitive, simpler and sturdier explanatory entities.

While metaphors can be described as models, their distinct and simple internal structure would be lost in the large range of heterogeneous practices placed under modelling. In her most recent book on models, Morgan (2012, p. xv) argues that she no longer attempts

to offer a definition of models because of the heterogeneity of objects that count as models. She contends that models are not easy to characterise and there are no easy answers as to what models are or how modelling works. In fact, 'there are lots of different kinds of things that legitimately count as models… and they often look and function very differently' (Morgan, 2012, p. xvi).

If some models have a simple internal structure, the different entities labelled as models are extremely broad. We could still of course label metaphors models. However, this adds an unnecessary conceptual layer: why refer to metaphors as models if we can directly refer to them as metaphors? It should be noted, nothing in my argument militates against the use of models except that no one has done that before (for this specific controversy at least) and I remain open to consider such an alternative.

The received view in economics, nevertheless, is that it is a modelling science – modelling is the principal tool in economics. Mäki (2002, p. 10) remarks that 'to do economics is to do modelling' and defines, following Robert Solow, model building as a 'fact oriented activity that takes as its objective to isolate key causal dependencies in reality' (p. 11). Earlier, Gibbard and Varian (1978) argued too, that economic theorising consists of investigating economic models. They claim that models are used 'whenever there is economic reasoning from exactly specified premises' (p. 666). Economic models, for Gibbard and Varian, have two elements: stories (that carry the interpretation) and an un-interpreted logical mathematical structure (the syntactical part). They argue that economic models pose counter-factual questions (of the following sort: what would happen if such and such was the case?) that are useful in generating explanations. Sugden (2002) too emphasises the explanatory power of models in economics seeing them as describing credible counterfactual worlds useful to warrant inferences from the model-world to the real world.

While Morgan (2002) supports Gibbard and Varian's claim that stories are necessary for economic modelling, she claims their account of how stories integrate deductive models is incomplete. McCloskey's account of the complementarity between metaphors (as models) and stories also fails to adequately describe models because models are not reducible to metaphors. For Morgan and Morrison (2000), models are *autonomous agents* partially independent from, but interacting with, theory and data. In subsequent work, Morgan (2002) identifies two aspects of models – the story/narrative and the structure/metaphor. The theoretical claims are embodied in the structure of the model which determines 'the relationships between the elements of the model' (Morgan, 2002, p. 195), connects the theory to the model and constrains the narrative. Morgan maps metaphors and (mathematical) structures on one side as elements of models arguing they need stories to produce knowledge about the world (p. 183). Stories are needed even when the mathematical elements that constitute the structure are interpreted (p. 189). The structure, specifically, contains mathematical equations that shape the story which, in turn, is not fully determined by structure (p.188). Models connect to the world in two distinct ways, firstly, in building the model, that is in the mathematics themselves involving the realism of assumptions and 'deeper questions about the nature of representation and denotation' (p.192), and secondly, in connecting the mathematics of the model to the world via story. The story here is 'a cognitive tool, a tool by which we explain something or come to understand something about the world' (p.193). Between theory and the world lies the model as an autonomous complex entity. The story becomes necessary when it is necessary to contextualise the model so that 'in telling stories with the model, we use it to explain the specifics of why coffee prices are high in 1976' (p. 194). On the relationship between structure and story Morgan claims that

> 'where to start the tale, which questions are interesting and relevant, and even the order of solving the model is somewhat open – the user has to make sensible choices in order to tell meaningful stories which are plausible and interesting about the world' (p. 195).

The gap in Morgan's account is the function of metaphors which are conflated with the mathematical structure. Instead, one function of metaphor is to bridge the gap between narrative and structure by generating a mapping. Stories connect the mathematical structure to the world using metaphors. I shall, in fact, argue that metaphors operate at two levels: at the highest level they sanction which stories can be told (the 'method of using metaphorical reasoning to construct historical narratives' Mirowski, 1994, p. 14) while they embed the story, at the lower level so to speak, with more specific metaphors. It is at the lower level that narrative integrates the mappings of metaphors to connect the mathematical structure of the model to the world.

Consider the following definitions of models and metaphors: 'the model as a metaphor' and 'the metaphor as a model' (Brown, 1977). Some authors define models in terms of metaphor (McCloskey, 1983; Bronk, 2008; Brown, 1977) while others see the model in terms of an analogy (Hesse, 1966; Klamer and Leonard, 1994), and yet for others, models are carriers of metaphors (Bicchieri, 1988a; Bouman, 1999; Morgan, 2002). As noted earlier, variegated opinions on what models are is to be expected, but that does not mean there is no agreement how metaphors work. The other side of the duality, 'metaphor is a model', requires explanation as well. This side appears redundant as noted earlier: an account of how metaphors work can be given without reference to models. As Maasen et al. (1995, p. 1) explain, in fact, interest lies not in duplicating and expounding 'fine grained terminological distinctions between metaphors, images, analogies, models, rhetoric, and systems of thought' but in how metaphors permit 'the transfer… of pieces of meaning from one delineable discourse to another'.

Another clarification needs to be made, namely, on the relationship between analogy and metaphor. Hesse's (1966) view is that while analogy resembles a relation of mathematical proportionality (the word for proportion in Greek is *analogia*), it is not constrained by it (pp. 66-7). Hesse also contends that Aristotle 'speaks of metaphor as being based on analogy' (p. 133) and, in her conception of theories as metaphors, she favours explanation as metaphoric re-description of the explanandum (the phenomena that needs explanation) against the orthodox deductive model of explanation. For McCloskey (1983), metaphors belong to the larger class of analogies whereas Mirowski (1988) uses both terms together and interchangeably. According to Hoffman et al. (1990, p. 213), metaphors arise before analogy which are '*post hoc* relative to the root metaphor'. Klamer and Leonard (1994, p. 34) are faithful to Aristotle's definition of analogy 'as a specie of metaphor' yet distinct. In a metaphor, Klamer and Leonard argue, there are attributes in common between the principal subject (mind, time, market) and the subsidiary domain (machine, money, game) whereas an analogy 'draws explicit parallel between subject and subsidiary domain' (p. 34).[4] An analogy is less than a full-blown metaphor since 'it does not capture all the associated commonplaces suggested by the metaphor' (p. 35) and requires no imaginative leap since it is based on the (Aristotelian) principle of proportionality. Klamer and Leonard agree with Hoffman et al. that analogy is an elaborated metaphor that focuses on certain relationships suggested in the metaphor. The metaphor is a heuristic which leads to the generation of analogies that form testable models. In their specification of metaphors, they note that

[4] As we shall see, contrary to what Leonard and Klamer claim, some metaphors are said to be heterologous in that the mapping is based on functions rather common properties in both domains.

> 'the mere coinage of a metaphor such as "human capital" does not make science. Science proceeds by taking a fertile metaphor and relentlessly articulating the nature of its subsidiary domains, probing the properties of that terrain, and testing the connections between that domain and the principal domain' (p.35).

An interesting implication is that, for Klamer and Leonard, models are not metaphors but an 'explicitly, most often formally articulated analogy' (p.35). Cohen (1994, p. 57), similarly, uses the Aristotelean definition of metaphors, defining analogies as denoting 'a similarity that centers on an equivalence or likeness of functions or relations or properties'. Lagueux (1999, p. 15) contends that analogies are methodologically more acceptable since metaphors violate 'what logic requires' whereas analogy is an explicit comparison in which the 'distinctiveness of the respective domains is explicitly preserved… [it is] perfectly suited to scientific analysis.' Given these variegated views, we can synthetise the following points: (i) metaphor and analogy are related and cannot be disentangled; (ii) metaphor uses analogy but is not reducible to it; (iii) analogy both contributes to the metaphorical mapping and can be the outcome (in the form of a testable model) of a further elaboration of the original metaphorical mapping.

3. Economic Metaphors: an Overview

Faced with a controversy in game theoretic modelling, how should an economist or a methodologist evaluate it? I will argue in this paper that some controversies could be explained in terms of the strategic choice of metaphors. The definition of metaphor as connecting, via a mapping, two distinct domains is all the tool kit needed to generate the explanation.

Economics contains a number of metaphorical expressions, including 'equilibrium', 'elasticity', 'human capital', 'accelerator', 'GNP is up', 'prices are inflated', 'liquid assets', 'price mechanism', and 'policy instrument' (as listed in Klamer and Leonard, 1994 and McCloskey, 1983). They are considered thought-changing, breaking the habit of thought as it were, by employing a deviation from the literal meaning to the figurative meaning: 'time is money', 'time flies', or 'mind as machine' (time is not money, time has no wings to fly, and the mind arises from an organic, not mechanic, organ). Metaphors, when not dead, are fluid and open to interpretation, being highly sensitive to the context in which they are used. The distinction between the literal and figurative meaning is also not necessarily dichotomous but continuous: expressions which begin as a metaphor harden, freeze, or die of overuse (Bicchieri, 1988a; Klamer and Leonard, 1994, p. 27). Their death in science, however, unlike poetry, signals a successful metaphor which has spread (Boyd, 1979 [1993]).

The metaphorical expressions listed above fill a gap in the economics lexicon and, while useful, they do not to elucidate how metaphors work. Such metaphorical expressions, it is said, are sanctioned by a higher-level metaphorical mapping between two domains (Lakoff, 1993, p. 209). I have found six different labels of these two domains.[5] In what follows, two accounts that reflect different perspectives on how metaphors work are discussed. The first view posits that metaphorical mappings generate a conceptual mapping of entities, properties, relations, and structure from a *source* to a *target domain* (Fernandez-Duque and

[5] Klamer and Leonard (1994) mention a few (i) subject / predicate, (ii) tenor / vehicle, (iii) target /import, and (iv) principal/subsidiary. Fernandez-Duque and Johnson use (v) source / target domain while Hesse (1966) and Bicchieri (1988a) (vi) secondary / primary subjects.

Johnson, 1999; Lakoff and Johnson, 1980; Lakoff, 1993). This mapping is illustrated with the 'the mind as machine' metaphor.

The 'Mind as Machine' Metaphor[6]

Source domain / subsidiary or secondary subject		*Target domain / principal or primary subject*
Machine	→	Mind
Functions within machine	→	Mental capacities
Products of the machine	→	Ideas
Automated machine functioning	→	Thinking
Normal machine functioning	→	Normal thought
Breakdown of machine	→	Inability to think

Each arrow above takes some entity or structure in the source domain and constructs a counterpart in the target domain. The mappings provide a 'fixed pattern of ontological correspondences across domains' that can be activated, not algorithms that mechanically take the source domain inputs and produce target domain outputs' (Lakoff, 1993, pp. 210-14).[7] These mappings also submit to the *invariance principle* which states that 'metaphorical mappings preserve the cognitive typology of the source domain, in ways consistent with the inherent structure of the target domain' (*ibid*.,). The invariance principle can be understood as a constraint on the correspondences that constitute the mapping where cognitive typology is the image-schematic structure of the domains. Similarly, the image-schematic structure of the target domain can in turn limit the possibilities of mappings from the source domain.

A more dynamic account of how metaphors function is given by Black (1962), and earlier Richards (1936), who contend that metaphorical meaning arises from the interaction between a *principal* and a *subsidiary subject* or, equivalently, between a *primary* and *secondary subject* (Black, 1979 [1993]).[8] Hesse's account of metaphors is that they work

> 'by transferring the associated ideas and implications of the secondary to the primary system. These select, emphasize, or suppress features of the primary; new slants on the primary are illuminated; the primary is seen through the frame of the secondary… it follows that the associated ideas of the primary are changed to some extent by the use of the metaphor… [T]he same applies to the secondary system, for its associations come to be affected by assimilation to the primary; the two systems are seen as more like each other; they seem to interact and adapt to one another…. Men are seen to be more like wolves after the wolf metaphor is used, and wolves seem to be more human' (Hesse, 1966, p. 163).

[6] From Fernandez-Duque and Johnson (1999, p. 85).
[7] More specifically, Lakoff (1993, p. 249) explains that the 'contemporary theory of metaphor is at odds with certain traditions in symbolic artificial intelligence and information processing psychology. Those fields assume that thought is a matter of algorithmic symbol manipulation, of the sort done by computer programs. This defining assumption is inconsistent with the contemporary theory of metaphor.'
[8] Aristotle identified four kinds of metaphors, and though he excluded them from logic (Klamer and Leonard, 1994, p. 24), his definition appears to be of the interactionist sort since it involves giving a 'thing a name that belongs to something else; the transference being either from genus to species, or from species to genus, or from species to species, or on grounds of analogy' (Poetica, 1457b)

Thus, while the (unidirectional) mapping gives the principal subject (man, time or mind) a name that belongs to the subsidiary subject (wolf, money or machine), the bi-directional interactionist account entails that the metaphor can modify the prevalent interpretation of both the principals and the subsidiaries as novel meanings, not reducible to or substitutable by a literal expression, arise. The interactionist view of metaphors allows us to clearly distinguish between analogy and metaphor, captures the *resonance* or *expansion* of a metaphor more adequately than the unidirectional view, and integrates performativity, for example, in the possibility that the market (as the principal subject that borrowed properties and relations from the subsidiary subject, the game) may in turn also modify our understanding of the game.

Distinctions between various kinds of metaphors have been made. Khalil (2000) identifies four kinds (the nominal, heterologous, homologous and unificational) while Klamer and Leonard (1994) identify three (the pedagogical, the heuristic and the constitutive).[9] Pedagogical metaphors are the simplest as they illuminate and clarify a complicated concept by providing mental images to help an audience. They are closest to Khalil's nominal metaphors which only use a superficial similarity between the principal and the subsidiary. They usually help answer the question 'what is the intuition?' and 'what is the story?' behind a mathematical model. It is pedagogical metaphors that scientists and economists usually have in mind when thinking of metaphors.

Nominal and pedagogical metaphors, however, are not the most influential since they may be omitted without affecting an argument. Heuristic metaphors are part and parcel of theories.[10] They cannot be paraphrased or substituted with a literal expression. Klamer and Leonard argue they are necessary to catalyse our thinking – they are thought-propelling. Accordingly, an example of a successful heuristic metaphor is that of 'human capital'. The 'human capital' metaphor signals the beginning of an inquiry and will, given the resonance it creates over time, generate additional developments including testable analogies.[11]

Constitutive metaphors frame our thinking, determine what makes sense, and work at the fundamental level of Kuhn's paradigm. They are spectacles necessary for the interpretation of our world and include 'those sets of assumptions, usually implicit, about what sort of things make up the world, how they act, how they hang together and, usually by implication, how they may be known… [they] constitute the ultimate presuppositions or frames of reference for discourse on the word or on any domain' (Brown, 1977, p. 125). Consider the following metaphors: 'we've hit a dead-end street', 'we can't turn back now', 'we may have to go our separate ways', 'look how far we've come', 'it's been a long, bumpy journey'. They all refer back to the same constitutive metaphor, namely, 'love as a journey' (Lakoff, 1993). The 'human capital' metaphor, similarly, is congruous with the constitutive metaphors of neoclassical economics (such as physical capital accumulation) and succeeded as a heuristic metaphor. Another example is the mechanistic world metaphor which generated concepts

[9] Maasen et al. (1995) identify three kinds of metaphors as well, the illustrative, heuristic and the constitutive. The definitions of the first two are the same as Klamer and Leonard but their definition of constitutive differs, however, since they see their function is to replace previous meaning by new ones.

[10] Boyd (1993, p. 486) refers to them as constitutive metaphors, but since this term is already employed by Klamer and Leonard to mean something else, it will be avoided.

[11] Human capital appeared roughly around the same time in Mincer (1958), Schultz (1961) and Becker (1964). Capital in economics classically refers to physical capital such as machinery and plant. Human capital, likewise, referred to education and skills as investments that generate returns for the owner. Human capital is also an input in the production process. The 'human capital' metaphor renders human capital interpretable as one of the inputs (alongside physical capital and technology) in a standard production function. Although an expenditure by individuals, human capital is distinct from the consumption of other goods since it provides a return in the future, like any other investment. In Schultz's case, a parallel between physical/capital investments and human skills investment is made explicit. In this way Schultz could use the existing capital terminology to explain large increases in national output. It is here, Klamer and Leonard argue, that the connection between thought in science and metaphor is strongest.

such as price mechanism, equilibrium and elasticities, among others (Brown, 1977; Hodgson, 1995). Hard to specify concretely, constitutive metaphors tend to operate below conscious awareness and 'can be exposed only by digging into or interpreting the relevant texts, both spoken and written' (Klamer and Leonard, 1994, p. 41). Constitutive metaphors answer the question of where our heuristic metaphors come from.

While the nominal distinction made by Khalil (2000) overlaps with Klamer and Leonard's (1994) pedagogical metaphor, the heuristic and constitutive metaphors, to the extent that they work for uncovering structures, processes and powers, may overlap as well with Khalil's heterologous, homologous and unificational metaphors. Khalil, however, is pessimistic about this overlap since he sees Klamer and Leonard, not as realists, but as sophists – more interested in persuasion than in uncovering real phenomenon. Heterologous metaphors exist when there is resemblance of analytical function, but the context or origins are not the same (for example, the wings of a bird and the wings of a bat, although both perform flying, emanate from a different context). As an example of a heterologous metaphor in economics, Khalil considers those on spontaneous order arising from climatic and ecological systems (subsidiary) and socio-political order (principal). Homologous metaphors exist when there is no common analytical function, but a similar scheme, context or common origin (thus, though the forelimbs of mice and bats have different functions they have the same origins and are homologous). Examples of homologous metaphors in economics include the evolution and entrenchment of habits and biological evolution; the division of labour within the firm and the differentiation of functions within organisms; and the autocrat of a chimpanzee troop with the modem state. Khalil identifies unificational metaphors as the strongest kind, in that the same law must be operating in the principal and subsidiary subject. Thus, the law of gravity is unificational because it is used to explain various physical and astronomical phenomena; the similarity of blood circulation in humans and chimpanzees is also considered a unificational metaphor. As for economics, Khalil claims that optimisation unifies disparate phenomena by drawing on the similarities between household and firm maximisation (utility and profits respectively).[12] Khalil, finally, explains that the use of any metaphor is appropriate if and only if it is classified in its appropriate category. He provides three levels of identification slips: from single degree (heterologous metaphor is used when the similarity is only nominal) to triple degree (unificational metaphor is used when the similarity is nominal) and offers, for each level of identification slip, an example from economics.

As this section has shown, metaphors can be more specifically defined than models. All types of metaphors generate a mapping from one domain to another. Equipped with this view of metaphors, the next section focuses on the backward induction controversy. In section 5 use will be made of the understanding gained in sections 3 and 4 to explain it.

4. The Backward Induction Controversy

'The economists do not know why they disagree' (McCloskey, 1990).

[12] Cohen (1994) adopts an overlapping but different classification from Khalil's, namely, analogy, homology, identity and metaphor. While Khalil considers all four kinds of relationships metaphorical, for Cohen, identity is similar to Khalil's unification metaphor while homology expresses similarity in form (not function) and is thus similar to Khalil's homologous metaphors. Aristotle has, in this regard, identified two types of analogies, analogies 'when there are properties in common' and (Platonic) analogies 'when there is similarity in the relation of the parts' (Hesse, 1966, p. 134; see also p.142). In his mature thought on analogy, Hesse claims that Aristotle combines both senses (p. 138).

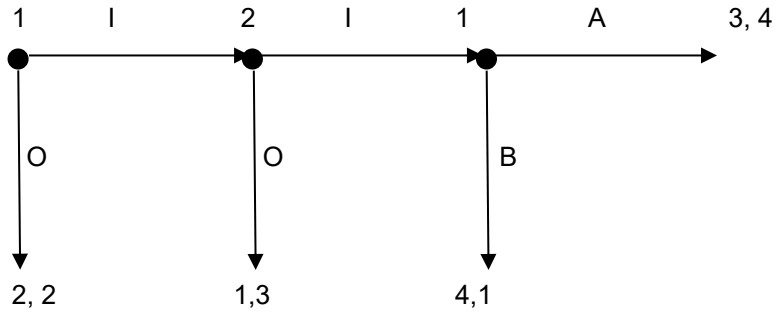
> '[O]ut-of-equilibrium play occurs with zero probability if the players are rational' (Grüne-Yanoff and Lehtinen 2012)

Backward induction (or BI) is a method to compute equilibria in finite, usually perfect information, extensive form games. It involves the analysis of games from back to front, proceeding by the elimination of dominated strategies.[13] However, the usefulness and epistemological function of BI led to a controversy to be elaborated upon in this section. The question is, in what terms can this controversy be explained?

BI led to the Nash refinement literature and to so-called subgame perfect equilibria (Selten, 1975). The term refinement is used since such equilibria involve additional criteria which take the form of eliminating non-credible threats. Subgames require an initial node and are self-contained games within a larger game (including the game itself). A node ***x*** initiates a subgame if neither ***x*** nor any of its successors are in an information set that contains nodes that are not successors of ***x***; the subgame is the tree structure defined by such a node ***x*** and its successors (Watson, 2013). A subgame perfect Nash equilibrium exists when there is a Nash equilibrium in every subgame of the larger game, and when a subgame is reached the players will play according to this equilibrium strategy. It follows that not all Nash equilibria are subgame perfect, but a subgame perfect equilibrium has to be a Nash equilibrium.[14] This classical account of playing games (to be further described below) is, via BI, considered 'the only possible pattern of play by rational players' (Bicchieri, 1988b, p. 383).

BI, combined with rationality and common knowledge of rationality (CKR), entails that in games such as the centipede, players choose at every decision node 'down' in Figure 1 until the first node is reached.

Figure 1 Rosenthal's (1981) extensive form centipede type game (player 1, player 2)



To understand why, note that in Figure 1 player 1 will opt for down (B) in the last node of the game because the payoff is highest (4>3). Knowing this player 2 plays down as well since this maximises payoff (3>1). Back to the first node of the game, player 1 knowing player 2 will play down if she had the chance will play O (2>1) ending the game before it starts. In repeated games, players play various rounds (of the same game). BI again implies that non-dominated

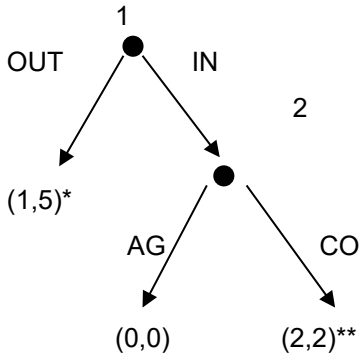
[13] The first explicit reference to BI is due to Kuhn (1953). Luce and Raiffa (1957, p. 68), though not the first, state that 'at a terminal choice point – we are assuming that all games have a stopping rule, and this enables us to work backward – the player whose move it is will naturally adopt the choice which suits him best. Thus, since the last choice is determinate, we may as well delete it and place the appropriate payoff directly at the terminal move position, if this is done for each terminal move, the penultimate moves now play role terminal moves, and so the process may be carried backward to the starting point.'

[14] This is because all non-credible threats Nash equilibria have been eliminated from the smaller set of subgame perfect Nash equilibria. Simulations have also shown that it is possible to derive Nash equilibria that are not subgame perfect (Binmore and Samuelson, 1996; Gale, Binmore and Samuelson, 1995).

choices are made: starting from the last game each player opts for the choice that maximises their own pay-off irrespective of what the other plays until the first game is reached. In the finitely repeated prisoner's dilemma, this means that players will defect on each and every round of the game. The repeated prisoner dilemma, although not a perfect information game, submits to a similar reasoning and both players are expected to defect. A third game where BI is applied is the chain-store game in Figure 2 (Selten, 1978). Here, an incumbent monopolist (player 2) holds a monopoly in *m* towns. The monopolist does not declare a price war when one of the *m* competitors, one in each town, decides to compete. In the chain store game, the monopolist's decision to play AG (for aggressive or price war) is seen as a non-credible threat since it is not in its interest to retaliate and it will play CO (for cooperate) (Gibbons, 1997). It should be noted here that this game has *two* Nash equilibria {IN, CO} and {OUT, AG} but only not declaring a price war {IN, CO} is subgame perfect.

Figure 2 Selten's (1978) Chain store game in extensive and normal form

1 \ 2	CO	AG
IN	2,2**	0,0
OUT	1,5	1,5*



*Nash Equilibrium
**Sub-Game Perfect Nash Equilibrium

Selten (1975, p. 35) early on identifies a difficulty with BI, noting that 'there cannot be mistakes if the players are absolutely rational'. Selten (1978) later observes that there is a paradox because, while it is more advantageous for the monopolist to cooperate in the short term (the BI and game theoretic decision), it is better to play aggressively in the long term (the deterrence, more convincing decision). Accordingly, the repeated chain store game is paradoxical because only BI is theoretically correct yet playing aggressively – and starting a price war – is much more convincing. The paradox is elegantly described by Hargreaves Heap and Varoufakis (1995) who argue that the Nash equilibrium (1,5) in figure 2 is eliminated by backward induction since it is not a credible threat. However, the subgame perfect Nash equilibrium (2,2) is singled out with the now out-of-equilibrium strategy (1,5). This creates a puzzle since while CKR is assumed, the rational strategy is identified by considering

> 'what would happen if what turns out to be an irrational move were to be made at some point… [an] equilibrium behaviour needs to be built on an analysis of out-of-equilibrium behaviour… we have to introduce the possibility of some lapse of rationality to explain what rationality demands' (pp. 87-88).

This analysis brings about two (difficult) questions for Hargreaves Heap and Varoufakis, namely, are such lapses from rationality consistent with CKR and how can one assume rational play in out-of-equilibrium play?

Selten, in this regard, argues that a satisfactory interpretation of equilibrium in extensive games seems to require that the possibility of mistakes is not completely excluded. Selten introduces irrational play assuming players are subject to rationality imperfections so that, at every information set *u,* there is a small probability ε_u for the breakdown of rationality. He sets the stage for the controversy, noting that there cannot be any unreached information set and that this is consistent with the definition of strategy profiles which inform what the player will do at every information set of the game, specifying behaviours even over unreached subgames.

Experimental results on the use of BI are ongoing but far from conclusive. In over 1000 experiments conducted since the late 1950s, cooperative choices were made about 30% of the time in the repeated prisoner dilemma (even as it emerged that experience raises the chances of defection or playing the BI equilibrium; see Andreoni and Miller, 1993; Colman, 1998, p. 356). Binmore, Shaked and Sutton (1985) find that in a bargaining game, individuals learn to play BI in round two when the roles are reversed. Balkenborg (1998) runs experiments over what he calls the basic (or stage) game and notes that 80% of the results support the outcome predicted by BI (with 13 sessions and 12 subjects, each playing the game 50 times, randomly varying with anonymous opponents). Johnson et al. (2002) test the extent to which deviation from the BI path is explained by 'limited cognition' or 'equilibrium social preferences'. They find that both contribute to explaining deviations from BI and suggest that individuals are not equipped to use BI without prior training. After parcelling the ultimatum game[15] into rationality, subgame consistency and truncation consistency, Binmore et al. (2002) find evidence against the use of BI. They thus back the long-held result of Güth et al. (1982). There are also experimental results against the use of BI in the ultimatum game (Henrich et al., 2005; Guala, 2008; Roth et al., 1991), the *p*-beauty contest (Camerer, 2003a), and the centipede game (McKelvey and Palfrey, 1992). Various explanations have been offered for these deviations from BI. Smith (2003), for instance, explains the deviations in the ultimatum, dictator and other trust games – in terms of reciprocity, instead of a preference for fairness in the utility function. More specifically, he posits a neurocogntive explanation based on the capacity of players to mind-read the other player's moves (McCabe et al., 2000).

A common critique against BI is that it is paradoxical (Basu, 1990; Bicchieri, 1989; Luce and Raiffa, 1957; Pettit and Sugden, 1989). The culprits are the conjunction of CKR and rationality. The argument is that if players are rational they may have to consider cooperation. Yet, and herein lies the paradox, BI renders cooperation non-utility maximising as it assumes (in the centipede game, for example) player 2 plays down if the second node, which is not meant to be reached, is reached. If node 2 is not meant to be reached according to the theory, how can it predict that player 2 plays down? Pettit and Sugden's (1989) argument against the BI equilibrium is innocuously simple and follows this train of thought, namely, that common belief in rationality breaks down – and the BI equilibrium does not prevail – when one of the players acts cooperatively in the repeated prisoner's dilemma. The paradox was demonstrated more rigorously in Reny's (1993) proof that rationality (as utility maximisation) and CKR are inconsistent in two-person perfect-information finite games. If either CKR or rationality is dropped, Reny further argues, BI is no longer the only type of rational play. Reny's own interpretation of his proof questions the plausibility of subgame perfection.

[15] In the ultimatum game, for example, player 1 makes an offer equal to a portion of a total sum to be shared with a receiver who then decides to accept or reject the offer. If the offer is rejected – as often happens when the offer is much under 50% of the total – no player gets any payoff. If the offer is accepted both players get the agreed sums. The subgame perfect equilibrium is for the receiver to accept any positive offer that is made since something is better than nothing. Rejection rates of positive non-trivial offers, however, are quite common across many cultures.

Until now the BI equilibrium has been criticised on at least two connected grounds: it is unreasonable for players not to cooperate when this will benefit all with enough rounds to play the game; and players must take hypothetical decisions in nodes they will never reach (CKR and rationality are inconsistent). The controversy, however, really begins after Aumann's (1995) formal mathematical proof that CKR and rationality in a perfect information game suffice to justify the BI equilibrium. More specifically, Aumann argues that when CKR and rationality prevail, no vertices off the BI path are reached (p. 18). Aumann sustains that the proof supports the intuition that common knowledge implies BI while relying on the usual meaning of concepts such as knowledge and rationality. Irony has its ways, since it was mostly as a response to Reny et al. that Aumann (1995) offered his proof. At this stage perplexity is allowed since we are left with a proof against a proof (Reny vs Aumann) on the one hand, or common sense and intuitions against the proof on the other (Aumann vs Pettit, Sugden, Bicchieri, Basu, Selten et al.). Even after Aumann (1995) published his proof, both supportive (Rabinowicz, 1998) and contesting counter-proofs (Binmore, 1997; Ben-Porath, 1997) were published.[16]

Binmore and Samuelson (1996) were quick to counter Aumann (1995), initially disputing the use of mathematical logic to establish the proof. Binmore (1997, p. 24) subsequently added that the proof reduces to 'inventing fancy formalisms… only to confuse matters'. Aumann's proof, it is claimed, brushes aside the question of how the players acquired rational and unambiguously-defined beliefs in the first place. For Binmore and Samuelson, equilibria should not be established via the static definition-axiom-theorem-proof format (that closes the mind) but via algorithms of players' reasoning and 'constructive' simulations of the equilibrating process.

According to Binmore and Samuelson, the problems of classic game theoretic rationality are compounded by CKR. Unless counterfactuals – such as rational players acting irrationally – are accounted for in the strategy profiles of players, rationality makes no sense. According to the critique, the BI path can only be justified with counterfactuals (of choices that could have been made but were not).[17] The possibility of hypothetical decisions of the sort 'what should player 2 do if player 1 does not play down but cross instead?' is now seen as requiring special attention. How would a player explain that the other player is not playing the BI path? What kind of mistake or irrationality brought us to node 50 out of 100 instead of ending the game at the first node? These questions were raised by Binmore and Samuelson (1996) and Binmore (1996; 1997) as a critique of Aumann (1995). While the critique acknowledges that rational play entails the first movers to play down (eg., Bicchieri, 1988b; Binmore, 1987, p. 196; 1997), the critique also states that rationality is not adequately modelled if players do not account for what they would have done if the other player does not follow the BI path.

Aumann's proof was meant to answer these critiques, which turned only more virulent, insisting his conception of rationality is mistaken as long as it does not specify what the players would play if they deviate from the path of BI. Aumann's (1996a, b) response is that his proof (i) does not necessarily imply that rational players will not deviate from the BI

[16] Without CKR, Rabinowicz defends BI for a class of BI-terminating games where rationality is a choice of moves, not strategies. Here BI-terminating games are games, such as the centipede games, where down ends the game, excluding the finitely repeated prisoner dilemma. Binmore uses a finite version of the centipede game to show that, even with CKR, the equilibrium of the game is a Nash mixed strategy equilibrium, not the BI equilibrium. Ben-Porath assumes CKR only at the first node, exploiting the distinction between certainty which allows surprises (playing cross with probability 0) and knowledge (which does not allow surprises), as well as the possibility of changing beliefs, Ben-Porath claims that the BI equilibrium is no longer the only justifiable equilibrium.

[17] Note that such arguments require observable behaviours by players or else there would not be a paradox, and indeed there is no such paradox in simultaneous-move games (Reny, 1993).

path; (ii) he insists that rational players may deviate at any point including the first move; and (iii) that the inductive choice used in the proof could be irrational. He acknowledges that his conception of rationality and strategy does account for player *i*'s knowledge of what the other player would do had *i* played across instead of down (in the centipede game). Aumann suggests his critics are confusing the assumption of rationality with CKR and shows that his theorem still holds if he adopts Binmore's stronger definition of rationality. It is relevant to add here that Aumann's interpretation has recently received the support of empirical game theorists (Gintis, 2009). In summary, Aumann (1996a, b) argues that as long as rationality is common knowledge, a player deciding across is absurd logically, since down is the only possible outcome at the first information set of the game.

By the looks of it, the debate has stalled, with both sides entrenched and unable to dig deeper for answers. The core of the controversy is what (if anything) can justify BI. For skeptics, when rationality and CKR do not contradict each other, something else beyond rationality and CKR, is needed, including maybe stories from outside the game. As tension emerges between game theoretic reasoning, on the one hand, and intuition or common sense, on the other hand, is it surprising that in the most formal modelling branch of economics – game theory – so much weight is given to intuition? If metaphors are as important as their proponents argue they are, this should not come as a surprise. The presence of intuitions and common sense may still be explained as an inchoate metaphorical choice that needs to be formally acknowledged. The literature on metaphors can step in to formally account for these choices.

5. The Strategic Use of Metaphor

How can metaphors be used to provide an explanation for the controversy on BI? Recall that Aumann assumes rationality and CKR sufficient to justify the use of BI in computing equilibria. This sufficiency relies on a mathematical proof that shows that irrational choices could not have been made (Aumann, 1996a, b). Binmore and Samuelson (1996), on the other hand, disagree and posit that his mathematical proof is irrelevant: something else is needed to justify BI. The nature of the controversy between Binmore and Samuelson, on the one side, and Aumann, on the other, remains puzzling and in need of explanation. Indeed, why is there a controversy if (i) the mathematical proof of BI with CKR is rigorous?[18] And (ii) if empirical tests unfavourable to BI could be – but are not – brandished to undermine its validity? Binmore and Samuelson (1996) argue that the controversy is not within mathematics: they (albeit not others) agree that the mathematical proof that CKR leads to the BI equilibrium is rigorous, yet they suggest that the proof has little value. The critics of Aumann are convinced that something is not right, and that the BI equilibrium cannot be justified in the way Aumann does (in fact his way is perceived as so fundamentally wrong by Binmore that it could prevent game theory from being taken seriously).

This section uses the definitions of metaphors provided earlier to explain the confusion around the status of BI. The first, if obvious, possibility is that Aumann is under the spell of a constitutive metaphor different from the one adopted by his critics. Keeping in mind that constitutive metaphors, as noted by Klamer and Leonard, are hard to specify concretely, we can posit that Aumann's constitutive metaphor casts the foundations of game theory in mathematical logic, which is the source domain of its theoretical results in terms of solution

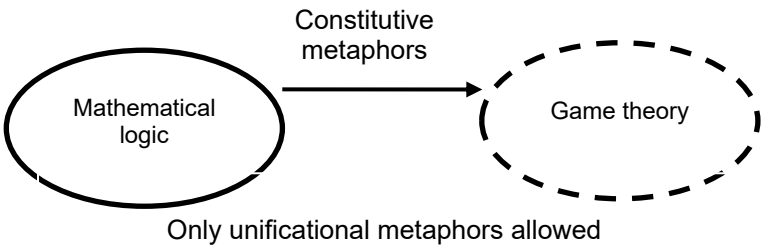
[18] Binmore (1987, p. 196), for example, claims that 'It is not disputed that the results of the play of this [centipede] game by rational players will be that I plays "down" at the first node' Binmore (1997) also provides his own proof.

concepts and equilibria. Within such a constitutive metaphor, there is little sensitivity to the context or type of game. Indeed, most of Aumann's results can be applied to a range of games, not one. Here there is less concern for the structure and context of the game than his critics would like. In mainstream economics there could be other competing and conflicting constitutive metaphors behind the critique's claim that game theory may become irrelevant if it adopts too abstract or idealised foundations à la Aumann. The critique operates under at least one, possibly various, constitutive metaphors that do not use the same source domain as the foundation of game theory.

If this controversy is indeed about the adoption of different constitutive metaphors, it should be acknowledged as such. The confrontation, accordingly, is not over who has the best mathematical proof or which theory is more supported empirically – but which constitutive metaphor is more adequate for the foundations of game theory. Focusing on constitutive metaphors also forces questions on explaining why different constitutive metaphors are employed by different individuals. While helpful in some ways, framing the controversy simply as one of constitutive metaphors fails to provide a comprehensive explanation for the controversy. For a start, it would force us to place Binmore, Samuelson or Bicchieri under the spell of one constitutive metaphor, a highly doubtful claim. Indeed, there could be various constitutive metaphors uniting the opponents of Aumann on what the foundations of game theory ought to be based. Secondly, the conflict of constitutive metaphors does not rule out the possibility that some of Aumann's critics adopt the same constitutive metaphor of mathematical logic (eg., Reny, 1993). And thirdly, among those who defend the equilibrium of BI, not all use mathematical logic 'that closes the mind' but stories and common-sense arguments (eg., Broome and Rabinowicz, 1999; Sobel, 1993). Unless there is a yet unidentified constitutive metaphor, which could tightly explain by regrouping those for and against the justification of BI via common knowledge of rationality, constitutive metaphors, for the time, being are not sufficient to explain the controversy away.

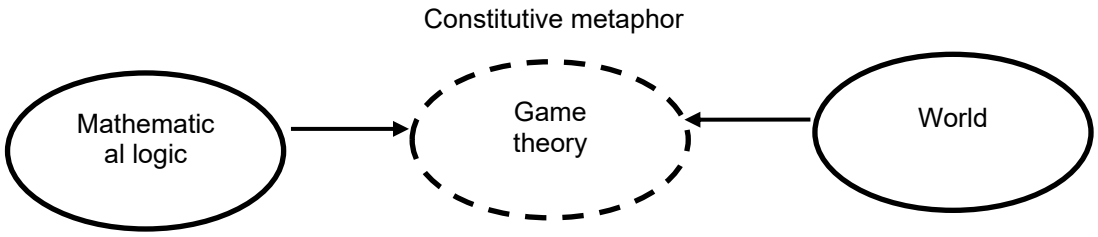
This does not mean constitutive metaphors play no role in the controversy. Inspired by Popper's (1972) three 'worlds', a finer possibility is to frame the controversy as one of three metaphorically interconnected, but relatively autonomous, domains namely, mathematical logic, the game and the economic world (see Figures 3-5). A similar division is used by Grüne-Yanoff and Schweinzer (2008) (and more recently Grüne-Yanoff and Lehtinen 2012) to describe the architecture of game theory. Their view is, as ours, but lacks the metaphorical theoretical justification that explains the triptychal architecture. Game theory connects or bridges the gap between the domains of mathematics and the world. It is a semi-autonomous model between mathematical logic and the complex economy (Morrison and Morgan, 1999). Each source domain maps its properties into game theory. Accordingly, mathematical logic and the world map their properties into decision theory, which then generates game-theoretic models. The metaphors on the right hand side that connect the model to the economy are much wider than the unificational metaphor that connects mathematical logic to the model – because the model, and more so the world, are highly complex entities.

Figure 3 Aumann's constitutive metaphor



Aumann's foundation of game theory is limited to the constitutive metaphor in Figure 3. His metaphor allows only unificational mappings that connect mathematical logic to game theory.[19] This is where Aumann is mostly active (in the controversy at least): he maps mathematical, logical properties into game theory, constructing different models that say nothing specific about the right-hand side – the economic world. In a wider constitutive metaphor – various kinds of heuristic, unificational, heterologous and homologous metaphors operate between the game and the world on the right-hand side. Binmore, Samuelson and others partake in the controversy using metaphors from both ends (Figure 4). Accordingly, they do not allow metaphors from the left-hand side only. While they use some left-hand side mapping, they also focus on metaphors that hook game theory to the world.

Figure 4 Binmore and Samuelson's metaphorical mappings



The changing metaphors across the triptych can lead to identificational slips of the form identified by Khalil, or to the breaches of invariance identified by Lakoff. Such slips and breaches are explained by the prevalent understanding of what games are and what they are attempting to do. They may be strategically used to undermine the work of an opponent. Binmore (1997), for example, freely uses proofs – and even has his own version of the BI equilibrium proof – yet he at the same time criticises Aumann's use of mathematical logic to prove the BI equilibrium. Binmore and Samuelson's slip is to project heterologous or homologous metaphors – that operate between the game and the economic world (via simulations and computational economics) – onto the unificational relationship that connects the game to mathematical logic. In the language of the Lakoffian invariance principle, their metaphorical mapping violates the image-schematic structure that mathematical logic maps into the game, its target. The acceptance of this identificational slip, or breach of invariance, is of course contingent on accepting our description of the controversy as a metaphorical triptych between mathematics, the game and the world.

Within each of the constitutive metaphors, the participants in the controversy employ distinct heuristic, heterologous, homologous or unificational metaphors. Since Aumann (1995) is working on the left side of the triptych, his proof is consistent with the constitutive metaphors that unify game theory and mathematical logic. Such unificational metaphors have the principal as the game and mathematics as subsidiary. The connection is unificational because the same principles of mathematical logic exist in the principal and the subsidiary subjects. Without exception, the properties of the mathematical proofs are mapped as possibilities of solution concepts in the game (they constrain the theoretical form of the game in the same way as the structure of the game constrains the metaphors that connect it to the economic world). One such proof uses CKR and rationality to explain the game, or the equilibrium of the game, via BI. When Aumann notes that he wants to keep the proof as

[19] Hesse (1966, p. 137) claims that Aristotle 'speaks in several places of the basic truths of logic and mathematics as being "one by analogy" when they apply in different fields.'

transparent and as simple as possible he echoes Bicchieri – that familiarity and manageability are two parameters that guide the selection of metaphor. Aumann is comfortable with the principles of mathematical logic (his subsidiary subject is both familiar and manageable) to prove that the equilibrium entails that the first mover plays down. Furthermore, Aumann (1995, p. 6) suggests, this proof is expansionary since there is a modern refinement literature supporting it and his own work extends a number of recent papers on similar fundamental notions of non-cooperative game theory.

Binmore and Samuelson are more sensitive to the fact that they are dealing with metaphorical choices and acknowledge that (i) context matters especially when nodes not meant to be reached by rational players, *are* reached (Binmore, 1987, p. 196); and that (ii) a preliminary informal classification of different equilibrating processes, through the choice of interesting environments, in which games are played should be made (Binmore, 1987, p. 183). As noted in section 2, context is critical for metaphors to be rigorously interpreted. Aumann, however, suggests that his proof is context-free and works as an ideal gas whose implications are not affected by what happens in practice. Statements by Binmore (1997, p. 28) that 'it is at the interpretive level that the importance of common knowledge assumptions needs to be acknowledged' are significant for choices of metaphors. Binmore and Samuelson are explicit that Aumann's subsidiary domain (mathematical logic) is inadequate to justify the computation of equilibria. They claim that the current sequence of 'axiom-definition-theorem-proof' does not just close the mind to irrelevancies (a good thing), but that it also closes the mind to issues it is perilous to neglect. They suggest instead that the proper way to identify potential equilibria is by using simulations and stories to interpret counterfactuals. Such modes of reasoning are sanctioned by the broader, constitutive metaphor that connects the game to the world.

Binmore (1987) presents his approach to game theory as fundamentally different from Aumann's. He, firstly, posits an algorithmic, 'machine programmable' definition of rationality.[20] Secondly, Binmore and Samuelson (1996, p. 114) proceed to search for stories that could explain deviation from the rational BI path.[21] Binmore and Samuelson list stories to overcome Aumann's problem, which is seen as the traditional approach to game theory. Their approach identifies mechanisms that explain deviation from rational play (trembling hand, irrational mistakes, defective reasoning). What is the relationship between stories and metaphors? The stories use metaphors to form subsidiary subjects whose properties are mapped onto the principal subject – the solution concept of the game that needs explanation. As Morgan (2007, p. 169 emphasis added) states for the prisoner's dilemma, 'the narratives translate the prisoners' situation into the economic situation – *they link particulars to particulars* – and 'explain' how it is, for example, that two large firms can end up doing damage to each other just as the prisoners end up with the double-defect outcome'. The constitutive metaphor sanctions a particular type of story which uses a particular type of heuristic, unificational, heterologous or homologous metaphor to connect distinct domains. The metaphor and the story thus complement each other.

[20] For Binmore (1987, p. 181), a rational decision process refers to the 'entire reasoning activity that intervenes between the receipt of a decision stimulus and the ultimate decision, including the manner in which the decision-maker forms the beliefs on which the decision is made. In particular, to be rational will not be taken to exclude the possible use of the scientific method.'

[21] Their alternative interpretation of the centipede game involves a husband who, after missing his mortgage, explains to his (furious) wife that he would not have lost the repayment had he been dealt the ace of diamonds rather than the queen of spades in last night's poker game. They point out such counterfactual stories are not obtained from abstract mathematical contemplation, but as stories from the world. In the centipede game Binmore and Samuelson discuss two possible competing interpretations of counter-factuals. Both stories provide an explanation of irrational play, or play that plays across not down or out.

Two of the stories identified by Binmore and Samuelson provide counterfactuals without which rationality and deviation cannot be accounted for. Selten (1978), on the one hand, posits deviations from the BI path as a trembling hand which makes mistakes. Kreps et al., (1982), on the other hand, focus on modelling incomplete information into the finitely repeated prisoner dilemma. Kreps et al., posit that when a player is not sure what the other will play, and there is a small positive probability ($\delta>0$) she may cooperate, tit-for-tat cooperation will be the equilibrium outcome for long periods in finite games, including the prisoner's dilemma. A third possibility, not discussed in Binmore and Samuelson, but suggested by Coleman (1998), is to substitute rationality (and therefore modify CKR) with non-monotonic reasoning that reflects common-sense, everyday reasoning. This modification, Coleman argues, solves the BI paradox by offering theoretical options for players that face the unexpected across play in the centipede game, cooperation in the repeated prisoner's dilemma, or the declaration of a price war to prevent further entry in the chain-store game. All three stories provide entry points for distinct metaphors.

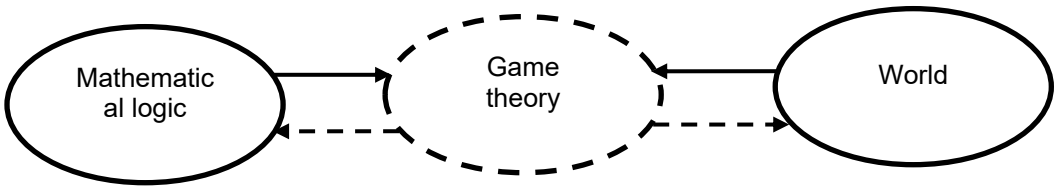
Who has the better story-metaphor combination? Binmore and Samuelson consider this is a wrongheaded question, which has no absolute answer, since it is always necessary 'to look at the context in which the game is played for inspiration on this score. But this context is exactly what is abstracted away when one adopts the conventional mathematical formalism' (p. 115). Thus, the trembling hand story is not applicable to chess – it provides a poor mapping from one domain to another (a chess player is unlikely to consistently make the same mistake due to a trembling hand). Binmore (1987), accordingly, states that irrational play in games such as chess should be modelled – not as trembling hand mistakes – but as defective reasoning. Which metaphor provides the most adequate mapping depends on the type of game and the context of application.

Understanding the dynamic interdependence-autonomy between the three domains of game theory can shed some light on its evolution. Whether the constitutive metaphors are fixed or not, whether one can take over from the other, is an open question I further elaborate upon in the conclusion. The triptych also depicts how game theory can performatively change both mathematical logic and the world (dashed arrows from game theory to mathematical logic and the world; Figure 5). An example of this possibility is provided by Morgan's analysis of a World War II text by game theoretician Rapoport (see also Rubinstein, 2006)

> 'While Rapoport suggests game theory was taken because of the "civilization" of war, it seems equally part of the process that war became acceptable because it was reinterpreted in game theory terms … the cold war came to be seen as a set of game situations… it comes to the point at which we understand and interpret that [nuclear arms] race as a prisoner's dilemma game' (Morgan, 2007, p. 159).

Properties of the model are mapped onto the world via game theory – the model changes the way the world is perceived ('the cold war came to be seen as a set of game situations'). But there is another metaphorical loop that maps properties of the model into mathematics, pushing for novel interpretations in mathematical logic and leading to developments in mathematics (mathematics as an applied science that can experience empirical discoveries and novel interpretations). Accordingly, the generation of new game theoretic models can lead to the development of novel mathematical objects, theories and techniques.

Figure 5 Performativity and game theory



The last point to be made here refers back to the BI micro-industry mentioned in the introduction. I distinguish between pre-Aumann (Basu, 1977; 1990; Bicchieri, 1988b; 1989; Binmore, 1987; Pettit and Sugden, 1989; Reny, 1993; Selten, 1978; Sobe,l 1993; Sugden, 1992) and post-Aumann (1995) publications (Ben-Porath, 1997; Broome and Rabinowicz, 1999; Aumann, 1996a,b; 1998; Binmore, 1996; 1997; Binmore and Samuelson, 1996; Rabinowicz, 1998; Stalnaker, 1996). Morgan proves helpful here again with her contention that game theory has grown

> 'from the narratives, which… go through a process of matching the economic situation with the game situation and then exploring how and why it does not fit. When it does not fit, a new version of the game is developed with slight changes in the rules, payoffs, or information arrangements' (Morgan, 2007, p. 176).

A review of only a few specimens, from post-Aumann (1995), supports Morgan's diagnosis. Stalnaker (1996), for example, posits common *beliefs* of rationality instead of CKR to defend the BI equilibrium (while Sugden (1992), pre-1995, does the opposite, namely, he uses so-called entrenched common beliefs to overcome the paradox of CKR and BI); Rabinowicz (1998) defends BI for a class of BI-terminating games where rationality is a choice of moves not strategies; Ben-Porath (1997) assumes CKR only at the first node, exploiting the distinction between certainty, which allows surprises (playing cross with probability 0), and knowledge which does not; Aumann (1998) distinguishes between *ex ante* and *ex post* knowledge operators of rationality and argues that the proof of the BI equilibrium in the centipede game via (a less intuitive) *ex ante* definition of rationality subsumes (a more intuitive) *ex post* definition of rationality. And so on and so forth. With some exceptions (cf., Camerer, 2003b and Colman, 1998), the publishing micro-industry on the BI controversy does not empirically confront BI, CKR or rationality. Instead, it creates interminable new taxonomies based on changing assumptions, introducing new definitions, logical proofs, lemmas and theorems. It is not clear to me where lies the epistemological contribution to social scientific knowledge of these additions to the BI controversy. I will further comment on this in the conclusion.

6. Conclusion

In this paper the cognitive efficacy of metaphors to explain a controversy in game theory was considered. It suggests that, appropriately employed, metaphors – as theoretical descriptions of the explanandum – can shed light on the source of the controversy around BI among game theorists. A metaphorical account casts the disagreement as one primarily due to protagonists operating under a different metaphorical spell. Those involved in this controversy published past each other over a period of a few years, possibly because the source of their misunderstanding – and that which would have aligned the discussion plane – is an

acknowledgement of the strategic use of metaphors. If economists trade in model building, model-based reasoning has not led to the breakthrough that would have reconciled their differences. The metaphor was introduced as a simpler, sturdier and possibly, looser mode of reasoning to explain why there is a controversy. The metaphorical explanation – if adopted – would also move the controversy to a common plane, focusing the discussion on the context and source of disagreement. Even if the disagreement does not vanish and protagonists stick to their guns, as it were, they would have at least narrowed their disagreement to the choice of metaphor.

Binmore and Samuleson's critique of Aumann drove us to consider various uses of metaphors. At the highest level, the level of the paradigm, the confrontation is over which constitutive metaphor is more adequate, not which proof is more convincing or the extent to which proofs are weakened by empirical evidence or common sense play. Other metaphors within the theme (or allowed mappings) of the constitutive metaphors were identified.

Our metaphorical account of the controversy entails that any potential change is a complex and negotiated endeavour over constitutive metaphors. Any change must be sanctioned by the constitutive metaphor which acts as a filter. Unless the change is consistent with the mapping of the constitutive metaphor, it will be rejected for committing identification slips and breaches of the invariance principle. Binmore and Samuelson's attack on the 'axiom-definition-theorem-proof' sequence could be an example of an identificational slip that challenges the prevalent constitutive metaphor that posits a unificational relationship between game theory and mathematical logic.

The benchmark of a successful constitutive metaphor was linked to its uptake and expansion in the discipline it operates in. A metaphor's success is partly a function of the new perspectives, interpretations and explanations it generates. Metaphor, as pointed by Hesse (1966, p. 177), forms an essential element in 'the continuous adaptation of our language to our continually expanding world'. The metaphorical mapping that projects mathematical properties and relationships onto strategic decisions giving rise to game theoretic modelling and under which some of our protagonists operate, is now commonly accepted. This metaphoric mapping expanded the analysis of strategic decisions into new directions. It is, however, now an established metaphor and may be categorised as dead, having achieved success.

We cannot rule out that, in the future, new metaphors will project novel mapping onto the strategic analysis of decisions, transforming, weakening or strengthening the grip of the now dead mathematical metaphor. Nevertheless, uptake and expansion remain insufficient success criteria to adjudicate whether a successful metaphorical choice has been made for a realist to whom scientific theories need to explain by referring to the world. Hesse identifies a difficulty, which turns out to be a strength, in the interactionist view of metaphors and their referents. Rather than prove that metaphors in science refer in the orthodox understanding of refer, she argues that the orthodox view of explanation fails to refer whereas the metaphoric view refers to the mappings of properties and functions from one domain to another. In this way the understanding of 'referring to' is no longer the static application of a covering law and correspondence rules that connect theory to observation, as in the orthodox deductive view of explanation. Instead,

> 'the process of metaphoric description is such as to cast doubt on any simple identification of the metaphor's reference with the primary system. It is claimed in the interaction view that a metaphor causes us to "see" the primary system differently and causes the meanings of the terms originally literal in the primary system to shift toward the metaphor' (Hesse, 1966, p. 167).

Hesse then asks,

> 'how can initial similarities… justify such changes in the meanings of words and even, apparently, in the things themselves? Man does not in fact change because someone uses the wolf metaphor. How then can we be justified in identifying what we see through the framework of the metaphor with the primary system itself? It seems that we cannot be entitled to say that men *are* wolves, sound *is* wave motion' (Hesse, 1966, p. 167).

Ultimately, Hesse, like Khalil, wants to argue that metaphors in science are consistent with realism – they explain by referring to the world. However, how this can be achieved if 'the interaction view implies that the meaning of the original literal language of the primary system is changed by the adoption of the metaphor' (p.169)?

Hesse contends that the use of metaphors discards deductive literal descriptions that are inadequate and, without abandoning deduction, the metaphoric view

> 'focuses attention on the interaction between metaphor and primary system, and on the criteria of acceptability of metaphoric descriptions of the primary system, and hence not so much upon the deductive relations that appear in this account as comparatively uninteresting pieces of logical machinery' (Hesse, 1966, p.174).

Unlike the deductive view of explanation, which makes use of correspondence rules that fail to refer because the meaning of explanandum is shifting with the introduction of new theoretical terms in the explanans, in a metaphoric explanation, 'there is no problem about connecting explanans and explanandum other than the general problem of understanding how metaphors are introduced and applied in their primary systems' (p. 175). Metaphors refer (in the strong sense of prediction)

> 'since the domain of the explanandum is redescribed in terminology transferred from the secondary system, the original observation language will both be shifted in meaning and extended in vocabulary, and hence that prediction in the strong sense will become possible. They may, of course, turn out not to be true, but that is an occupational hazard of an explanation or prediction' (p. 176).

Accordingly, metaphoric explanation is consistent with realism, not in the orthodox sense where theories refer to observation, but by referring to the properties and functions transferred from one domain to another. Metaphoric explanation is thus consistent with an interactionist relationship between primary and secondary subjects in which the perception of the primary subject is consistently shifting with novel metaphorical mappings, offering a view of scientific explanation consistent with a continuously expanding meaning of the explanandum, one that is not engraved in stone, but is as malleable as the social world.

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Reassessing Marshall's Producers' Surplus: a Case for Protectionism

Daniel Linotte, St Antony's College, Oxford, UK
daniel.linotte@gmail.com

Abstract

The rationale for liberal economic policies refers *inter alia* to the so-called producer and consumer surpluses, namely welfare concepts which were proposed by Alfred Marshall in his seminal work *Principles of Economics*, first published in 1890. In the case of trade policy, relying on surpluses and referring to the 'small country case', it is recommended to remove tariff barriers imposed on the imports of commodities because it should increase welfare and, in theory at least, the losers of such a trade policy orientation can be compensated with the use of adequate transfers from winners.

Despite extensive use, the concept of surpluses still raises key questions that may alter the case for free trade. Thus, from a purely semantic perspective, the concept of producer, as presented in Marshall's work, seems to be broader than the concept which is proposed in the dominant economic discourse; in other words, workers should also be seen as producers.

Assuming that the workers are considered as producers, their wage rents must be taken into account when discussing the impacts of trade liberalisation; in addition, the welfare costs of unemployment caused by the opening of national economies should be included – as a result, the case for free trade weakens considerably, it could even vanish.

Keywords: Alfred Marshall, trade liberalisation, producer surplus, wages, unemployment, ethics.

Introduction

Trade liberalisation remains a high priority on the agenda of most Western leaders, especially in the EU. Such a strategic policy option is supported by negotiations taking place at different levels: bilaterally, i.e. between countries, in the context of regional economic integration agreements, and within the framework of the World Trade Organisation (WTO). Overall, more than 500 free trade agreements have been notified to the General Agreement on Tariffs and Trade (GATT) / WTO and most of them are being implemented.

The rationale for liberal economic policies refers *inter alia* to the so-called producer and consumer surpluses, namely the welfare concepts that were proposed by Alfred Marshall in his seminal work *Principles of Economics*, first published in 1890. Thus, in the case of trade policy, relying on surpluses and referring to the 'small country case', it is recommended to remove tariff barriers imposed on the imports of commodities because it should increase welfare and, in theory at least, the losers of such a trade policy orientation can be compensated with the use of adequate transfers from winners.

Despite extensive use, the concepts of surpluses still raise key questions that may alter the case for free trade:

(i) In his *Principles*, Marshall draws a line between two supply curves, one for output expansion and one for output contraction, with two different elasticities. Such a distinction is not taken into account when discussing the welfare impacts on producers of trade liberalization – referring to them would reduce the benefits of free trade.

(ii) From a purely semantic perspective, the concept of producer, as presented in Marshall's work, seems to be broader than that which is proposed in the dominant economic discourse; in other words, workers should be seen as producers.
(iii) Assuming workers are fully considered as producers, at least their wage rents should also be taken into account when discussing the impacts of trade liberalisation.
(iv) Considering the unemployed, wage losses may not fully reflect the decline of welfare created *inter alia* by the loss of jobs, which means that the welfare consequences of liberalisation may go beyond the loss of income and rents.

These issues are presented and discussed in this paper.

In particular, following a short overview of free trade agreements and ongoing trade negotiations (section 1), the traditional case for trade liberalisation is presented in section 2. The importance of the traditional case is underlined referring to WITS (World Integrated Trade Solutions), namely a trade liberalisation simulation tool proposed on the website of the World Bank (section 3). The two Marshallian supply curves are considered, as well as their implications for the assessment of the welfare impacts of trade liberalisation (section 4). Following the work of Marshall, the workers, fully recognised as producers, with their wages and corresponding surpluses, must be added to the welfare analytical framework. Combining profits and wages losses means that value-added changes are taken into account; such a perspective, more in line with Marshall's theory, leads to new conclusions about the welfare impacts of trade liberalisation – eventually, it would not support trade liberalisation and could even justify protectionism (section 5).

Referring to trade liberalisation, evidence about wages and the cost of unemployment for the unemployed in leading economies, is provided in section 6. Reported facts underline the need to move welfare analysis beyond the traditional surpluses and rents when discussing the impact of free trade agreements and, subsequently, consider a paradigm that would include, for instance, Sen's capabilities approach (section 7).

1. Free Trade Agreements in the Global Economy

Free Trade Agreements (FTAs) are nothing new. One of the first, well-known FTAs is the so-called 'Cobden-Chevalier Treaty' which was concluded between the UK and France in 1860 – it was initiated by Cobden to consolidate peaceful relations between the two former belligerents and, as a result, remove the threat of a French invasion of the UK. Considering regional trade agreements, the German 1834 *Zollverein* (customs union) is the traditional historical reference; it was also perceived as a major step toward the 1871 German unification and, moreover, it is often associated with the academic work of the German-American economist Friedrich List (1789-1846), who advocated protectionism to support the development of 'infant industries' – by adopting such a policy, Germany would become one of the leading industrial powers before WWI, alongside the UK and the US.

Considering the post-WWII period, overall, 124 FTAs have been notified to GATT between 1948 and 1995. Since the creation of the WTO in 1995, more than 400 agreements were added.

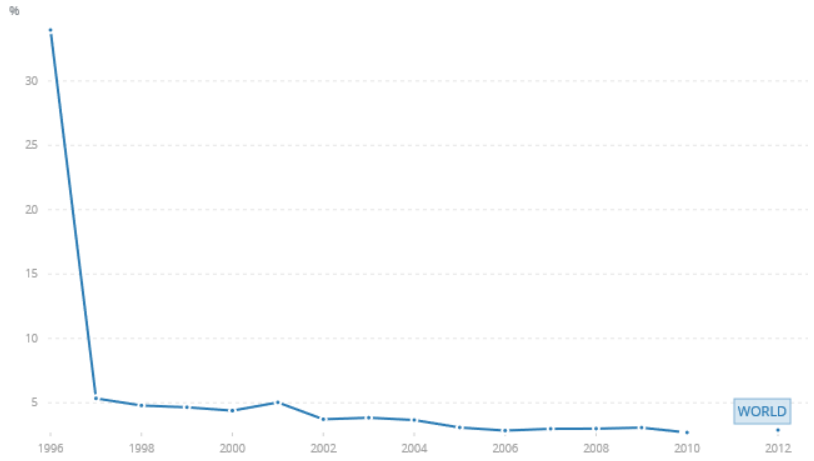
More recently, efforts are being made to merge progressively bilateral FTAs and replace them by regional trade agreements. Such important moves include negotiations for a Trans-Pacific Partnership (TPP) Agreement between ASEAN countries and six other WTO members, and the decision to achieve the African Tripartite Agreement between three regions

(Common Market for Eastern and Southern Africa (COMESA), East African Community (EAC) and Southern African Development Community (SADC)).

FTAs first concentrated on trade in goods, in particular non-agricultural ones. Following the creation of WTO, with the conclusion of the Uruguay Round, trade in services and non-tariff barriers (which include *inter alia* technical standards, sanitary and pythosanitary measures, customs procedures and formalities and international payments regulations) are now being addressed. For Non-Tariff Barriers (NTBs), there are also attempts to measure tariff equivalents with CGEMs (Computable General Equilibrium Models).

In other words, the number and the coverage of FTAs have increased during the last two to three decades – as a result, the average rate of protection for the world is presently very low (see graph 1).

Graph 1 Average nominal rate of protection for the world

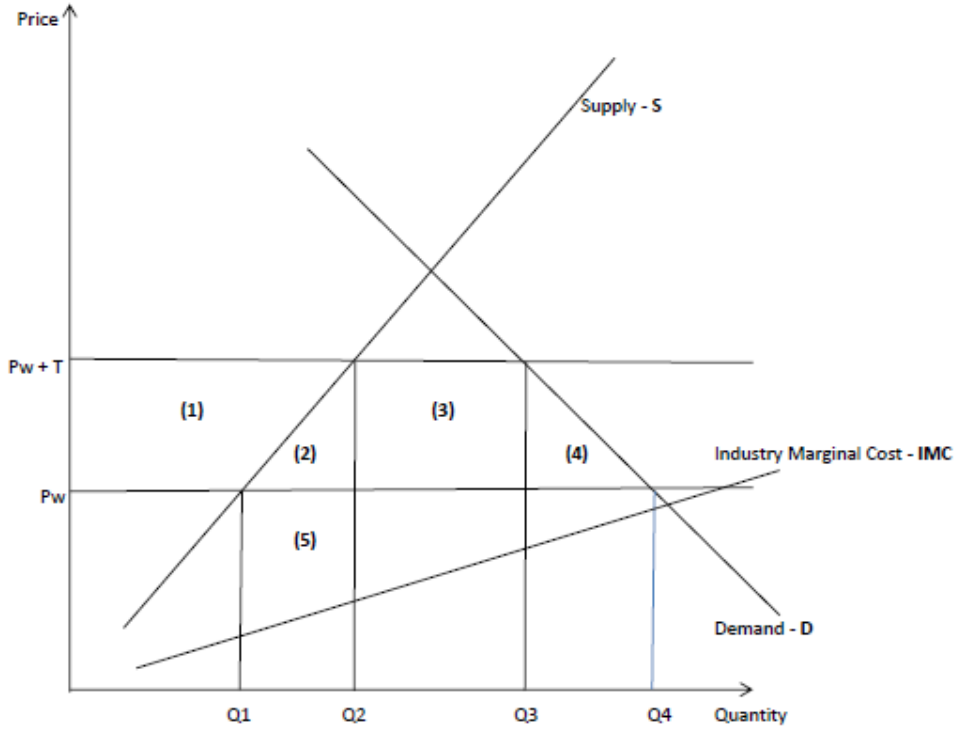


Source: World Integrated Trade Solution (WITS) the World Bank website.

2. Trade Liberalisation and Welfare: the Dominant Paradigm

In addition to Ricardo's law of comparative advantage, several frameworks have been proposed to justify free trade. In that respect, one of the most popular tools for the study of the economic, trade and welfare impacts of trade liberalisation is based on 'partial equilibrium comparative static analysis'. The partial equilibrium perspective considers only the effects of a given policy action – e.g. the removal of an import duty – in the market directly affected. It does not take into account the interactions between the various markets in a given economy. One of the advantages of such an approach is that it mainly refers to the concept of rent or surplus, in monetary terms, for both producers and consumers. In addition, this approach avoids an aggregation bias corresponding to situations where tariff removal would create nonexistent welfare benefits because of not taking into account the conditions related to single products (for more details, see Amjadi et al., 2011).

Figure 1: Welfare impact of trade liberalization – the traditional producer surplus versus value-added



Referring to figure 1, presenting the small country case, seen as a price-taker on world markets, the removal of a tariff T imposed on an imported good lowers its price on the domestic market from (Pw + T) to Pw. As a result of tariff removal, domestic production falls from Q2 to Q1, and consumption increases from Q3 to Q4. In terms of welfare, the so-called producers lose profits or area (1), because of the lower price and reduced sales. The state loses all its revenue measured by the area (3). Consumers or buyers gain [(1) + (2) + (3) + (4)]. In total, there is a net welfare gain for the small importing country equal to the sum of the traditional welfare triangles (2) and (4), with area (2) categorised as a production gain from the (presumed) better reallocation or redeployment of resources elsewhere in the domestic economy, while area (4) is the consumer gain caused by the expansion of the market, as a result of the lower price. Under 'normal conditions', [(2) + (4)] is always a positive value, which means the outcome of the proposed standard analysis justifies the removal of the import duty or the full and unequivocal adoption of free trade. The same analysis can also be used to justify the complete removal of export subsidies, with fiscal and consumer gains outweighing producer loss.

Historical perspective

The standard or textbook economic and welfare analysis of trade liberalisation is the outcome of specific interpretations of Marshall's work. Thus, two of Marshall's successors initiated the analysis presented in this section: Henry Cunynghame, who studied with Marshall, is credited with applying the Marshallian curves to assess the impact of liberalisation on a single market, for two open countries trading together and using import and export taxes (Murphy, 2017). In an article published in 1908, Enrico Barone – a prolific neoclassical and mathematical economist (seen as the father of scientific socialism), and military historian – derived the familiar welfare conclusions, without referring to the workers (Heal, 1976; McLure, 2006).

Marshall applied his analysis to trade policy, using consumer and producer surpluses; however, this work was not published at the time (Whitaker, 1975).

3. WITS and the free trade bias

Relying on the traditional welfare paradigm, as presented in the previous section, World Integrated Trade Solution (WITS), is an on-line tool developed jointly by four organisations, namely United Nations Conference on Trade and Development (UNCTAD), United Nations Statistical Office (UNSO), WTO and the World Bank. It can be used free-of-charge for estimating *ex ante* impacts of the removal of import duties. These impacts relate to trade diversion and creation, price, tariff revenue, consumer surplus and welfare changes.

Surprisingly, WITS says little or virtually nothing about domestic supply conditions. The exclusion of producers could reflect a pro-free trade bias and express the liberalisation agenda of the organisations involved in the development of WITS – this seems obvious for WTO and the World Bank.

4. Non Unique Supply Curves and the Producer's Loss of Welfare

Figure 2: Marshall' supply curves

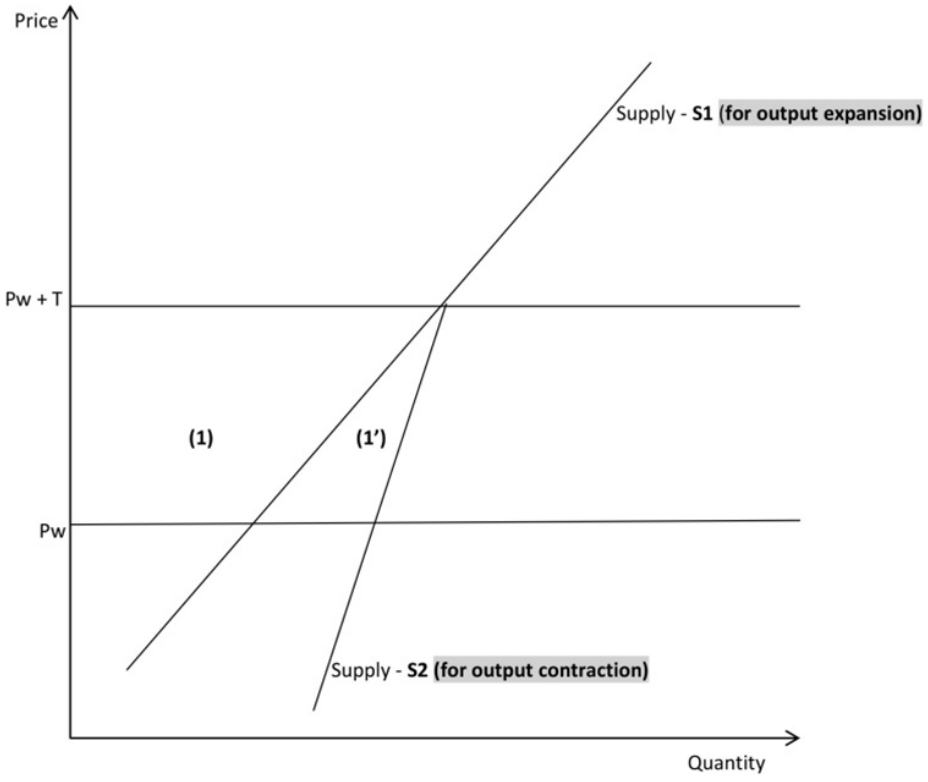


Figure 2, reflecting Marshall's views, as they are presented in Appendix H of the *Principles*, and limited to the domestic supply side, shows two supply curves. S1 corresponds to output expansion, created by a price increase. S2 is the supply curve corresponding to a price decline, following, for instance, the removal of an import duty – it has a smaller price-elasticity than S1. The difference between the price-elasticities of S1 and S2 can be explained by the

fact that the economies that were associated with organising higher output levels are not totally lost when output levels contract, because of the fall in price paid to the producers; in other words, the new supply curve will reflect the improvements in production conditions achieved during previous periods (for instance, with the adoption of so-called 'lean production' methods that were introduced by Japanese firms, Toyota in particular) and, as a result, limit the fall in output when the price decreases.

The welfare implication of such a distinction between the two supply curves is straightforward: with S2, the loss of welfare corresponds to [(1) + (1')], which is larger than (1) – reflecting the lower price-elasticity of S2. In other words, by introducing S2, the costs of trade liberalisation for the producers are larger than the traditional one, as reported in figure 1. It also means that the case for trade liberalisation is somehow softened. However, when adding the demand side and the state, in terms of welfare, the overall impact remains positive, which still supports a liberal orientation for the trade policy regime.

5. The Workers Seen as Producers and Corresponding Welfare Implication

Considering producers *per se*, in his *Principles of Economics*, Marshall mentions both 'producers' and the workers, who are seen as: (i) direct producers, with their wages, and (ii) indirect producers, or owners of capital.

> 'While national income or dividend is completely absorbed in remunerating the owner of each agent of production at its marginal rate, it yet generally yields him a surplus which has two distinct, though not independent sides. It yields to him, as consumer, a surplus consisting of the excess of the total utility to him of the commodity over the real value to him of what he paid for it…
>
> Another side of the surplus which a man derives from his surroundings is better seen when he is regarded as producer, whether by direct labour, or by the accumulated, that is acquired and saved, material resources in his possession.
>
> As a worker, he derives a worker's surplus, through being remunerated for all his work at the same rate as for that last part, which he is only just willing to render for its reward; though much of the work may have given him positive pleasure.
>
> As capitalist (or… as owner of accumulated wealth in any form) he derives a saver's surplus through being remunerated for all his saving…' (Marshall, 1920, Appendix K, p. 494)

In other words*, stricto sensu*, workers' surpluses would first correspond to pleasure given by work and their remuneration. Overall, and despite some semantic 'issues' related to the complexity of Marshall's analysis, when considering his work, wages should include a category of surplus reflecting the importance of workers as producers – which is what they are, in fact.

The Case for Protectionism

Wages are now added to profit when analysing the welfare impacts of the removal of import duties. In other words, the wage element must be removed from the supply curve S, and what

remain are the industry marginal costs – or IMC in figure 1 – related to various inputs, not the factors of production, which does not prevent suppliers from continuing their quest for a maximum profit along the supply curve S (for the sake of simplicity, there are no separate supply curves, as presented in section 4).

Again, the full elimination of the import duty affects firms' profits, state revenues and consumers' welfare – the sum of all impacts corresponds to the traditional welfare triangles, (2) and (4) in figure 1.

Referring to workers, the loss of wages corresponds to the difference between the supply curve and the industry marginal cost curve, IMC, related to the use of inputs. Thus, the wage change caused by the reduction of production from Q2 to Q1 corresponds to [(2) + (5)], a result which raises questions:

- What is the corresponding loss of welfare?
- Should 'all-of-wages' be taken into account, or just corresponding rents, referring to labour supply conditions?

There is no straight answer to these questions.

Considering wage rents, *a priori* no one knows where the workers affected by liberalisation come from, i.e. to what segments of the labour supply they can be associated with, and what are the corresponding rents. However, it can be assumed that these rents are positive values, which means that the workers' rent-based loss of welfare represents a non-zero fraction of [(2)+(5)]; more formally, the workers' loss of welfare equals: {α [(2) + (5)]}, with '$0 < \alpha < 1$'.

When adding wage rents, the total welfare change caused by the elimination of an import duty is: {(2) + (4) - α [(2) + (5)]}, which can be positive, zero or negative, depending on the actual value of α, an unknown parameter. If all 'all-of-wages' could be seen as rents (in which case α = 1 – an extreme and 'absurd' case, perhaps), the total welfare change would correspond to [(4)-(5)], with a corresponding cost for producers that is equal to the value-added change. In other words, the (lower) greater is α, the (greater) lower is the case for free trade.

Moreover, workers could also lose their jobs and stay unemployed for long periods of time – and some of them without unemployment benefits. Such desperate situations, which go far beyond 'partial equilibrium comparative static analysis', imply that welfare losses could be much larger than wage rents, and for some workers they could correspond to 'all-of-wages', even more, etc.

To conclude, by adding wage rents and the (un)employment status of the workers to the welfare analysis of the impacts of trade liberalisation, the case for free trade weakens, and there could eventually be a relevant argument for protectionism. In fact, from a theoretical perspective, there seems to be no 'magic' formulae or combination of geometric shapes (triangles and more complex ones) to evaluate the final welfare change and allow for a firm and final decision about the implications of the trade policy regime.

6. Evidence About Wages and Unemployment

The relationship between trade and wages is a complex one. It seems that past studies are not fully conclusive. Nevertheless, in this section we report evidence about wage dynamics in developed countries. Trends are definitely worrying and, to some extent, they can be related to the on-going globalisation process. Also, considering the evidence from the US and other

advanced countries, for the first time after WWII, the younger generations might be confronted with declining living standards in the long run. In addition, in most Western countries, there are increasing inequalities, which undermines social consensus and stimulates the rise of populism and xenophobia.

After WWII, real wages in US manufacturing grew in line with productivity for about three decades, including the 'golden sixties'. After the first oil shock in 1973, wage dynamics started being disconnected from productivity growth. Thus, according to Bivens and Mishel (2015), net productivity grew by 1.33% percent each year between 1973 and 2014, which is significantly above the annual low 0.20% change for the median hourly compensation (see table 1 for more details).

Table 1 Median hourly compensation and productivity growth in the US, 1973–2014 (Yearly change, %)

Reference variables	**1973–2014**	**2000-2014**	**2007-2014**
Median hourly wage	0.09	0.03	-0.30
Median hourly compensation	0.20	0.13	-0.30
Net productivity	1.33	1.41	0.91
Net productivity–median compensation gap	1.13	1.28	1.22

Source: J. Bivens and L. Mishel (2015), page 8.

The large discrepancy between most wages and productivity corresponds to growing inequalities among workers and between social classes – with a large share of productivity gains going straight 'in the pockets of extraordinarily highly paid managers and owners of capital' (Bivens and Mishel, 2015, p. 23), which could reflect a dramatic shift of bargaining power between workers and their representatives, namely the trade unions, and the ruling elites, and institutional changes on labour markets.

In addition, linkages are made with international trade and the new global economy to explain wage dynamics and inequalities. In the 1960s, US manufacturing was first confronted with growing competition from Japan and European economies. In more recent years, the rise of China as a major producer and key-exporter, and the threat of offshoring production are perceived as factors to explain the significant decline of manufacturing and the pressuring of wages in the US (Levy and Kochan, 2012). Considering services – progress with computers, telecoms, and data gathering and processing – also allows profitable offshoring investments and operations in low-wage countries such as India.

A similar phenomenon is observed in Germany, the leading and largest economy in the EU. Despite higher education levels, German employees have been confronted with stagnant or declining wages since the 1990s. As a result, the share of wages in national income reached a 'historical low' in 2007 and 2008, at 61% (Brenke, 2009). Furthermore, for the most recent years, favourable labour market conditions, with a comparatively low unemployment rate in the EU context, did not help improve workers earnings (DIW Berlin, 2015).

In Japan, for both men and women, real wages increased during the 1990s. During the 2000s, real wages started declining, especially for the 'middle wage workers' (Yokoyama et al., 2016). The UK displays similar features (Bell, 2015).

As mentioned, the reported changes and tendencies can be attributed to several factors, in particular the opening of national economies, with the lowering of trade barriers, for

both goods and services. After years of negotiation, China became also a WTO member in 2001, which means it benefits from the Most-Favoured-Nation (MFN) status in terms of market access and national treatment, contributing to more competition in the global economy and the subsequent decline of Western industries. Moreover, China is involved in trade disputes against Western countries, rendering much more difficult the protection of companies and workers in these countries. Such hard facts underline that the social implications of trade liberalisation can hardly be predicted with the traditional welfare approach relying on Marshallian triangles only and require a broader paradigm to be more conclusive about the linkages between trade and the well-being of people.

In addition to flat real incomes, some countries are enduring high levels of unemployment, which can also be related to trade and may result in high costs, especially for the unemployed.

Assessing the welfare costs of unemployment is a difficult exercise, which must take into account various factors such as the actual loss of income and the worsening of self-esteem, other psychological dimensions, health conditions, and in some cases the voluntary termination of life – or suicide.

Relying on Milton Friedman's permanent income hypothesis and data provided by the German Socio-Economic Panel on the former territories of East Germany over the period 1992-2005, Knabe and Räetzel (2007) conclude that the non-pecuniary costs of unemployment can amount to 2.3 times the loss of income.

Considering mental health *per se*, several studies underline the negative impact of unemployment on the psychological balance of the unemployed, especially in the 30 to 50 or 55 age range. Males are also seen as more vulnerable than females (IWH, 2009). Unemployment may lead to schizophrenia and hospitalisation. The impact of unemployment on physical health is another field of investigation; there is an interface between mental and physical health – with unemployment, stress increases considerably and poor diets prevail, contributing *inter alia* to heart disease; moreover, being unemployed may delay the use of health care because of pecuniary considerations and lead to risk-taking behaviour. The unemployed may also end up living on the street, become homeless and lose any positive status in the society. Such living conditions may imply that the unemployed are no longer part of the labour force, there could be no more job search activity and survival depends on the generosity of others; life expectancy is also shortened (NCH, 2007).

The worst possible cost of unemployment, related to marginalisation and extreme despair, is suicide:

> 'In June 2009, Christelle Pardo, pregnant and with her five-month-old baby in her arms, jumped to her death from the balcony of her sister's flat in Hackney.
>
> Her Jobseeker's Allowance had been stopped because of her pregnancy and this meant that she also lost her Housing Benefit: the local authority was demanding that she return £200 in overpaid HB. She had been turned down for other benefits – her appeals had been turned down twice; her last call (for help) … was made just the day before her suicide' (TUC, 2010, p.1).

With adequate support, the Christelle Pardo case could have ended differently – her fate is not an exception also. Quantitative studies do show that, on average, one in five suicides is associated with unemployment (University of Zurich, 2015). In Japan, recession led to a sharp increase in the total number of suicides, about 30,000 in excess for more than ten years after

1998. These changes are supported by regression analysis; thus, referring to two models (see table 2), when unemployment increases, the number of suicides increases (Chen et al., 2012, p. 85).

Table 2 Regression analysis – suicides and unemployment rates in Japan using quarterly panel data

Dependent variable: Number of male suicide victims per 1000 **Reference period: 1991 QI-2005 QII**		
Variables	**Model 1**	**Model 2**
Unemployment rate	0.424*** (0.106)	0.354*** (0.106)
Number of bankruptcies	--	0.072*** (0.023)
Constant	0.012*** (0.004)	0.008** (0.004)
Number of observations	376	376
R-squared	0.083	0.111
Notes: - Between parentheses, standard errors; - *** (**) means 1% (5%) significance level.		

Source: J. Chen and al. (2012), p. 85.

Since 2008, Greece has been confronted with a sharp economic and financial crisis and 40% of the households now have at least one member who is unemployed; moreover, youth unemployment is more than 60%. Here also, the impact of socio-economic conditions on suicide is significant; in addition, the use of antidepressants and the number of divorces and HIV cases are increasing (Madianos et al., 2015).

The welfare costs of unemployment can reach very high levels indeed, especially for unemployed people and their relatives and families, which underlines the need to add dimensions beyond the traditional welfare triangles, the wages and their rents, to analyse the consequence of trade policies.

7. Moving Welfare Analysis Beyond Rents

Considering issues like mental and physical health, homelessness, and life expectancy, including suicide – because of despair, seems to correspond to paradigms developed, among others, by Sen, Rawls and Nozick. According to Sen, it is most surprising that welfare economics has been unable to integrate such dimensions that are very common in development economics:

> 'One of the extraordinary features of standard welfare economics has been the neglect of information about health, morbidity and longevity. Though these variables have often been taken seriously in the development literature ..., they have typically been ignored in welfare-economic treatises' (Sen, quoted in Atkinson, 1998, p. 8).

Sen proposes an approach based on capabilities to go beyond what he calls 'welfarism' – capabilities refer to the scope of the choice an individual has to function normally. They also

relate to the possibility a person has to achieve his/her potential, his/her 'dreams' perhaps. After being turned down for social benefits, it seems that Mrs Pardo was no longer in a position to live normally.

Rawls focuses on the worst positions in society, which should determine public choice – a 'max-min perspective'. Such extreme positions relate first to the access to primary goods, defined as 'things that every rational man is presumed to want' (Atkinson, 1998, p. 8). Again, Mrs Pardo and her little child seemed to be in one of the worst possible situations.

Nozick's perspective is different. Amongst other things, he considers the way income distribution has been 'brought about' (Atkinson, 1998, p. 8) – to what extent it is legitimate. Such an approach may help clarify the evolution of income distribution in many countries over the last two or three decades; in that respect, the growing discrepancy between productivity and wages, with fast growing bonuses for the 'principals' (owners) and their 'agents' (managers), is worth analysing – in addition to the growing pressures from trade.

These paradigms may suggest new approaches to analyse the impacts of trade liberalisation policies, moving above and beyond the traditional welfare triangles and wage losses. It implies that social sciences must merge their views when studying the consequences of policy choice on the lives of individuals and connections within societies. Such a vision requires a certain emphasis on altruistic thinking and values – for complementing and countering egoistic perspectives found in conventional economics textbooks.

Final remarks

I) Marshall's traditional welfare triangles are widely used in academia and economic policy circles to justify the removal of import duties, and to analyse other policy measures such as, for instance, the use and the elimination of production and export subsidies.

II) Referring to a new understanding of the *Principles*, the traditional or textbook interpretation of Marshall's work is questionable.

III) Considering Marshall' seminal work, there is a distinction between two supply curves, one for output expansion (S1), and one for output contraction (S2) which has a lower price-elasticity than S1; the output contraction curve should be used for assessing welfare changes caused by the removal of an import duty and the subsequent lowering of the price, which implies a higher loss of surplus for the producers/firms and, as a result, it weakens the standard case for free trade.

IV) Workers are not taken into account in the traditional welfare analysis, which refers only to firms, consumers and the state. The exclusion of workers can be related to Barone's 1908 welfare paradigm developed for the analysis of international trade, namely a rather specific interpretation of Marshall's concepts of surpluses and rents.

V) A candid reading of Marshall's work helps identify workers as producers, with their own surplus and/or rents.

VI) When adding workers and their surpluses to the traditional welfare analysis, the case for free trade may just vanish because the welfare impact of the removal of an import duty can be negative – an impossible outcome with the so-called traditional welfare triangles.

VII) In addition, even if the wage-related rent is totally unknown, for most workers, it can be assumed that losing a job and being unemployed has serious welfare consequences. For instance, in the case of long-term unemployment and no unemployment benefit, the loss of welfare should perhaps be measured by *at least* the loss of income.

VIII) The last argument would imply that welfare analysis should move beyond rents and explicitly take into account, for instance, Sen's capabilities related to incomes and wealth. According to Sen, such a move overtakes the so-called traditional 'welfarism'.

IX) Considering the evidence, for the US, Germany and Japan at least, wages have – at most – remained flat for decades, a phenomenon that may have been partly induced by the further opening of their economies and the rise of China as a major trading nation, following its accession to the WTO. In addition, in some countries, unemployment can reach a high level, which may be related to trade and has negative welfare implications.

In conclusion, Marshall's concepts should definitely be reconsidered to better assess the economic and welfare consequences of economic policies, in particular existing and future trade agreements. Furthermore, as policy-related welfare analyses should move beyond rents, it could imply 'breaking the wall' between key academic disciplines, combining at least economics, medicine and psychology (for health and behavioural issues), and philosophy, with a reference to the concepts and the ethical perspectives proposed by Sen and others.

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The Decline of the 'Original Institutional Economics' in the Post-World War II Period and the Perspectives of Today[1]

Arturo Hermann, Italian National Institute of Statistics, Rome, Italy
ahermann@istat.it

Abstract

Original, or 'old', institutional economics (OIE) – also known as 'institutionalism' – played a key role in its early stages; it could be said that it was once the 'mainstream economics' of the time. This period ran approximately from the first important contributions of Thorstein Veblen in 1898 to the implementation of the *New Deal* in the early 1930s, where many institutionalists played a significant role.

However, notwithstanding its promising scientific and institutional affirmation, institutional economics underwent a period of marked decline that spanned from the mid-1930s to the late 1980s, when a new season for institutional economics was set in motion.

In order to cast some light on this complex issue – without any claim of completeness – we have organised the work as follows: in the first section we consider the main interpretations of this phenomenon. In the subsequent sections we analyse a number of 'endogenous' aspects which might have played a significant role in the period of decline: **(i)** the relations of institutional economics with Keynes's macroeconomic theory; **(ii)** the links between theoretical and empirical analysis and the supposed lack of a clear theory; **(iii)** the interdisciplinary orientation.

Keywords: Original institutional economics, social valuation, political economy, interdisciplinarity

JEL Codes: B25, B41, B52, E61

1. The Decline of Institutionalism and the Main Existing Interpretations

The Ascendance and Decline of Institutionalism

Institutional economics originated in the United States in the first decades of the 20th century. Its cultural roots can be identified in the philosophy and psychology of Pragmatism – in particular in the theories of Charles Sanders Peirce, John Dewey and William James – and in the German historical school, whose principles were developed by the scholar, Richard T. Ely, who had a considerable influence on the formation of the first generation of institutionalists.

The main founders of institutional economics were Thorstein Veblen, John Rogers

[1] This paper is a development of work originally published in a chapter of my book, *The Systemic Nature of the Economic Crisis: The Perspectives of Heterodox Economics and Psychoanalysis*, London and New York, Routledge 2015; and, in Italian, in the journal *Il Pensiero Economico Moderno*, 2015 (4). In the present version all the sections have been substantially broadened (and one more, the fourth, added) in order to develop more fully the issues addressed. In particular, the sections related to the central theme of the relations between theoretical and empirical analysis have been totally re-elaborated.

Commons, Walton Hale Hamilton, Wesley Clair Mitchell and Clarence Ayres. Relevant contributions were also made by L. Ardzooni, A.A. Berle, J.C. Bonbright, J.M. Clark, M.A. Copeland, J. Fagg Foster, I. Lubin, Gardiner C. Means, Walter Stewart and many others.

Significant contributions with important connections to institutional economics were provided by, among others, John Kenneth Galbraith, Fred Hirsch, Albert Hirschman, Gunnar Myrdal, Karl Polanyi and Michael Polanyi.

Within institutional economics, two main strands[2] can be identified: **(i)** the *Original (or Old) Institutional Economics* (OIE), formed by the first institutionalists and by subsequent scholars who shared their main concepts; and **(ii)** the *New Institutional Economics* (NIE), which was born in the post-WWII period, composed of economists adopting principles mainly related to the Neoclassical and Austrian schools.

In this regard, it is interesting to observe the significant links between the OIE and, among others, the following theories: **(a)** various strands of sociology and social psychology, including the "Sociological or Ecological School of Chicago", the social psychology of William James and of William Ogburn; **(b)** a number of theories of technological innovation, often referred to as neo-Schumpeterians, which share important concepts with the OIE: for instance, the importance of path-dependency processes in explaining the characteristics of science, technology and innovation in any given context.

The pivotal concepts characterising the OIE can be summarised as follows: ceremonial/instrumental behaviour, instincts, culture, evolution, habits, path-dependency, tacit knowledge, power, technology, collective action, social provisioning, market imperfections, social planning, working rules and social valuing. As noted by numerous authors, the OIE does not present a completely unitary framework. Within this ambit, two main strands can be identified:

i. An approach (the Veblen-Ayres tradition), stressing the dichotomy between ceremonial and instrumental institutions; the role of habits of thought and action; the cumulative character of technology in its relations with workmanship and parental bent propensions.

ii. An approach put forward in different ways by J.R. Commons, W.H. Hamilton and W.C. Mitchell., which centres on the evolutionary relations between the economy, law and institutions; the nature of transactions, institutions and collective action; the role of conflicts of interest and the social valuing associated with them; the theoretical and empirical analysis of business cycles and their relations with institutional setting and policy action; and the role of social psychology for understanding economic and social phenomena.

Notwithstanding a number of differences between (and within) these approaches, the elements of convergence are remarkable. In our view, the observed differences tend to be concerned more the issues addressed than in the basic aspects of the OIE.

Within this conceptual framework, OIE stresses that the presence of an institutional context – with its values, norms, conflicts, organisations, routines, habits and customs – constitutes a necessary factor for understanding the human activity of social provisioning. In other words, every economic action embodies, at the same time, a social, institutional, historical and psychological dimension. Thus, an understanding of economic actions demands a joint analysis of all these dimensions which, for this reason, necessitates the adoption of an interdisciplinary approach.

[2] As clarified in the introduction, in this work we do not address the relations between OIE and NIE but concentrate our attention on the factors underlying the evolution of the OIE.

In extreme synthesis the leading ideas of the OIE are the following: **(i)** the belief in the complex and interactive character of 'human nature', and the consequent importance of the social and institutional framework for its amelioration; **(ii)** the refusal of any abstract and deductive theorising detached from the observation of reality, and the consequent emphasis on inductive methodology based on case studies and statistical analysis; **(iii)** the importance attributed to the notion of 'social control' – meaning the proactive role of institutions and policies in addressing economic and social problems; **(iv)** an interdisciplinary orientation – in particular with the philosophy and psychology of pragmatism and other related contributions of social psychology – in order to acquire a more realistic account of the characteristics of human nature in its individual and social unfolding.

These ideas had their origin in important universities – in particular, Amherst, Chicago, Columbia, Wisconsin, which were associated with various strands of OIE – which became the springboard of collaborations with numerous research institutions and governmental bodies.

The general sentiment pervading these initiatives was one of optimism about the possibilities of social progress. Such orientation was by no means confined to institutional economists as it involved the philosophy and psychology of pragmatism, and various strands of psychology, sociology and political science.

In this context, OIE played a relevant role in its early period, and it can safely be said that it came to be, although perhaps only by a slight margin, the 'mainstream economics' of the time. This period ran approximately from Veblen's first important contribution in 1898 – the article 'Why Is Economics Not an Evolutionary Science?' – to the implementation of the *New Deal* in the early 1930s, where many institutionalists played a significant role.

So after such a popular period for OIE, the question poses itself, what are the main causes of its subsequent decline? As noted by a number of contributions (see, for instance, Hodgson, 2004; Myrdal, 1972; Rutherford, 2011), one relevant factor that triggered the decline of institutional economics was the eruption of the Great Crisis of 1929. But how relevant was this? The answer is two-fold: **(i)** institutionalists were unable to forecast the eruption of the crisis; **(ii)** the proposed remedies for the crisis were (or, at least, seemed to be) not as path-breaking as those advocated by Keynesian exponents. As regards the first point, it is true that in the period before the crisis, economists (neoclassical, institutionalist, and otherwise) were neither able to predict the crisis nor, shortly after its onset, to fully grasp its structural and far-reaching dimension. As regards the second point, the picture is more complex. In fact, many institutional economists provided significant suggestions[3] for overcoming the economic crisis. These suggestions centred on the role of market power and sticky prices in creating a high margin of profits and an unfavourable income distribution for working classes which, in turn, led to their insufficient capacity to consume. This situation was not counteracted by new investments, as a large part of the profits was saved or invested in financial activities.

In policy action – and in particular in the framing of the *New Deal* – institutional economists[4] played an active role. Their proposals centred on realising some forms of economic planning, with a view to reduce mark ups and so obtain prices more oriented to costs. In this way, a more equitable distribution of income could ensue, which would steer a parallel increase in citizens' capacity to consume. In addition to policy changes, a number of institutionalists also proposed a programme of public works, but the widespread feeling was

[3] See in particular Rutherford (2011).
[4] See in particular, Berle and Means (1932); Levin, Moulton and Warburton (1934); Mills (1936); Moulton (1935, 1943); Nourse (1944); Nourse and others (1934); Tugwell (1924); Tugwell and Hill (1934).

that a high level of a public budget deficit and public spending would 'crowd out' private initiative in the middle to long run.

This was partly true, but, as the arguments employed by institutionalists partly resonated with neoclassical theory, they conveyed the impression, no matter how well founded, that their policy proposals were not sufficiently innovative to really lift the economy out of crisis. This impression was reinforced, as we will see presently, by a rather sceptical attitude of many institutionalists towards Keynes's theory.

For this reason, since the kind of economic planning advocated by institutionalists was applied only to a limited extent and that a significant part of the *New Deal* was focused on public spending – and as the policy of public (and deficit) spending was considered an eminently Keynesian innovation – the main merit of this programme was attributed to Keynesian theory.

Another related aspect that contributed to OIE's decline was the parallel strength, not only of Keynesian economics, but also of more orthodox fields of economics. As we know, in the late 1930s and even more so in the post-WWII period, there came about a massive development in the field of 'micro-foundations' of economic action. This was also realised through the development of the 'New Institutional Economics' (NIE) which, while recognising the importance of institutions, tended to interpret their functioning through the lens of the rational agent model.

These more 'orthodox' contributions formed a kind of 'alliance'[5] in the post-WWII period between economic models based on an extensive use of mathematics and econometric techniques trying to 'validate' the underlying hypotheses. This is not the place for detailed assessment of these models except to say that while often focusing on important aspects of economic action, they are trapped in the typical shortcomings of a positivist methodology, namely reductionism and simplification. The approach of the OIE, however, allows for a more thorough analysis of concepts like market imperfections, agency and expectations that, although more developed in the 'mainstream' domain, have strong institutional foundations. So, as we will try to show, the crisis of institutional economics can be traced back to these mathematical or 'scientific' approaches which set an over-arching agenda for economics.

The Main Interpretations of the Institutionalism Decline

According to Geoffrey Hodgson (2004), the weak aspects of institutionalism lie in the different opinions of its exponents on many issues:

(a) The prevalence in the post-WWII period of the Ayres' tradition had a negative effect on the development of OIE. This came about for two reasons: **(i)** the emphasis placed by Ayres ─ who followed a narrow interpretation of Veblen's analysis ─ on the 'always progressive role' of technology and the 'always negative character' of institutions appraised only as a 'ceremonially-based' obstacle to economic and technical progress; **(ii)** the abandonment, in the Ayres' tradition of the OIE, of the analysis of prices and of microeconomic relations in favour of a faith in progress based on a kind of technocratic determinism. As a result, OIE lost interpretative power of many relevant phenomena.

(b) The lack of agreement on the 'fundamentals' of institutional economics: these are, in Hodgson's words, 'the necessity of "metaphysical presuppositions" for theory, the principle of determinacy, the degree of emphasis on human agency or volition, the degree

[5] For more details on this process refer to Yonay (1998)

of application of Darwinian principles to economics, the recognition of the enabling as well as the constraining possibilities of institutions, the degree of acceptance of Jamesian instinct-habit psychology, and the degree of accommodation to behaviourist psychology' (Hodgson, 2004, p. 393).

(c) These problematic aspects were reinforced by the abandonment of a 'truly Darwinian programme' from Veblen and even more by his followers who, in Hodgson's view, adopted only a mild version of evolutionism.

Malcolm Rutherford (2011), in his reconstruction of the institutionalist movement in America during the period 1918-1947, provides a different account for the OIE's decline. He argued that OIE did not fulfil its intention to provide a strong psychological foundation to its theoretical framework. There were attempts made, but they were rather fragmented and rarely went beyond the stage of acute intuitions. This situation was reinforced by a similar situation in psychology (see also later).

Another reason for OIE's decline was that the theory of business cycles, despite its relevant developments, remained in a rather confused state at theoretical and policy level.

In fact, the comprehensive work of Wesley Mitchell on business cycles, while contributing with factual data to detect their complexity and unpredictability, did not provide a clear theoretical explanation for their evolution. This opened the door to the massive attack on institutionalism as being 'a narrative without a theory'. In Rutherford's words,

> 'Perhaps the most important displacement of all [of institutionalism] was that produced by the arrival of positivist ideas of science. These ideas allowed Keynesian and neoclassical economists to successfully adopt the mantle of scientific method while characterizing institutionalism as naïve empiricism… Under these circumstances, institutionalism could maintain little of the appeal that it had in the early 1920s… The rhetoric of science had been taken over by Keynesian and neoclassical economics supported by econometric methods, and the ideas of social control had been adapted and rebranded by those associated with Keynesian policy and the welfare state. Indeed, the appeal of Keynesian economics was, at base, exactly the same appeal to science and social control that institutionalism had held out previously, and generated the same enthusiasm and success' (Rutherford, 2011, p. 353).

An aspect of this shift was the growing formalisation of economics which, according to Morgan and Rutherford (1998), was chiefly to be ascribed to the rise of McCarthysm in the early post-WWII period. In their words, 'The cold war enforced, if it did not create, the trend toward economists offering professionally neutral, objective expertise, which contrasted strongly with the ethical, and strongly held, advocacy of the late-nineteenth-century professional economist' (Morgan and Rutherford, 1998, p. 16).

While agreeing with most of the aspects underscored by the previous studies, we also think that there are a few less convincing aspects. As for Hodgson, the role attributed by him to Darwinism in economics seems a bit one-sided. True, some Darwinian concepts can help understand the characteristics of socio-economic evolution, but it also seems true that our behaviour cannot be reduced to only a biological metaphor. In fact, human behaviour is much more open than that of animals to the manifold influence of cultural conditions. For instance, it is easily observable nowadays in western countries a relative decline of jazz music from its golden time, with a parallel rise of various versions of pop music. True, the

application of Darwinian concepts of struggle for survival, replication, selection and evolution can help understand the dynamics of this phenomenon. However, we should not forget that we are dealing only with a metaphor, for the simple reason that, in the example, the evolution of musical tastes has little to do with any 'objective necessity' related to the imperatives of 'natural selection'. Conversely, such evolution constitutes an utterly cultural phenomenon which can find different expressions in various contexts. The same applies, of course, to many other economic and social issues.

Also technological progress, for instance, does not show a deterministic pattern but is heavily 'embedded' in the economic and social structure. An interdisciplinary approach, however, can help attain a more far-reaching understanding of these phenomena.

With respect to Hodgson's stress on the negative role of the abandonment of the theories of pricing, we can note that a tradition in this respect existed in institutionalism. A tradition that began with the seminal contributions of J.R. Commons and W.H. Hamilton, who elaborated central concepts for the theory and policy of competition, industrial relations and public utilities regulation. They detailed, for example, the legal foundations of transactions, markets and competition, the notion of reasonable value and due process of law, and the complex character of policy action.

Therefore, even if we agree that the Veblen-Ayres' tradition can have, in some way, weakened the focus on microeconomics, we believe that the critical factor for the crisis of institutionalism rests, as also underlined by Hodgson, on an insufficient clarification of the central aspects of method and theory. And that such weakness left OIE relatively defenceless against the increasing adoption in the profession of a positivist stance, which found expression in the widespread employment of a maths and econometrics.

As for Rutherford's analysis, we think that, in dealing with Keynes's approach, it mainly rests on a quite simplified account of his theory as a mere advocacy of deficit spending. We believe that a distinction needs to be made between three aspects: **(i)** the complexity of Keynes's macroeconomic theory; **(ii)** the subsequent neo-Keynesian developments most often including neoclassical elements; and **(iii)** the simplified account of Keynesian theory in public debate as a mere advocacy of deficit spending. Regarding point (iii), as we will see presently, Keynes remarked that large deficits cannot be considered a permanent solution for economic imbalances.

We also agree with Morgan and Rutherford's account of the role of McCarthyism in pushing forwards a growing formalisation in economics. However, we think that other explanations are also required for casting light on the rise of formalism in economics and the parallel decline of institutionalism in the post-WWII period.

This is because this formalistic trend is by no means confined to the McCarthysm phenomenon. In fact, **(i)** this trend continued in the USA even throughout the more progressive decades of the 1960s and 1970s, and it is still apparent now; **(ii)** it extended well beyond the USA to become a worldwide phenomenon; **(iii)** it constituted a typical aspect not only of the orthodox domain, but also of various fields of heterodox economics (for instance, various streams of radical Keynesianism and Marxism).

Hence, the great importance ascribed to formalism can be regarded as an aspect of the general affirmation, as also stressed by Rutherford (2011), of a positivist trend in social sciences. It would seem that what lost ground in the post-WWII period was a *humanistic* perspective in economics and other social sciences, and this constituted a major factor in the decline of the OIE's perspective.

We think that one explanation for this loss of humanistic perspective can be that in this period there was a (more or less conscious) belief that the advances achieved in natural and technical domains could be automatically transposed to social sciences. This factor can

also explain why the positivist drift was also strong in the early 20th century (see also later) and is still present today.

The issue remains, however, as to the 'endogenous reasons' that rendered the institutionalist reaction to this shift ineffective. We will consider three factors which can help explain the OIE's decline: **(i)** the relations of institutional economics with Keynes's macroeconomic theory; **(ii)** the links between theoretical and empirical analysis and the supposed lack of a clear theory; and **(iii)** the interdisciplinary orientation.

2. Original Institutional Economics and Keynes's Macroeconomic Theory

In general, although with a number of exceptions, institutional economics has never had much enthusiasm[6] for Keynes's macroeconomic theory (in particular, 1931 and 1936). In fact, the key message conveyed in Keynes's theory (simplifying greatly) was that the best way to push the economy was through public spending and deficit spending. And, furthermore, that large deficit could be maintained over time without much damage to the economic system.

In relation to this simplified interpretation, many institutional economists were rather critical and skeptical of Keynesian theories. Most of them remain unconvinced of Keynes's macroeconomic approach which, in their views, did not consider the variety of microeconomic aspects and, in particular, they underscored the danger of a policy of deficit spending[7] on the inflation rate and on the crowding out of the private sector.

This, of course, does not mean that the OIE paid little attention to macroeconomic issues. In fact, it is worth stressing that many economists belonging to institutionalism (or to fields close to it) provided relevant empirical and theoretical contributions to macroeconomic imbalances. We can mention: **(i)** the contributions to the issue of business cycles provided by Veblen and Mitchell, and the analysis of the relevance of macroeconomic stability expounded by John R. Commons; **(ii)** the important but rather neglected field of underconsumption; **(iii)** the macroeconomic approach of, among others, Alvin Hansen and M. Ezekiel, which have various parallels with Keynes's theory.

However, notwithstanding this progress, it seems safe to say that the dialogue between institutional and Keynesian economists has not been very effective from either side. The weak aspect of the institutionalist attitude does not lie in pointing out the limitations of Keynes's theory, but in not fully grasping, on the one hand, the inadequacy of mainstream 'macroeconomics' based on 'Say's law' and, on the other, the challenge posed by the Keynesian approach that, although flawed by some weak aspects[8], goes well beyond a simple advocacy of deficit spending. In fact, Keynes's theory did nothing less than to build, virtually from scratch, the modern macroeconomic theory. As a matter of fact, before that time, no real macroeconomics existed at all. As is known, both classic and neoclassic economics strictly adhered to the so-called 'Say's law', according to which aggregate supply 'automatically creates' its own demand. If economic systems worked like this, no macroeconomics would be needed at all, since the sum of the individual behaviour (in particular, consumers and firms) would explain the aggregate outcome.[9]

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[6] See also Rutherford, 2011, ch.10.

[7] In this regard, Keynes remarked that large deficits cannot be considered a permanent solution for economic imbalances. In his view, what was needed for a structural solution of economic imbalances is a combination of macroeconomic and structural policies (see also later) able to reduce the tendency of economic systems to get easily trapped in under-employment equilibria.

[8] We have addressed in more detail the main aspect of Keynes's macroeconomic approach, also in relation with the theories of underconsumption, in Hermann (2017).

[9] It can be interesting to note that it has been the adherence to such 'law' that has permitted 'the logical shift' from classical to neoclassical economics. In fact, classic economics, although relying on the

In this world, optimisation and economic progress would proceed in tandem, provided that the market was let to work free from interferences from the public sector.

This picture was completely reversed by Keynes's theory (and also by theories close to institutionalism, such as those of underconsumption[10]). While his theory assumes a reasonable 'perfection' – or, at least, no major imperfections – of markets at the microeconomic level, it also explains how the macroeconomic outcome can easily be at variance with an optimal allocation of resources. This is due to the structural tendency of aggregate demand to lag behind aggregate supply.

The main causes of this phenomenon are **(a)** a relatively low level of propensity to consume, which can be traced back to wide differences in incomes, since the propensity to consume for higher incomes is likely to be less; and **(b)** the effects of technological progress which, by tending to make many jobs redundant, require an increasing aggregate supply in order to secure a full employment level.

This is, however, not the end of the story, as at the least three other factors should be added for 'closing' such macroeconomic system: **(i)** the tendency of nominal wages to lag behind the inflation rate, with a consequent diminution of real wages;[11] **(ii)** the dynamics of real interest rates, their dependence upon monetary policies and their (negative) effects on the expected profits of firms (or marginal efficiency of capital, MEC); **(iii)** the role of 'animal spirits', namely, the tendency of persons to embark on economic initiatives not so much on account of the prospective returns, but owing to an instinctive proclivity to action.

These aspects mainly pertain to the short-term, so we should also add Keynes's analysis of the long-term perspectives in economy and society, which was developed in particular in the *Essays in Persuasion*. Here he explained how focusing attention on short-term problems constitutes only a part of a more profound awareness of the structural transformations of society. The full unfolding of these tendencies can open up new avenues of progress, in which the 'economic motive' associated with the more negative traits of capitalism – selfishness, greediness, avarice – can gradually become unimportant and be replaced by social and cooperative relations.

Keynes was also fully aware – by making explicit reference to Commons's taxonomy – of the transformation of individual capitalism into a 'concerted capitalism', in which the role of public action, also in the form of semi-autonomous agencies, would play a pivotal role.

Turning to our theme, this forward-looking and articulated theory ─ which, of course, needed to be developed in various respects ─ was partly overlooked by institutional economists, as they tended to consider a simplified version of it. Relatedly, the same holds true for Keynesian economists, who paid little attention to institutional theories.

The result of this gulf between the two groups caused a delay in better clarifying fundamental aspects of the economic systems that would have benefited from a more systematic collaboration between these theories. We can mention, in particular, the following intertwined aspects: **(i)** the role of legal and institutional frameworks in promoting a balanced economic and social development; **(ii)** the role of public spending and credit creation in the

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hypothesis of perfect markets, is still constructed through the identification of neat social classes (in particular, workers and capitalists), whereas in neoclassical economics there exist only economic agents.

[10] These economists (in particular, J.A. Hobson, A.F. Mummery, W. Catchings and W.F. Foster) stressed, in different ways, that one source of economic stagnation is the insufficient capacity to consume of the working classes, which is accompanied by an excess of saving by wealthy individuals and by corporations.

[11] This diminution performs, in Keynes's analysis, a complex effect: in fact, if, on the one hand, a reduction in the cost of labour can incentivise investment, on the other hand, the reduction of real wages can reduce effective demand, also because the propensity to consume is likely to be higher with lower incomes.

formation of effective demand; **(iii)** the links between macroeconomic and structural policies; **(iv)** the nature of expectations; **(v)** the manifold expression of market imperfections and their relations with social structure.

3. Relations between Theoretical and Empirical Analysis

One motive of the ascendance of institutional economics lies in its claim to be more concerned with investigation into the facts and data of the real world. This world, and in particular the economic domain, was becoming more and more complex and was characterised, along with the emergence of the modern corporation, by a growing importance given to market imperfections. Neoclassical economics, with its abstract[12] and deductive theorising, was considered unfit to adequately address these new phenomena. Hence, a novel approach was needed, and institutional economics seemed ready to take up the challenge.

As noted above, institutional economists became deeply involved in many relevant issues, such as labour legislation, structure of costs and prices, business cycles, antitrust policies, public utilities regulation, public works and other areas of public intervention. As noted by Rutherford,

> 'All of this [activity] seems to indicate the strength of the institutionalist movement. Well established at leading universities and research institutes, with excellent access to external funding sources, involved with important government legislation and programs, and linked to recent developments in related disciplines. In all of these respects, institutionalism had as much or more strength than neoclassical economics…. Nevertheless, when Wisconsin and Columbia resumed hiring in 1946-1947, it was not institutionalists who were hired, but Keynesians and neoclassical economists, indicating that some very significant shifts in the academic environment must have taken place between the 1930s and 1946-1947 when hiring resumed' (Rutherford, 2011, p. 350).

Thus, the question poses itself once more: why did this decline occur in spite of the highly relevant orientation of institutional economics? One reason, as we have just seen, was constituted by the affirmation of Keynesian economics. But this was by no means the sole cause. In fact, another and related reason for such decline rests in the unclear and often contradictory way in which institutional economics addressed the central issue of empirical analysis.

In order to better develop this issue, let's have a closer look at the methodological underpinnings of empirical work carried out by institutional economists. This work went along three main avenues: **(i)** statistical analysis of the main economic categories (consumption, investments, profits, prices) at various levels of disaggregation, in order to enquire into the dynamics of business cycles and the characteristics of industrial sectors; **(ii)** analysis of the legislation and court decisions, with particular reference to the issues of industrial and competition policies, and of public utilities regulation; **(iii)** case studies related to particular firms, industrial sectors and other economic realities.

[12] As noted before, in this period neoclassical economics had not yet established a systematic collaboration with econometrics.

These activities were flourishing and produced significant results, but something stood in the way to hinder their full unfolding. As noted before, this was due to the massive development, in the post-WWII period, of mathematical models and econometric analysis on the part of both neoclassical and various strands of the new Keynesian economics.

Of course, mathematical models and econometric models are quite different things – a mathematical model might not be amenable to econometric analysis and we can perform econometric estimates without a clear model, mathematic or otherwise, underlying them – but in the widespread opinion both were considered as a step towards a more 'scientific and objective way' of investigating the economic phenomena. In fact, mathematical models, whether or not allowing econometric estimates, were constructed as 'a piece of theory' amenable, actually or at least potentially, to empirical testing. The philosophical basis of this development was positivism, which formed from a narrow conception of behaviourism.

The institutionalists' reaction to these events was largely ineffectual, because they partly shared (at a higher or lesser degree) a kind of positivist[13] attitude. For this reason, notwithstanding their relevant contributions on all the three headings (i) to (iii), they only considered statistical analysis to be truly 'empirical and scientific' (the heading (i) above).

In this regard, their philosophical background oscillated between pragmatism and positivism, and was never sufficiently clarified. In fact, they adopted John Dewey's notion of behaviourism, but this was often intermingled with a positivist notion of behaviourism. However, these notions are very different and should not be confused, as they relate to the following aspects:

(A) The pragmatist conception of behaviourism ─ especially in the perspectives of John Dewey and William James ─ refers to the importance of analysing the 'experience' of a person in its entirety. Hence, we should consider not only the more directly observable and 'measurable' behaviour, but also the whole set of feelings and orientations in their individual and collective dimension.

(B) In the positivistic conception, only directly observable behaviour is considered 'scientific', because, it is claimed, only this kind of behaviour can be 'measured' in a more neutral and objective way.

These notions carry very different implications for social analysis. We can see this with a simple example: let us suppose we are investigating child behaviour at school. Following a positivist orientation, the researchers will try to find a set of factors which identify the 'normal' or 'optimal' behaviour at the school – for instance, the rate of attendance and the level or proficiency – and then they will proceed to estimate, by a variety of statistical and econometric techniques, the degree of fulfilment of these objectives.

Conversely, pragmatist oriented researchers would probably carry out the same kind of analysis, but would not stop there. Their results would not be the end of the story, but would constitute only the basis for further investigation into the individual and social factors that lead to a certain behaviour.

In fact, if we are studying children's behaviour, we should not forget that we are dealing with persons living in a social context. Hence, in order to get a more complete assessment of the 'normality' of their behaviour, we should ideally get a profile for each child's

[13] Of course, this did not apply only to OIE but was a relevant aspect of social and psychological sciences in the early 20th century. One explanation might be that there was a (more or less conscious) widespread idea that the advances achieved in natural and technical domains could be automatically applied to social sciences. As already noted, this can also explain ─ along with other more 'endogenous' factors ─ the tendency towards a growing formalism in economics in the post-WWII period.

personal life, with the related emotions and conflicts. This study would also involve the main characteristics of the child's family and social relations, and of the social and institutional framework surrounding each child. For instance, social classes and groups and the organisation of education, with all their values and conflicts.

Needless to say, we are aware of the difficulty of such analysis and of the expediency of identifying single parts in a complex phenomenon. Within this ambit, it can be useful to look for correlations between aggregate phenomena, namely, between phenomena involving a collective dimension.

However, we should be aware that every generalisation involves a simplification, in the sense that many other factors are left out. This applies especially when we try to establish a causality between factors in order 'to demonstrate' the validity of a theory.

In this regard, it is pertinent to remark that aggregate analysis, however important, cannot become a substitute for a more comprehensive study of each person (and situation) considered. For this reason, as we will see, a plurality of methodologies is needed in order to carry out a comprehensive empirical analysis.

In facing these issues, institutionalists did not clearly identify and confront the various meanings of empirical analysis. They swung between a (relatively) uncritical endorsement of positivist methodology and an advocacy of the importance of a far reaching approach whose application, however, remained largely below its potential.

If we consider that this came about when neoclassical and Keynesian economics – and, later on, other fields significantly related to neoclassical economics such as public choice, new institutional economics and new regulatory economics – were investigating a number of relevant phenomena (in particular, macroeconomic imbalances, market imperfections, the role of public action and of interest groups) the reasons for the crisis for institutionalism appear clearly.

A good strategy for institutionalists to cope with this challenge would have been for them to chart an open and thorough confrontation on the various theories dealing with these issues. For instance, discussing the hypothesis of rational economic behaviour, institutional economists can point out that economic incentives can be important in some circumstances but cannot be reduced to the maximisation behaviour implied in the neoclassical conception of *homo oeconomicus*.

In fact, as underscored by various groups, a host of psychological and social factors are likely to enter the picture in the motivation of economic action. For this reason, the quest for money can indicate not only a desire for money as such, but also a need to gain social approval by following a socially accepted behaviour.

In order to thoroughly address these aspects, the empirical analysis would require a plurality of methodologies. In this sense, statistical and econometric estimates should be coupled with case studies, historical analysis of larger contexts, focus groups on particular problems, in addition to obtaining a more active involvement of the actors implied.

One relevant consequence of this broadened enquiry is that it would lead to a more pluralistic interpretation of the examined phenomena. In fact, considering these phenomena in their real complexity would make it easier to engage in a comprehensive confrontation of different explicative theories.

A possible drawback of this methodology – pointed out in particular by mainstream economists – is that, by putting too many factors in the basket, it would engender uncertainty and confusion. This is true in a degree but, at the same time, it is largely overstated. In fact, complexity does exist, and trying to disregard significant factors in order to simplify the picture would run a double risk. Not only of omitting a number of factors, but also of not making clear the underlying criteria and values of the researcher.

In this regard, what is needed for clarifying the criteria and values employed in the analysis is a thorough process of social valuing. A central aspect of this more comprehensive analysis relates to the interdisciplinary orientation of institutional economics.

4. 'Data without Theories' versus 'Metaphysics Driven Theories'

The title of this paragraph refers to the harsh polemic that arose[14] between neoclassical and institutional economists over the theoretical foundations of the discipline. This debate centred around Mitchell's Presidential Address[15] before the American Economic Association in 1924. Let us now briefly address the main contents of this controversy.

The View of Wesley Clair Mitchell

In his address he started by stressing the complementarity existing between the 'qualitative analysis' ─ which he relates[16] in particular to the work of neoclassical economists regarding the 'utility function' of an individual ─ and the quantitative analysis of more objective factors, like prices and quantities in the market.

However, in Mitchell's opinion, as qualitative analysis cannot directly demonstrate whether individuals really maximise their utility, it becomes more expedient to employ and develop the techniques of quantitative analysis. In this way, by obtaining more information on a set of aggregate economic phenomena ─ for instance between prices and quantities ─ some interesting 'inferences' can be made about the behaviour of economic agents considered as a group. In his words,

> 'It seems unlikely that quantitative workers will retain a keen interest in imaginary individuals coming to imaginary markets with ready-made scales of bid and offer prices. Their theories will probably be theories about the relationships of variables that measure objective processes… the "psychological" element in the work of these men will consist mainly of objective analysis of the economic behaviour of groups. Motives will not be disregarded, but they will be treated as problems requiring study, instead of being taken for granted as constituted explanations' (Mitchell, 1925, pp. 26-27, quoted from W. Mitchell *The Backward Art of Spending Money and Other Essays*, New York, Kelley, 1950).

We can note that Mitchell's position, however innovative in many respects, is weakened by a kind of positivist stance according to which only 'measurable' phenomena are amenable to

[14] For more detail on these aspects refer to Rutherford (2011) and Yonay (1998).
[15] This took the form of an article 'Quantitative Analysis in Economic Theory', Presidential Address delivered at the 37th Annual Meeting of the American Economic Association, held in Chicago, December 1924. It was then published in *American Economic Review*, vol XV, pp.1-12, March 1925. This was followed by a rejoinder 'The Present Status and Prospect of Quantitative Economics', Round Table discussion at the American Economic Association meeting, December 1927, reprinted in *American Economic Review*, vol. XVIII, supplement, pp.39-41, March 1928.
[16] Actually, neoclassical economics is based on 'pre-defined postulates' that render difficult a scientific enquiry into the whole set of measurable and non-measurable aspects that combine to explain the links between individual and collective behavior. In this regard, the institutionalist perspective seems much more fitted to address these issues. This is because OIE ─ and the same can be said for related disciplines like the pragmatism of John Dewey and William James, humanistic and social psychology, and psychoanalysis ─ arrives at certain (provisional) conclusions on, say, the types and degree of 'rationality' of economic agents, on the basis of a continual analysis of real phenomena.

scientific verification. This can be seen in the following passage where, after stressing that 'motives will not be disregarded', he adds,

> 'Psychologists are rapidly moving toward an objective conception and a quantitative treatment of their problems. Their emphasis upon stimulus and response sequences, upon conditioned reflexes; their eager efforts to develop performance tests, their attempt to build up a technique of experiment, favor the spread of the conception that all of the social sciences have a common aim ─ the understanding of human behaviour; a common method ─ the quantitative analysis of behaviour records, and a common aspiration ─ to devise ways of experimenting upon behaviour '(Mitchell, 1925, quoted: p. 27).

The Neoclassical Rejoinder

The neoclassical answer was that theories should be constructed independently of real data, in particular those having a supposed 'narrative character'. In this sense, theory should guide the empirical analysis, and not the other way round.

Building on this, neoclassic exponents sharply attacked institutionalism as a discipline without a theory, mainly based on data gathering. This can be true to a degree ─ owing to the above mentioned limitations ─ but it is also true that neoclassical position is quite weak. In order to see the shortcomings of neoclassical methodology, let us quote some passages from the Norwegian economist, Ragnar Frisch,

> 'Let us imagine a scientist who is watching the shifting aspect of the surface of water. An empirical description of the ups and downs of the surface of the water would not lead anywhere, however minute the description was. In order to gain a real understanding of the phenomenon, our scientist would have to introduce at least three different sets of *ideas*: first, the idea of direct action of wind on the surface of the water. This would account for small waves. Next, the idea of propagation of long swells coming from the ocean. And third, the idea of ebb and flow caused by the attraction of the moon. Without introducing a model world containing these three kinds of waves, he would be hopelessly lost in his attempting at understanding the phenomena… Or… Let us imagine that somebody wanted to explain the movement of the moon around the earth, and in order to do so, obtained the co-operation of a number of observatories on the earth. The observations obtained in this way would be interesting enough in themselves, but they would not contain any significant contribution to the problem at hand: the explanation of the orbit of the moon. The man who indicated the road to a real explanation of the phenomenon did it without any telescopes. His tools were just a pencil and a sheet of paper, and is name was *Isaac Newton*. In his imaginative mind he constructed a model world where bodies attracted each other with a force proportional to the masses of the bodies and inversely proportional to the square over their distance….The real discovery was brought about by a brain, not by a staff of patient observers. It seems to me that much of the work which has been done in economics in the recent years in its significance is comparable to minute observations of the surface of the mon in order to find out its orbit' (Frisch, 2011 [1930]).

Further Remarks

What can we say about such controversy?

We agree on the need to provide theoretical foundations to the phenomena under examination, but believe R. Frisch's remarks are rather ungrounded. It is untrue that, in the examples, these 'laws' have been construed by an abstract deduction based on an abstract reasoning. Quite the contrary, these laws have been inferred inductively from a careful observation of reality. Accordingly, we can note that Newton himself derived the law of gravitation from induction, namely, from observing natural phenomena. And that, from Newton's time onwards, such law and its underlying theory have been (and are being) more and more refined and clarified as a result of a better knowledge of physical world. To that purpose, we need more powerful telescopes to see better the characteristics of outer space but we also need more powerful microscopes in order to enquire better into the physical characteristics of the Earth and other planets.

Therefore, the supposed similarity of the neoclassical method with the one typical of natural sciences is misplaced. In natural sciences, the scientific laws do not 'reduce and simplify' the complexity of the world, but add some elements to its explanation. This comes about because in natural sciences the degree of synthesis and abstraction required to the formulation of every scientific 'law' is checked by a continuous interaction between theory and observation.

Conversely, in our view, neoclassical economics' methodology seems more similar to metaphysics than to scientific enquiry. As we know, this theory is based on two basic and grand postulates ─ the '(instrumental) rationality' of economic behaviour and the optimising properties of the market ─ which have the character of a wishful thinking and are not open to any real 'confirmation'.

In fact, even when empirical analysis is found to be useful for studying the performance of these 'laws' in real situations, the results obtained can never change or refine such postulates, just because they have the nature of metaphysical entities. For instance, if an empirical analysis indicates that individuals behave rationally according to some proxy (for instance, if consumers choose the item at its lowest price) the theory is 'confirmed'. If, however, empirical observations point out the presence of 'irrational behaviour' (when, for instance, consumers systematically do not choose the lowest price), this does not impinge on the prime postulates, but tends to be 'rationalised away' by treating these results as exceptions or due perhaps to some unwelcome 'exogenous' factor.

It is plain that this methodology can open the door to a process of simplification and misinterpretation of economic and social reality. In this regard, it is interesting to note that the positivist methodology relying only on 'quantitative phenomena' opens the door to the supremacy of the basic tenets of neoclassical theory.

In fact, as quantitative enquiry alone cannot reach the "soul and the heart" of the phenomena (see also the next paragraph), the implicit philosophical and psychological foundations underpinning the 'basic principles of a theory' can never be really questioned.

In some way, a similar story took place in psychology, where the progressive affirmation of a narrow conception of behaviourism was not effectively questioned by more humanistic fields of psychology.

The Various Dimensions of Empirical Analysis

On these issues, institutionalists' reaction was not very effective. We can see this, for instance, in Mitchell's answer (1928) to the criticisms levelled at his previous position on the importance of quantitative analysis. In such rejoinder, true, he clarified the possible limitations of qualitative analysis, which stem from the circumstance that economic and social phenomena can never be investigated with the precision of a laboratory experiment.

However (see also the next section) his view that the qualitative phenomena can properly be investigated only through identifying some measurable proxy weakens and confuses his (correct) stress on the importance of empirical analysis.

Therefore, a fairly obvious rejoinder would have been that every theory should find confirmation in the empirical evidence writ large. This would include the whole set of observable phenomena, both measurable and not measurable. We can identify three levels of analysis:

(A) Many socio-economic phenomena have, along with some measurable dimensions, a set of qualitative aspects that require a qualitative interpretation (which involves a process of social valuation also related to the interpretation of quantitative data). For instance, in the analysis of a particular market, we certainly need quantitative data on supply, demand and prices. But, in order to get a comprehensive understanding of the phenomenon, we also need a wide range of qualitative information on the characteristics of the institutional, social, cultural and psychological features that combine to define such market structure.

(B) There are various phenomena for which there are no measurable dimensions but can be nonetheless scientifically addressed. For instance, in a music school how can we 'assess' the proficiency of students without resorting to measurable proxies? Or, how can we 'demonstrate' that, for instance, we love our friends, or that John is more friendly than Patrick? In this regard we think that, although in these matters there is no direct demonstration as in the case of, say, identifying the fastest runners, a more 'qualitative-oriented' demonstration is possible. For instance, arts criticism and schools music have elaborated many criteria for assessing artistic creations and musical proficiency, and humanistic psychology has devised many criteria for understanding the qualitative aspects of feelings (which, as noted before, have nothing to do with the *a priori* identification of 'laws' typical of neoclassical economics). Needless to say, these assessments will always be more tentative and open to question than, say, the speed of runners. However, this does not imply that these findings are 'less scientific', but only that the issues addressed are more complex.

(C) As a way of synthesis, how can we assess whether (and in what degree) a collective and evolutionary context (with its culture, institutions, norms, organisations and policy action) is conducive to economic and social progress? Of course, we can identify quantitative proxies for many phenomena, but this does not eliminate the necessity ─ in order to avoid the danger of simplification and reductionism ─ of evaluating the qualitative and specific aspects of the phenomena considered. Such appraisals would involve a process of social valuation, which is a distinctive notion[17] of OIE. Such concepts lie at the heart of policy action and are also likely to influence the interpretation of quantitative data.

[17] For a good analysis of such concepts see, for instance, Tool (1986).

In concluding this section, and also as a way to introduce the next, we can so synthesise the relevance for OIE of clearly adopting a more comprehensive conception of empirical analysis:

(I) A distinctive trait of the OIE is a holistic conception of the persons in their individual and collective expressions.

(II) This requires an interdisciplinary approach in order to better analyse the multifarious ties between persons and their economic, social and cultural contexts.

(III) For this reason, the OIE approach strongly demands a conception of 'scientific evidence' that ─ by going beyond the narrow positivistic claim that only 'measurable phenomena' can have 'scientific validation' ─ would fully consider the whole set of qualitative and non-measurable phenomena.

(IV) There were, between early institutionalists, various (and evolving) opinions on the 'pragmatist' and 'positivist' conception of economics: for instance, the differences ─ along with significant common aspects ─ between J.R. Commons's and W.H. Hamilton's reliance on case-studies and legal analysis, on the one side, and W. Mitchell's emphasis on statistical analysis are well known. However, the methodological implications of these approaches were most often implicit, because they also tended to be intermingled with other issues. Hence, Mitchell's Presidential Address did not manage to steer institutionalists towards a thorough debate on these aspects.

5. The Need of an Interdisciplinary Approach

As we have seen, one distinctive aspect of institutional economics was its interdisciplinary orientation. This applies in particular to psychology, where institutionalists explicitly set out on their research agenda a close collaboration. This can be seen, for instance, in the following passage by Mitchell,

> 'As soon as an economist has assimilated this idea that he is dealing with one aspect of human behaviour, he faces his share in that problem so conspicuous in current psychology, nature and nurture, the propensities with which men are born and their modifications in experience. I do not imply that the economist must read all the literature upon instincts and repressions which the psychologists publish. Doubtless acquaintance with that literature is helpful; it suggests a wide variety of hypotheses, and it makes one critical of the naïve theories of human mind which each mind proffers in profusion' (Mitchell quoted in Tugwell, 1924, p. 23).

However, despite this far-sighted agenda, institutionalism did not fully realise its promise. True, there were, in the institutionalism heyday, several contributions that employed (and even created) psychological concepts for explaining economic behaviour. These contributions, however, despite their innovativeness, rarely went beyond the form of acute intuitions. They remained ─ with the partial exceptions of Veblen's theory of instincts and Commons's 'negotiational psychology' ─ in a rather undefined and 'liquid' state which never hardened into a more systematic theory able to constitute an alternative to neoclassical economics. There are several reasons for this outcome, some 'endogenous' and other 'exogenous'. Among the endogenous factors we can mention:

(I) In the first decades of the 20th century, both neoclassical and institutional economics were still 'young disciplines' and, for this reason, were characterised by an intense debate, within and between their fields, over their core concepts and the implications for policy action. These discussions, although more so in the newly-born institutional economics, were relevant also for neoclassical economics. One consequence of this was that the boundaries[18] between neoclassical and institutional economics were more blurred than today. Hence, on the one hand, **(a)** many neoclassical economists seemed more willing to acknowledge that their basic hypotheses ─ the rationality of economic agents and the perfection of markets ─ were most often quite unrealistic and that public intervention was needed in many cases to reduce market imperfections. And, on the other hand, **(b)** many institutional economists accepted in various degrees the principles of neoclassical economics.

(II) Manifold influences intervened between institutionalists and neoclassicists, which created a lively intellectual atmosphere. Such process was strengthened by the parallel developments in the psychology and philosophy of Pragmatism, and by various developments in sociology and social psychology. We remember, among others, the contributions of John Dewey, William James, George Herbert Mead, Charles Sanders Peirce in the sphere of Pragmatism and of Ernest W. Burgess, Charles Horton Cooley, Everett Hughes, William F. Ogburn, Carleton H. Parker, William Thomas in the realm of sociology and social psychology. As already noted, these contributions were infused with a feeling of optimism about the potentialities of public policies to foster economic and social progress, and a positive intellectual bridge was laid out with many institutional economists. In our view, this process, while providing interesting insights, also had a weakness. This can be located in a partial lack of awareness that, in order to go beyond the simplistic hypotheses of neoclassical economics ─ in particular, rational economic behaviour and perfect markets[19], with the consequent optimising equilibrium ─ and to explain the positive and negative aspects of the real world, a brand new theory of the human mind was highly needed.

(III) Another reason that could have contributed to this difficulty was the rather unclear state of the relations between theoretical and empirical analysis. As noted before, this aspect characterised not only institutionalism but also social and psychological sciences.

These aspects can also be latched to the following 'exogenous factors':

[18] Refer for more detail to the interesting reconstruction of Yonay (1998).

[19] As already noted, many neoclassical economists were aware that these hypotheses were too simple to capture the complexity of the real economic behavior. However, they tended to regard such hypotheses as a useful approximation and to consider unnecessary any interdisciplinary collaboration. For instance, in the case of rational behavior, they tend to think that, true, there are complex reasons underlying economic behaviour but it is not the business of economists to enquire into them. For the purpose of economics, it is sufficient to hold that, at least in ordinary situations, people behave in a sufficient rational way ─ or, at least, not in a persistent irrational way. However, in our view the unconvincing aspects of neoclassical hypotheses remain. In fact, as highlighted by many contributions of social and psychological sciences, while it is untrue that people behave in a persistent irrational way, it is likewise unrealistic to suppose a tendency towards an abstract and rational economic behavior. This comes about because such behaviour is heavily embedded in the evolution of social and cultural spheres, with all the related sets of values, motivations, conflicts and contradictions at individual and collective level. Hence, only a careful study of the given situation can shed light on the real social and psychological forces underlying economic action.

(IV) Psychology was characterised, in the early decades of the 20th century, by the development of various and often conflicting theories, which made it difficult for social scientists to get a clear orientation between them. Also it became difficult for social scientists to employ a number of relevant psychological concepts (for instance, cognitive limits and biases, the role of emotions, the interrelations between cognitive and emotional sphere, which only later on reached a more fully-fledged development) to the study of economic and social phenomena.

(V) At the same time, and in parallel with the relatively slow progress of other fields of psychology, there was a quick affirmation of behaviouristic psychology – in the positivistic meaning referred to above – according to which the only relevant behaviour is the one that can be observed and 'measured' through a number of proxies.

The Relevance of Qualitative Analysis

For all these reasons, the institutionalists' theory of economic behaviour was not strong enough to constitute a well-framed alternative to the narrow conception of *homo oeconomicus*. Their contributions were significant but piecemeal, and sometimes tended to shift towards a narrow conception of behaviourism. This can be seen in Mitchell's Presidential Address mentioned before. The following passage constitutes a good synthesis of Mitchell's position,

> '…. "Institutions" is merely a convenient term for the more important among the widely prevalent, highly standardized social habits. And so it seems that the behavioristic viewpoint will make economic theory more and more a study of economic institutions… The extension and improvement of statistical compilations is therefore a factor of the first consequence for the progress of economic theory. Gradually economics will become a quantitative science. It will be less concerned with puzzles about economic motives and more concerned about the objective validity of the account it gives of economic processes' (*ibidem*, p. 25, 27).

The rationale underlying Mitchell's position was, at that time, quite innovative: in fact, it rested on the purpose of getting more precise data in order to go beyond a mere theoretical speculation associated with facts. This was particularly the case for the analysis of business cycles, where he clearly recognised the complexity, the importance of context and the specificity and common aspects of the various cycles.

As also noted before, we believe that Mitchell's position on the importance of data is quite appropriate, with its limitation resting on considering as reliable data only those based on statistical aggregates.

In this context, the stress on the quantitative side of phenomena gradually became a common sentiment in this period, and was emphatically expressed by the following passages from F.C. Mills,

> 'The modern economist enumerates, measures, weighs… "When you cannot measure what are you speaking about, when you cannot express it in numbers", said Lord Kelvin, "your knowledge is of a meager and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of a *science*, whatever the

> matter may be"… In summary: Our useful knowledge of events in the world about us is essentially statistical in nature; that is, it is not concerned fundamentally with unique, individual events, but with aggregates of events which may be described in terms of averages, of typical characteristics. In generalizing about such aggregates we are of necessity precluded from speaking in terms of invariant laws' (Mills, 1924, p. 37, p. 46).

Now, of course, we acknowledge the pertinence of statistical analysis for gathering a better knowledge of economic and social phenomena. The aspect upon which we cannot agree is that the relevant data can be obtained *only* from statistical enquiry. As observed before, economic and social reality is composed of many qualitative and non-measurable phenomena which could be addressed in a scientific way.

For instance, in the above example of child behaviour at school, it is certainly useful to collect statistics on attendance and performance, as well as on the characteristics of the school system and of the family and social structure of the children. But these data are neither the only relevant ones nor the only obtainable ones.

One solution to this problem, which lies at the heart of the positivist attitude, is to broaden and refine statistical procedure by including more variables, by rendering the proxies more precise, and by devising more effective indicators.

This pathway can be useful, of course, but it is also true that statistical analysis cannot capture all the complexity of the phenomena under examination. The reason for this is simple enough, that statistical data ─ for instance, on children behaviour, consumption and investment ─ are obtained by comparing some measurable dimension of phenomena which are themselves composed of many other aspects. Hence, these phenomena (and in particular the complexity of persons in their individual and collective expression) are always something more than (and hence cannot be reduced to) the sum of their 'more measurable' parts. For this reason we can never directly measure 'the heart and soul' of the living persons.

For this reason – and in order to avoid the well-known dangers of simplification and reductionism – statistical analysis should always be coupled with case studies and other methods for acquiring more 'direct and qualitative'[20] data on the phenomena under investigation.

These two conceptions of scientific analysis carry very different perspectives on the scope of institutional economics, also in its relation with psychological sciences. In the case of positivist attitude, the only aspects deemed scientific are those amenable to quantitative expression whereas, in the case of pragmatist and humanistic perspectives, the analysis tries to consider all the relevant aspects – both 'qualitative' and 'quantitative' – which concur to identify economic and social phenomena at individual and collective level.

As noted before, the failure of institutionalists to single out the various conceptions of empirical analysis have impaired their potential for a more comprehensive investigation of economic and social phenomena. Also for this reason, developments in economics (in particular, neoclassical and neo-Keynesians) in the post-World War II period have become more and more 'quantitative' by relying almost exclusively on econometric analysis. In this way, as already noted, the validity of the basic neoclassical hypotheses of market perfection and rational economic behaviour can never be really questioned.

However, this rather gloomy picture requires a couple of notes. Firstly, despite the limitations mentioned, some interdisciplinary synergy has always occurred in the

[20] It is interesting to note that, from a different perspective, N. Georgescu-Roegen (1971) underscored that one central implication of the entropy law for economic analysis is to facilitate the analysis of the qualitative transformations of the system.

institutionalist domain. This applies especially to the philosophy and psychology of pragmatism, with the use of the concepts of habits, social norms, social identity, cognitive faculties, purposive action for the interpretation of economic and social phenomena. Secondly, two important institutional economists, Thorstein Veblen (1914) and John Rogers Commons (1934), developed an economic theory which makes explicit use of psychological concepts.

However, these insights remained largely unfledged for many years. In fact, the various contributions provided by institutionalists mostly remained at the stage of intuition and did not reach the state of a more organic theory. One reason for this was the notable fragmentation ─ despite many relevant common aspects ─ of the institutional field. In this sense, not only there was little synergy between the main fields of institutionalism; but also within each field, each contribution seemed to go its own way, as an intellectual island without much synergy with the others.

6. Conclusions: the Institutionalism's Eclipse and the New Wave of Today

As a consequence of the limitations outlined above and of more 'exogenous factors' as well, institutional economics became progressively marginalised in the profession and in the society at large in the post-WWII period.

In fact, from the more orthodox perspective, it was all too easy to dismiss such school as 'a narrative without a theory'. Perhaps even worse, institutional economics largely failed to make waves even in the field[21] of heterodox oriented theories. We have seen before its problematic relation with Keynesian theories.

In addition, the interchange with Marxism and other theories of social justice was not a smooth one. Here, however, the situation seems slightly better. In fact, contrary to Keynesian economics, institutionalism has established a more systematic collaboration with Marxism, in particular regarding the concept of power and the character and evolution of capitalistic institutions. However, despite this interchange, in Marxism and other more 'radical fields' of social sciences, institutionalism has most often not gained a great appeal – it was not even much known within the progressive field – as it was considered either a kind of utopian radicalism à la Veblen or a kind of reformism à la Commons, 'too gradualist' to fit the impatience of the revolutionary aspirations.

In this very difficult situation, institutionalists were nonetheless able to survive and produce notable contributions on a wide range of theoretical and applied issues. And, despite the insulation and fragmentation which characterised institutionalism, and the whole realm of social and psychological sciences, some useful reciprocal influence did occur in the field of economics. Indeed, it would be a mistake to infer that institutionalists' contributions – owing to their relatively marginalised position – went unnoticed among the economics profession at large. Concepts[22] like the importance of the institutions in economic and social life, the role of habits, the structure of power, the imperfection of markets, the distinction between the instrumental and ceremonial aspects of institutions, the role of social valuing, the relevance of the formal and informal rules, the characteristics of cultures, and the overall evolutionary

[21] Needless to say, this is a broad assessment that requires a much more careful analysis of specific factors: for instance, what happened in this respect in Europe and USA; and what had been the evolution of the various fields of OIE, also in relation to the parallel evolution of other strands of heterodox economics.

[22] For more details on these developments refer, among others, to Gruchy (1972), Hodgson, Samuels and Tool (1994), Tool (1988).

perspective pervading all these aspects, have exerted a discreet but enduring influence on the way of reasoning of many economists.

This situation characterised the post-WWII period until, approximately, the late 1980s. After that period, there has been a kind of new spring of institutionalism and other heterodox perspectives, which is notable and still on the ascendance. For example, can see the probable influence of, among many others, the following factors: **(i)** the crisis following the oil shocks of the 1970s made evident the insufficiency of the simplest versions of Keynesian policies; **(ii)** the growing awareness of the inadequacy of the more extreme versions of both central planning and neo-liberalism to address the imbalances of economic and social phenomena: in particular, highly uneven distribution of income and wealth, unemployment and deterioration of working conditions, environmental decay, political and social conflicts. These imbalances culminated in the recent economic crisis, which has triggered a kind of a general reshuffle of all the received economic and social theories.

This has happened also within mainstream domain. True, even in our time, neoclassically oriented theories are still the 'mainstream position' but their leading role is much more blurred and problematic than before. With regard to heterodox economics, there has been a flourish of new initiatives. New associations have been created – for instance, the Association for Heterodox Economics (AHE), the European Association for Evolutionary Political Economy (EAEPE) and the World Economics Association (WEA) – and the existing ones (in particular, AFIT, AFEE, ICAPE, URPE) have become more active and influential. They organise an annual conference and other initiatives, in particular for students. They also promote, or are involved in, the activities of a number of scientific journals. There is a growing attention to heterodox issues and there is a steady increase in the people involved in these activities.

The *spectrum* of subject-matters covered by heterodox contributions is ample and continually widening. There are also many works which apply these theories to the study of specific economic and social problems, often considered in their cultural and historical perspective. Despite this progress, the situation for heterodox economics remains troublesome. One reason is that this germination of ideas and contributions has not succeeded in securing an adequate foothold as regards financing and academic positions for heterodox economists.

This situation is particularly dangerous for the future of heterodox economics because it does not offer adequate perspectives of tenure and career for the younger generation of economists.

A detailed analysis of this side of the problem[23] is beyond the scope of the work. Perhaps, what is needed for the advancement of heterodox economics is a more systematic attention to policy issues. As a matter of fact, if we present our activities as a forum for pluralism, this looks fine, but risks to be perceived both by the more informed audience and by the lay people as an interesting intellectual venture with, however, no tangible results in terms of better policies. And this in a period where there is a high (explicit and latent) demand for new policy solutions for the major economic and social problems.

In order to attain this purpose, an adequate strengthening of the interdisciplinary potential of institutional economics seems paramount. In particular, a more systematic collaboration between institutional economics and psychological sciences can help locate the multiple levels of collective action, and in particular: **(i)** the complexity of individual motivations and systems of values, where the relational and social dimensions play a paramount role; **(ii)** the complexity of policy action, which involves not only governmental institutions but also every other level of collective action; **(iii)** consequently, the fact that the dynamics of

[23] For more details see Elsner and Lee (2008), Lee (2009), Lee et.al. (2010), Reardon (2009).

institutions and the dynamics of policies represent complementary aspects of collective action, where, in the first (the institutions) the stress is on structure, decision-making process and cultural evolution, while in the second (the policies) the focus is on action and results.

In the analysis of these problems, by clarifying the needs and conflicts arising at individual and social level, institutionalism, also in collaboration with other strands of heterodox economics, can help formulate policies more precisely based on the motivations and experiences of people involved in collective action.

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Comments on Arturo Hermann's paper, 'The Decline of the "Original Institutional Economics"'

Anne Mayhew, University of Tennessee, Knoxville, Tennessee, USA
amayhew@utk.edu

Keywords: Original Institutional Economics, behaviorism, pragmatism, positivism, W.D. Mitchell, M.A. Copeland, John Dewey, Thorstein B. Veblen

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Arturo Hermann is to be commended for his attempt to identify the main reason for the decline in disciplinary power and prestige of Institutional Economics, by which he means the Original Institutional Economics, or OIE, as I will call it in this essay. Hermann notes that there is a strong case to be made that OIE was the dominant approach among economists in the U.S. in the first three plus decades of the 20th century. Why then did OIE decline in importance in the years after WWII? Why did a deductivist, non-evolutionary theory that stands quite apart from the development of other social sciences come to dominance, while a more scientific, evolutionary and interdisciplinary approach to study of the economy was marginalised in most of the academic world?

Hermann surveys the answers that have been given to this question. He quite correctly, in my view, identifies the following as among the most powerful answers: 1) the narrowness of approach into which Clarence Ayres and his followers forced the Veblenian branch of institutionalism; 2) a lack of agreement on 'fundamentals' among OIE practitioners; 3) the success of Keynesian and neoclassical economists in their claim of being more truly scientific than the 'naïve empiricist' Institutional economists.

The change in economic practice and the shift in prestige among the different practices is a complex story, as Hermann recognises, but it is also one that he does not recount quite accurately. In this brief comment I will focus on only one of the explanations for OIE's decline that Hermann discusses: the relationship between theoretical and empirical analysis and the failure of OI economists to articulate a satisfactory (to other economists) reconciliation of the two. On this point Hermann argues that a chief failure of the OI economists was that 'their philosophical background oscillated between pragmatism and positivism, and was never sufficiently clarified'. As Hermann explains it, to the extent that OI economists tended toward 'positivism', they considered statistical evidence to be the only truly scientific aspect of their work and relegated analyses of legislation and court decisions, as well as case studies, to less than truly scientific status. Hermann then argues that 'John Dewey's notion of behaviourism,' which is, he says, 'the pragmatist conception of behaviourism, requires that the "experience" of a person in its entirety' be analysed.

The words 'behaviourism', 'pragmatism', and 'scientific' are very tricky words and especially so when we in the early 21st century try to understand the arguments that engaged our intellectual ancestors in the late-19th and early-20th centuries. There were, in effect, several different discussions going on among pioneering sociologists, philosophers, emerging anthropologists, those would become psychologists and even a few economists, and each group was fighting against previously dominant ideas among their own ancestors. The ancestral groups did not entirely overlap, so that some of the ideas against which each separate group was fighting were of little or no importance to the other groups. And some of

the words for the emerging thought that was held to be superior among all of the pioneering social scientists were the same even though with shades of different meaning depending upon the discussion in which they were used.

This confused and confusing intellectual history has, unfortunately, led Hermann astray. He is simply wrong in saying that a fundamental problem for OI economics in the 1930s and 40s was an unreconciled tension between 'behaviourism' and 'pragmatism'. When Hermann says that this was the case he is arguing that OI economists fell victim to a belief that only the behaviour that could be 'measured' through the use of statistics could be considered 'scientific' and therefore worthwhile for purposes of understanding economic processes, and is thus assuming that an intellectual fight in the discipline of Psychology had equal relevance in Economics. Now, it was indeed the case that the Institutional economists of the 1920s and 30s were enthusiastic about the use of statistics as a way of describing and understanding socioeconomic processes. They did not, however, regard legal and political analyses or case studies as somehow less valuable or legitimate. A quick review of the Institutionalist literature of the period, as is provided by Malcolm Rutherford in Chapter 2 of his 2011 book, will show this to be the case. Valued OI work took many forms and as Rutherford wrote:

> 'Being "scientific" meant not being satisfied with speculative armchair theorizing based on unreal premises, including an outmoded psychology... Being scientific did mean being investigative, exposing hypotheses to critical empirical examination, and bring economic thinking into line with recent developments in related disciplines such as psychology, sociology, anthropology, law, and philosophy' (Rutherford, 2011, p. 26).

The work cited by Rutherford as meeting this requirement will be seen to involve a wide variety of analytical approaches.

That OI economists did think and often did write that numbers might be the best tool under many circumstances for understanding the experience of people in the U.S. economy can be illustrated by the work of Morris Copeland on money flows. (See Copeland, 1952; Dawson, 1996, Part II.) Copeland (following the lead, as he said, of J.R. Commons) recognised that transactions that were completed by money flows were basic provisioning actions in our modern economy and he set out to trace those money flows. As he did so he was not unaware of the full range of experiences that accompanied these transactions (Copeland, 1946). But, in an 'accounting economy' dealing with accounts is a good way to understand what is happening. Here is how Copeland put it,

> 'Because money plays a major role in organizing our economy, it has frequently been characterized as a money economy. In a significant sense it is also an accounting economy. Our system of moneyflows has become so complex that many transactors must keep detailed accounting records. Such records, and financial statements and reports derived from the, today help significantly to organize economic activity '(p. 8).

Copeland was not saying that behaviour must be quantified for analysis to be scientific. He was saying two things that set his work, and that of his mentor, Wesley C. Mitchell, apart from earlier work in Economics. First that the 20th-century U.S. economy was largely organised by money, so money flows were important although largely ignored in earlier economic analysis. And, he was saying that we should use the records that the subjects of enquiry used in order

to understand what they were doing. In stating the importance of his approach, Copeland was arguing with the classical and neoclassical ancestors of his chosen discipline.

So too was Wesley Mitchell arguing with economists when he wrote the passage that Hermann quotes from Mitchell's Presidential Address to the American Economic Association. What Mitchell was arguing was not that quantitative data was required for scientific exploration; he was quite specifically arguing that Alfred Marshall had conceived of 'economic behaviour as controlled by two opposing sets of motives, the motives that impel us toward consumption and the motives that repel us from labour and waiting' (Mitchell, 1925, p. 25.) Mitchell went on to say that he doubted that it would be possible to make more precise statements about these motives, just as he doubted that it was going to be possible to measure 'pleasure' or 'the strength of desire'. He went on to say that use of these terms 'are something that the theorist adds to the data' and that 'In the present state of knowledge of human nature, such interpretations smack more of metaphysics than of science' (1925, p. 25).

I can see why Hermann equates this position with the kind of positivism of mechanistic behaviourism advocated by J.B. Watson, but it is nonetheless a mistake to do so. Watson had rejected the 'functional psychology' that was part of the social science context in which Veblen, Dewey, Mitchell, Copeland and other OI economists were developing their ideas and arguments. (For more on this, see Rucker 1969, Chapter 3 entitled 'Psychology: Functionalism and Behaviorism'.) It may also be tempting to equate what Mitchell wrote in his Presidential address with later 'positivistic' forms of argument about the irrelevance of assumptions so long as predictions are shown to be correct. But that too would be a mistake. Mitchell sought realism in assumptions, in evidence, and in conclusions.

So, contrary to what Hermann argues, my reading of the literature does not lead me to the conclusion that the Institutional economists fell victim to a 'positivist attitude' by considering statistical analysis to be the only truly scientific form of evidence. The debate over the use of 'measurable' observations was of greater significance in some other disciplines and, arguably, in some other forms of economic analysis where the important variables did not emerge in numerical form from the human actions deemed important. Neither Mitchell nor Copeland had to, nor did they want to, 'impute' values; they wanted to use the values recorded by the subjects of their studies.

Not only do I think Hermann wrong in thinking that OIE was weakened by the adoption of a 'positivist attitude,' I also think that he is wrong in saying that this alleged adoption created a conflict with a 'pragmatic conception of behaviourism' in OIE. Behaviourism – as an analytical approach – was, as already noted, important in the development of modern Psychology, but actually had very little importance in the development of OIE. However, Pragmatism was, and continues to be, an important foundation for OIE. What the Pragmatic tradition, as established by C.S. Pierce, W.W. James, J. Dewey and others, means is that individuals and the groups of people who are the basic unit of OI analysis are thought to adapt 'means to ends that cumulatively change as the [life] process goes on' (Veblen, 1906, pp. 74-75). Pragmatism is not, as is often thought, simply a matter of expediency, nor is it in any sense a form of analysis that sets it apart from 'positivism'. Rather, it a fundamental assumption that the ends or objectives of human action change as the means of achieving them change. This process is cumulative and interactive. The importance of this for OI economists was that it meant there was no one thing – such as maximisation of utility – that could be both taken as an unchanging goal and be given any specific meaning. The acceptance of the Pragmatists' view of humans and their evolution through time and space also meant that individuals took *and changed* their tastes and

preferences as part of an ongoing social process that was shaped by the means of achieving what was desired/needed.

This proposition represented a fundamental challenge to economic thought. The introduction of active human agents who were changed by their environments even as they changed those environments, could not be reconciled with the 'globules of desire' that, in the famous Veblen passage, 'oscillate under the impulse of stimuli that shift about the area, but leave him intact' (Veblen, 1898, p. 389.) The problem was that the 'principles of economics' that Alfred Marshall had so meticulously set forth were among those aspects of 19th-century thought that were hard to test using newly-available statistics and were hard to reconcile with the economy as the OI economists were describing it. Mitchell thought, as he said in his Presidential Address, that economists would change their questions and would simply not try to prove the truth of those principles. But that is not how it went down.

Ragnar Frisch, whom Hermann quotes to illustrate the weakness of the neoclassical rejoinder to Mitchell's Presidential Address, gave the most direct statement about being unwilling to abandon the 'principles' that were inherited and were held to be both true and of greatest importance for economic analysis. The background is this: Henry Schultz, an agricultural economist who – as the US Department of Agriculture began collecting data on prices and purchases of goods such as sugar – realised that the observed price and purchase combinations, if translated into Marshallian terms, were points of intersection between a theoretical and unknown demand curve, with a theoretical and unknown supply curve. In order to construct a demand schedule you had to hold the supply schedule fixed or *vice versa*. The question was how to adjust for the many other things that, as textbooks briefly remind students, must be held constant for the common textbook demand-supply curves to look as they do.

It was at this point that Ragnar Frisch and his co-author Frederick Waugh in effect created modern econometric protocol and set neo-classical economics on its modern and somewhat awkward path. (For much more on this see J. Morgan, 2016 and the essays contained in that volume.) In a manner almost identical to the example that Hermann provides of how the truth is known about the tides, Frisch and Waugh declared that

> 'An empirically determined relation is "true" if it approximates fairly well a certain well-defined *theoretical* relationship, assumed to represent the nature of the phenomenon studied. There does not seem to be any other way of giving a meaning to the expression "a true relationship"' (Frisch and Waugh, 1933, p. 38).

Marshall's supply and demand schedules were thus declared, that to which statistical evidence had to bend.

The declaration of Frisch and Waugh should be taken as much more than a solution to a technical problem for statisticians. It was also a statement that Marshall's principles should be taken as definitive. By itself this would not have been sufficient to lead to the decline of OIE, an approach that involved a fundamental rejection of Marshallian economics as set forth in his *Principles.* Many other things were involved as well.

My view is that OIE declined, in part, because of the support and the prestige that Frisch and Waugh and their colleagues brought to the new field of econometrics, and to their adoption of Marshall's principles as fundamentally true. I have also written elsewhere about the narrowness of approach adopted by many followers of Ayres as a major reason for the decline of OIE (see Mayhew, 2008). And, as Hermann notes in his final remarks, the failure of OIE to offer deterministic results and its gradualist approach to socioeconomic change have

contributed to that failure. So too has the reformist role that Keynesianism (of an earlier sort) and then Post-Keynesianism have offered to academics eager to help create better economies. The story of the decline, and indeed, the failure of reformist and gradualist social science in the post-WWII era, remains to be fully told. I am pretty sure that when it is told, and told well, that the failure of OIE will not be explained as Hermann has in the essay under review.

Finally, I will note that one of the continuing strengths of OIE, a strength not usually so regarded in the mainstream of economics, is the continuing interdisciplinarity of work done by OI economists. Hermann takes the importance of Psychology to the 1920s OI economists to indicate that that is still the field of greatest interdisciplinary importance. Not so. Having abandoned the notion of a narrowly rational economic man/person, OI economists have moved on and found opportunities to work with legal scholars, sociologists, anthropologists, political scientists and many others. Psychology is where the action was back in the first two or three decades of the 20th century. For those interested in economic issues and policies, that is no longer the case, but OI economists have not, as a result, turned inward. That interdisciplinarity flourishes can be confirmed by looking at the affiliations of those who have published in the leading OIE journal, *The Journal of Economic Issues,* over the last several decades.

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