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business research***

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# INHERITED PHILOSOPHY OF SCIENCE? ECONOMICS AND INTERNATIONAL BUSINESS RESEARCH

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## ABSTRACT

*International business (IB) research is traditionally heavily reliant on economics. In this paper, we review selected debates in the philosophy of science of economics and consider their relevance for economics-based IB research, given important characteristics of IB such as phenomenon-orientedness, concern with data and facts, and limited use of formal mathematical models and unrealistic assumptions in the analysis. We argue that, like in the case of mainstream economics, Lakatos' concept of scientific research programmes (SRP) is more useful for understanding the philosophy of science of economics-based IB than Popper's falsificationism. Following this, we discuss characteristics of two possible IB SRPs; Internalization theory and Dunning's OLI paradigm. Finally, we discuss the approach to modelling in IB, finding it to reflect a relative commitment to scientific realism.*

## INTRODUCTION

International business (IB) research has largely relied on existing theories such as transaction cost economics and institutional theory (Doz, 2011), to the extent of being “an appendage of economics, psychology, sociology and related management disciplines” (Devinney, Pedersen, & Tihanyi, 2010, p. 35). Of these ancestor disciplines, economics has traditionally dominated IB thought, particularly when it comes to firm behaviour (Toyne & Nigh, 1997a, p. 177); and for Buckley, IB is simply “a branch of applied economics” (Buckley, 1996, p. 8). The foundations of IB include the works of economists such as Buckley and Casson (1976), Hennart (1982), Hymer (1960/1976), McManus (1972), Rugman (1981), Caves (1996) and Dunning (1980). A question, then, is whether the philosophy of science of IB is simply inherited from economics; or whether there is something about the IB context and application of economics which makes it distinct from a philosophy of science point of view. Adapting Sullivan’s (1998) metaphor, what is the relative importance of the “genes” and the distinct “environment” of IB?

In contrast to the little attention to such issues in IB (Toyne & Nigh, 1997b),<sup>1</sup> extensive philosophy of science debates have taken place in economics over the last decades, concerning *inter alia* the applicability of the ideas of Popper, Kuhn and Lakatos to economics; the philosophical implications of (mathematical) modelling, as well as scientific realism and critical realism. The basic premise for this paper is that given the important role of economics in much IB scholarship, studying the philosophy of science of economics may aid IB scholars in better understanding the philosophy of science issues surrounding their own work. In Toyne’s words (1997, p. 630):

To the extent that we [as IB scholars] are dependent on the ontological and epistemological assumptions, paradigms, theories and constructs of the social sciences and the business disciplines, we must be sensitive to the philosophy of science debate that is occurring in these fields, and to the implications that this debate has for our work.

As Toyne’s remarks suggest, an important part of our task is to investigate more closely how economics is used in IB research, and what are the philosophy of science implications of this

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<sup>1</sup> Certainly, several IB authors have noted a positivistic focus in much IB research (Birkinshaw, Brannen, & Tung, 2011). Some IB scholars describe themselves mainly as economists (Boddewyn, 1997). Overall, the issue of a distinct IB ontology and epistemology appears so far to have proven elusive (Sullivan, 1998; Toyne & Nigh, 1997b).

usage. Some of the characteristics of the IB breed of economics that spring to mind are the following. First, it is often noted that IB is traditionally an empirically driven or phenomenon-oriented field, while much economics research has been criticized for a perceived lack of connection with the real world. IB has also been related to important policy issues. Sometimes, distinct IB contributions have arisen from omissions in economics, such as a lack of a proper theory of the multinational enterprise (MNE) (Devinney, et al., 2010). IB also generally involves less use of formal mathematical modelling, and of the idealizing and unrealistic assumptions often associated with economics. To foreshadow an argument to be developed and qualified below, an IB scholar would less likely subscribe to economist Milton Friedman's (1953/2008) view that the only way to judge a theory is by looking at whether it gives correct predictions or not, and that descriptively false assumptions are, if anything, a virtue of a theory. Though IB theory certainly also makes important simplifications (Hashai, Asmussen, Benito, & Petersen, 2010), attention to data and context has always been an important ingredient in IB research (Buckley & Casson, 2003).

This may have implications for the philosophy of science of IB. First, one may ask whether the greater empirical focus of IB implies a greater weight on falsificationism, i.e. the idea that science is characterized by developing theories that are empirically refutable (Popper, 1959); or a greater emphasis on scientific realism. On the other hand, given IB's reliance on economics, one may ask to what extent IB economics is distinct from the economics scientific research programme (SRP) in Lakatos' (1970) terms; i.e., whether IB is simply part of the protective belt of auxiliary hypotheses around the hard core of assumptions on maximization and equilibrium in the economics SRP. One might also inquire about the status of the dominant analytical framework for IB, Dunning's eclectic paradigm (Dunning & Lundan, 2008), as an IB SRP. These are among the questions that will be discussed in this paper.

Our method is essentially indirect. We start in the next section with a brief and inevitably simplified description of the philosophy of science of economics. Since we largely attempt to infer IB's philosophy of science from its practice, we must also consider how actual economics practice reflects official economics methodology. Only by doing so can we try to ensure that we are in fact comparing apples to apples, rather than to idealized apples. Based on the insights from this discussion, we turn in the following section to the main contribution of this paper, which is an analysis of the distinct characteristics of IB economics and its "revealed" philosophy of science. Much previous discussion related to the philosophy of science of IB has concerned what exactly is the domain of IB research (Toyne & Nigh, 1998;

Toyne & Nigh, 1997b). Here, we partly bypass this challenging issue by restricting our scope to a specific set of IB studies: We focus on the research descending most directly from economics and related to MNEs and international production; i.e. that relating to the internalization school and transaction cost economics, as well as the economics-based paradigm of Dunning (representative works include Buckley & Casson, 1976; Dunning, 1980, 1988; Dunning & Lundan, 2008; Gatignon & Anderson, 1988; Hennart, 1982, 2000; Rugman, 1981). We will not focus on the Uppsala internationalization school (Johanson & Vahlne, 1977) and more or less closely related work (Aharoni, 1966, 2010; Aharoni, Tihanyi, & Connelly, 2011; Buckley, Devinney, & Louviere, 2007; Perlmutter, 1969); some references to these streams will however be made as we go along.<sup>2</sup> Nevertheless, we remain firmly within some attempted definitions of the core issues of IB, such as Hennart's (2010, p. 257) according to which "IB scholars study the governance of interdependencies between individuals located in different countries, and hence separated by geographic, institutional and cultural distance". In a final concluding section, we summarize and discuss our findings and suggest some directions for further work.

While our limited scope implies that we can only give a partial view on the philosophy of science of IB, it is an important view given the central role of economics and rationality-based perspectives in IB. The interest of such an analysis is further heightened by proposals that the economic principle of maximization be a unifying assumption for a general IB theory (Casson, 1997). There is indeed a divide in IB research between rationalist theory/economics scholars lamenting a loss of parsimony by moving from a "positivistic" to a "multidisciplinary" approach on the one hand (Buckley & Casson, 2003, p. 222), and those that object to the dominance of rationality-based research in IB or at least wish to see it complemented by insights from "behavioural" theories and a greater focus on managerial decision-making on the other hand (Aharoni, 2010; Brouthers & Hennart, 2007).

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<sup>2</sup> It is perhaps also worth pointing out that we do not consider here the standard general equilibrium economics approach to MNEs, though this literature is also increasingly concerned with firm heterogeneity (e.g. Helpman, 2006; Helpman, Melitz, & Yeaple, 2004). The prospect for a (re)integration of the mainstream economics approach to the MNE with IB economics would be an interesting issue for future work.

## A BRIEF TOUR OF THE PHILOSOPHY OF ECONOMICS

### *A brief exposition of economics*

Most economics textbooks start with some version of Lionel Robbins' definition of economics as "the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses" (1932/2008, p. 75). While the main object of study is obviously markets of various types, it is often argued that economics is above all a distinct method based on a set of core principles that can be applied to a wide range of issues (Casson, 1997). Thus, for example, for Becker (1978, p. 5) the heart of the economic approach is "[t]he combined assumptions of maximizing behaviour, market equilibrium and stable preferences, used relentlessly and unflinchingly".<sup>3</sup> As is well known, formal mathematical modelling and quantitative statistical (econometric) analysis also play a key role in most economic research. Exaggerating somewhat, the philosophical self-image of economics is that of a "genuine science" based on deduction and developing refutable implications (Lazear, 2000, p. 99). Indeed, the ideal for economics is traditionally the physical sciences, with core concepts such as equilibrium borrowed from physics.

This image is, of course, too simple. Though economics has been relatively slow to pick up on philosophy of science debates in natural sciences (Beed, 1991), an extensive literature has explored issues of philosophy of science in economics. Economics in fact has a long methodological tradition going back to at least to the 19<sup>th</sup> century (Blaug, 1980). Until about the 1960s, the scattered discussions of economic methodology were, however, largely centred around the views of certain prominent economists (Backhouse, 2012). One view that has remained influential is that of Milton Friedman, whose essay, *The methodology of positive economics* (1953/2008), has been argued to capture many economists' intuition about their field. Friedman famously argued that the only<sup>4</sup> way to test economic theories is to compare their predictions with reality and that unrealistic assumptions are, if anything, a virtue of a theory. The following passage from the essay is probably one of the most quoted or paraphrased in economic methodology (1953/2008, p. 218):

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<sup>3</sup> It is worth emphasising that rationality is a more fundamental assumption than the self-interest assumption commonly associated with economics (Angner & Loewenstein, 2012). Rationality in economics is understood instrumentally as how individuals' actions promote their goals (Torsvik, 2005); or put differently, rationality is expressed in "the maximisation assumption" that "[f]or all decision makers there is something they maximize" (Boland, 1981, p. 1034)

<sup>4</sup> "Mainly" would be more accurate, as Friedman's essay has more nuances than we can provide here. Entire essays could be (and, indeed, have been) written about possible interpretations (Mayer, 1993).

Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense).

For Friedman, it is important to distinguish the assumptions of a theory from the phenomena which the theory is intended to explain. Though the assumptions may also be compared with reality, it is the comparison of the predictions with reality that matters. Friedman famously gives the example of predicting the shots made by an expert billiard player, proposing that

excellent predictions would be yielded by the hypothesis that the billiard player made his shots *as if* he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas (1953/2008, p. 222, emphasis in original).

By analogy, Friedman hypothesises that firms behave *as if* they were rationally seeking to maximize expected returns, having full knowledge of all necessary data (i.e., that they perfectly know all the relevant cost and demand functions, are able to calculate marginal revenue and cost, and choose actions equating these). Of course, this is not a realistic description of business managers any more than the preceding was of billiard players. For Friedman, however, the point is that a model with these unrealistic assumptions may nevertheless give good predictions; and predictions are what matter for judging a theory. Not all economists agreed with Friedman. One objection is that one does not only want theories to predict, but also to *explain* (Blaug, 1980). In any case, Friedman’s view provides a useful frame of reference for the following discussion. For one thing, it lends itself to a variety of interpretations. The focus on empirical predictions has been interpreted both in terms of verificationism and falsificationism; and the issue of realisticness of assumptions is closely related to modelling and to the issue of scientific realism versus instrumentalism (Hanisch, 2003). On the other hand, the prominent role given to maximization can be linked to the question of paradigmatic features (in Kuhn’s sense) and scientific research programmes (in Lakatos’ sense) in economics. We explore these issues below.

*Popper, Kuhn and Lakatos and the rise of the economic methodology field*

It was not until the 1970s that economic methodology emerged as a distinct field. The following decades witnessed lively debates on the role of falsification in economics, and on whether economics could be understood in terms of Lakatos' scientific research programmes (Backhouse, 2012).<sup>5</sup> As is well known, Popper (1959) replaced the principle of verifiability of the logical positivists with the principle of falsifiability as the demarcation criterion for science;<sup>6</sup> arguing there can be no logic of proof; only one of disproof. Thus, for a theory to qualify as science, it must be made clear what observations would refute the theory. In particular, Popper criticized the use of "immunizing strategies"; i.e., ad hoc modifications to a theory in order to accommodate contradictory empirical evidence. Popper's views have resonated strongly with economists. However, it has been argued that despite official rhetoric, falsificationism is little practiced (Blaug, 1980; McCloskey, 1983). Blaug (1984, p. 375) argues that "the central weakness of modern economics is in fact the reluctance to produce theories which yield unambiguously refutable implications". Now, few economists believe that economic theory does not need to be tested empirically. Certainly, "[t]here is a great deal amount of empirical activity in economics, the facts do matter, but they matter in a much more subtle and complex way than falsificationism allows" (Hands, 1993, p. 68).

Indeed, falsificationism demands more than simply showing the theory to be consistent with evidence. As Hands (1993, p. 62) puts it, the falsificationist "proposes a bold conjecture" which is then "severely tested by comparing its least likely consequences with the relevant empirical data". Blaug (1984, p. 376) goes as far as arguing that "economists spend much of their time showing that the real world bears out their predictions, thus replacing falsificationism, which is difficult, with confirmation, which is easy". Through a series of "case studies", Blaug (1980) argues persuasively that falsificationism does not have top priority in economic research. A prime example of this is the theoretical general equilibrium analysis programme, concerned in particular with the conditions for the existence of equilibrium. It hardly needs to be argued that falsificationism is not the main driving force behind this research.

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<sup>5</sup> Few economists have, however, embraced the total relativism of e.g. Feyerabend (1975).

<sup>6</sup> A naïve version of falsificationism would seem to suggest that a single empirical refutation means scientists should abandon it. Of course, this is inappropriate *inter alia* because many implications, not least in the social sciences, are stochastic; a theory must be repeatedly refuted before it can be said to be falsified (Blaug, 1984).



Next, taking an example from international trade theory, Blaug shows how the apparent refutation of the Heckscher-Ohlin theorem (HOT) by Leontief's (1953) finding that US trade patterns (i.e., exporting labour-intensive goods and importing capital-intensive goods) were the opposite of what the theory would predict (the *Leontief paradox*) led few trade economists to abandon the theory.<sup>7</sup> For these economists, "the factual accuracy of the HOT was a minor question because it was regarded anyway as only the first approximation to the real-world conditions of different taxes, tariffs, transport costs, economies of scale, demand conditions, factor mobilities and imperfections of competition" (1980, p. 211). Economists continued refining the theory, addressing puzzles raised by the Leontief paradox such as what a factor is and the implications of including multiple factors. Finally, discussing Becker's work on the economics of the family (an often cited example of "economic imperialism") Blaug (1980) starts by commending Becker's announcement of a falsificationist approach by among others assuming stable preferences: The rationale being, of course, that if preferences were allowed to change, they could always be invoked in an ad hoc manner to explain deviations from the theory's predictions. On the other hand, Blaug argues that these studies are, in practice, essentially concerned with verification; taking a real-life feature previously not studied by economists, and showing that it can be explained by the economic approach. What these studies do not do is predict novel facts that the theory was not originally designed to explain. Neither is the economic approach explicitly tested *vis-à-vis* alternative theories taken; e.g., from sociology or anthropology. Popper indeed noted that when testing one theory, we usually have an alternative "up our sleeves", and "the falsifying experiment is usually a crucial experiment designed to decide between the two" (Popper, 1959, p. 87n). Such tests are rarely found in economics.<sup>8</sup>

If falsificationism is not widely practised, there may be good reasons for this. Falsification is problematic even in principle following the Duhem-Quine thesis (Quine, 1953) that one can never conclusively falsify a single hypothesis, since it is always tested in conjunction with various auxiliary hypotheses (including hypotheses on observation), and we cannot know which hypothesis is responsible for the empirical disconfirmation. Strong auxiliary hypotheses are, of course, an important part of economics (Blaug, 1980; Hands, 1993); some of these are even infalsifiable (e.g., eventually diminishing returns). Furthermore,

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<sup>7</sup> The HOT states that a country will export those goods whose production is intensive in the country's relatively abundant production factor, and import those goods whose production is intensive in the country's relatively scarce factor. This is a clear empirical implication which may be refuted.

<sup>8</sup> Even explicit tests of competing economic theories are rare. For one honourable exception, see Markusen and Maskus (2003) who explicitly attempt to discriminate between alternative economic theories of FDI.

mathematical modelling often requires a number of assumptions to ensure tractability, such as constant returns to scale. Any one of these hypotheses may be the culprit in the case of empirical refutation. On the other hand, failing to find empirical support for an economic theory will lead few economists to question the basic assumptions of individual maximization and equilibrium. Indeed, as noted by Casson (1997, p. 185), “[t]he assumption of optimization is essentially immune to refutation. By itself it implies nothing. It is only when combined with assumptions about preferences and constraints that it yields predictions.” This is clearly at odds with falsificationism: “immunizing any part of scientific theory would be in conflict with Popper’s falsificationist method of bold conjecture and severe test” (Hands, 1993, p. 67).

This suggests that economics is better understood in terms of the work of Kuhn and Lakatos than that of Popper.<sup>9</sup> Kuhn (1962) showed that Popper’s account of science did not reflect actual scientific practice: Scientists do not readily cast away theories following apparent disconfirmations, and fortunately so. Instead, Kuhn described *normal science* taking place within paradigms, consisting of “the general theoretical assumptions and laws and techniques for their application that the members of a particular scientific community adopt” (Chalmers, 1999, p. 108). Working within a paradigm allows scientists to refine their theories and methods, conduct experiments, etc., while taking their foundations as given. However, a paradigm will inevitably encounter anomalies it cannot explain. When these become serious enough, the paradigm will face a *crisis*, in particular if there is a new competing paradigm which can account for the anomalies. At some point, the change to a new and essentially incompatible paradigm takes place via a *scientific revolution*. Kuhn argues, however, that this change cannot be made on strictly objective grounds and will to some extent involve a “leap of faith”, for two main reasons (Okasha, 2002). First, there is *incommensurability* related to the lack of a common theoretical language or framework with which to compare competing paradigms. Second, the idea of *theory-ladenness of data* implies that data are inevitably

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<sup>9</sup> It is here interesting to note another, less well known part of Popper’s work, namely the method of *situational analysis* which he designed specifically for the social sciences (Hands, 1985). Here, Popper suggested that the postulate of rationality (the rationality principle) should always be maintained, and one should attempt to find explanations for social behaviour in terms of the “situation” in the situation in which the agents find themselves (the concept of situation also includes the actors’ goals). When facing empirical refutation, the assumption should always be that the situation has not been specified correctly. This is clearly analogous to the maximization assumption, and indeed, Popper referred to this as the economic method. We will not here dwell on the issue of whether there is an inconsistency in Popper’s work in this respect (see Hands, 1985).

“contaminated” by theory, and that there are therefore no theory-independent data which could be used to decide between the theories.<sup>10</sup>

Coats (1969, p. 292) argues that despite some heterodoxy, “economics has been dominated throughout its history by a single paradigm – the theory of economic equilibrium via the market mechanism”. Nevertheless, Coats (1969) argues that the Keynesian revolution was a paradigm shift. John Maynard Keynes departed from the principle of methodological individualism, as the consumption function was not derived from individual maximizing behaviour (Blaug, 1984). Keynes also allowed for persistent disequilibrium in the labour market (or more precisely an “underemployment equilibrium”; which did not fit with the idea that markets would always work to ensure equilibrium).

The question raised by Kuhn’s work was whether one can really speak about scientific progress at all. Lakatos’ (1970) answer to the dual limitations of Popper’s falsificationism and Kuhn’s relativism (Chalmers, 1999) was to suggest that scientific research programmes (SRPs) rather than individual theories are the appropriate units of appraisal for scientific progress; and that some parts of an SRP are more fundamental than others. According to Lakatos, SRPs consist on the one hand of a *hard core* of unquestioned fundamental metaphysical assumptions; and on the other hand a *protective belt* of more peripheral assumptions that may be allowed to be falsified. The SRP thus provides systematic guidelines (which are missing in falsificationism) for which hypotheses should be modified following empirical disconfirmation. Finally, Lakatos described *positive and negative heuristics* relating to the “do’s” and “don’ts” in terms of how research should be conducted within the SRP. The negative heuristic is primarily about protecting the hard core by making all adjustments in the protective belt. In contrast, the positive heuristic provides a forward-looking component and

[c]onsists of a partially articulated set of suggestions or hints on how to change, develop the “refutable variants” of the research programme, how to modify, sophisticate, the “refutable” protective belt (Lakatos, 1970, p. 135)

One important characteristic of the positive heuristic is that it can also anticipate what kinds of refutations will be met during the development of the programme; and indeed it “forges

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<sup>10</sup> Of course, both these ideas are exaggerated (Chalmers, 1999). It is, however, notable that some of the data of economics depends on the eye of the beholder (Hands 1993 gives the example of “involuntary unemployment”).

ahead with almost complete disregard of ‘refutations’”; while it is the “verifications” that “provide the contact point with reality” (1970, p. 137).

It is not surprising that economic methodologists picked up on Lakatos’ ideas. Indeed, Lakatos specifically referred to economics and argued that strict falsificationism would imply throwing out all budding social science theories, without necessarily having good alternatives; programs need time for development in order to achieve their potential, before being fully confronted with the data (Chalmers, 1999). Thus, following criticism that general equilibrium analysis (GEA) is unfalsifiable, Weintraub (1985) recast it as an SRP involving six hard core propositions: “HC1, There exist economic agents; HC2, Agents have preferences over outcomes; HC3, Agents independently optimize subject to constraints; HC4, Choices are made in interrelated markets; HC5, Agents have full relevant knowledge, and; HC6, Observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium states” (1985, p. 109).<sup>11</sup> The protective belt on its hand consists of GEA’s actual theories, auxiliary hypotheses, empirical methods, etc.; in practice covering most of applied microeconomics (Hands, 1993, p. 67). Similarly, Latsis formulated four hard core assumptions of the neoclassical theory of the firm SRP: (i) Profit maximization; (ii) Perfect knowledge; (iii) Independence of decisions; and (iv) Perfect markets (Latsis, 1972, p. 209). Importantly, any question about the realism of such hard core assumptions fails to recognize their role in the methodology of the SRP (Blaug, 1980). On the other hand, questions relating to the protective belt are perfectly legitimate; i.e. those relating to the realism of auxiliary assumptions that determine the applicability of the theory, such as homogenous products, “large” numbers of firms, and free entry and exit (Latsis, 1972; Blaug, 1980).

What then about the important question of scientific progress in Lakatos’ setup; and how does economics perform on this account? For Lakatos, an SRP is *progressive* when it is able to remain coherent and at the same time is able to predict “novel facts” that competing programs cannot. On the other hand, “if and when the programme ceases to anticipate novel facts, its hard core might have to be abandoned” (Lakatos, 1970, p. 134). Not surprisingly, economists have disagreed on whether specific economic SRPs such as the general equilibrium

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<sup>11</sup> This and the following example are only given to provide a flavour of how economic methodologists have applied Lakatos’ ideas. Indeed, economists have suggested a variety of different SRPs with different hard cores (Caldwell, 1991).

programme are progressive or degenerating (Toruno, 1988).<sup>12</sup> Writing in 1991 Caldwell found “reason to doubt that there exist many examples of progressive research programmes in economics” (Caldwell, 1991, p. 100). While the details of such assessments might also be interesting for IB scholars (Toruno, 1988), we must here be content with one simple example. Considering the Keynesian revolution in an SRP framework, Blaug (1984) notes that it appeared progressive in the 1930s when it led to confirmed predictions that were surprising in terms of the prevailing framework at the time; in particular the role of government spending in moving the economy out of a recession through multiplier effects. On the other hand, the Keynesian program appeared to be degenerating in the 1970s when it could not account for stagflation (Caldwell, 1991); it was then superseded by an alternative SRP based on the idea of rational expectations.<sup>13</sup> In general, the application of the SRP concept has been hampered by the fact that the idea of “novel facts” is somewhat unclear (Chalmers, 1999).

*Recent debates: Models and scientific realism*

In contemporary debates, Popper, Kuhn and Lakatos have faded into the background and new philosophical issues have been taken up (Backhouse, 2012; Hodge, 2008; Lawson, 1997); of which we will focus the issue of modelling and its relationship with scientific realism. These issues link back to Friedman’s view above and are an appropriate way to end our brief tour of the philosophy of science of economics. Critics often question whether much economics research has anything to do with reality at all (Mäki, 2012). Indeed, economists are somewhat self-conscious about this as illustrated by the following quote from Klamer and Colander (1990, p. 19; cited in Røgeberg and Nordberg, 2005)

‘Of course this assumption is absurd,’ a well-known economist noted during a recent seminar, ‘but hey, isn’t all we do absurd and utterly unrealistic?’ People laughed, and he continued solving the model.

One could ask whether scientific realism is even relevant for the social sciences. According to standard definitions from a natural science context, scientific realism “holds that the physical world exists independently of human thought and perception.” (Okasha, 2002, p. 58). That is, under the standard definition of scientific realism the item under study is not dependent on

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<sup>12</sup> It would lead us too far to discuss the scientific progress of general equilibrium theory as an SRP here; it is nevertheless interesting to note that the purely theoretical GE programme seems to have been more or less abandoned by economists, following disappointing results (Katzner, 2010; Kirman, 1989).

<sup>13</sup> Keynesian and Post-Keynesian SRPs still co-exist with the neoclassical perspective, and are cited as examples of competing SRPs (e.g., Toruno, 1988).

human minds for its existence, which obviously does not fit for e.g. money, central banks and firms. To accommodate such phenomena, Mäki (2012) proposes a *minimal scientific realism*, where one important change is replacing the criterion of *mind-independency* with *science-independency*. Now, how can social reality be independent of economics, given that economic theories clearly affect both practice (Black-Scholes formula) and ideology? Or consider the claim that economics and business students act more self-interestedly (Hellmich, 2012).<sup>14</sup> Here, Mäki (2012, p. 21) argues that economics may well change the beliefs and motives of actors, but is not directly *constitutive* of these practices, and that in this sense it does not directly affect economic reality.<sup>15</sup> The question of realism (or better: realisticness) of assumptions continues to provide food for debate for economists and their critics (Mäki, 2012). According to the instrumentalist view, some types of theoretical entities can be regarded as “convenient fictions” (Okasha, 2002) that may allow good predictions to be made (we recognize Friedman’s ideas here). In contrast, realists maintain that scientists should also aim to discover “truths” and explain phenomena (Hausman, 2008). Not all methodologists accept a direct link between realism in assumptions and scientific realism, however. For example, Mäki (2012) sees unrealistic assumptions as perfectly compatible with (a suitable version of) scientific realism; indeed, “unrealistic assumptions can even be the very means for striving for the truth” (Morgan & Knuuttila, 2012, p. 53). Thus, “when an economist makes unrealistic assumptions [...] she is deliberately employing strategic falsehoods in order to attain some epistemic and pragmatic gains. This is the point of idealization” (Mäki, 2012, p. 10).

Unrealistic assumptions are often linked to (mathematical) models, and some insights on the relationship of these models to realism can be gained by considering different approaches to modelling. Morgan and Knuuttila (2012) discuss two different views of models as *idealizations* or *constructions/fictions*. Idealization is “typically portrayed as a process that starts with the complicated world with the aim of simplifying it and isolating a small part of it for model representation” (2012, p. 51). In this view, models are used to abstract “causally relevant capacities or factors of the real world for the purpose of working out deductively what effects those few isolated capacities or factors have in particular model (i.e., controlled) environments” (2012, p. 52), and is essentially about capturing “only those core causal factors,

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<sup>14</sup> Cf. also the argument by Ghoshal and Moran (1996) that the opportunism postulate of transaction cost economics (TCE) negatively affects behaviour and that TCE therefore is “bad for practice”.

<sup>15</sup> In addition, some theories cannot have such an effect; for example assuming that people are perfectly rational does not make them so.

capacities or essentials of a causal mechanism that bring about a certain target phenomenon” (2012, p. 53). Walliser (2002) describes modelling in terms of reasoning on a sub-system represented in a simplified manner, isolating what the researcher believes to be the most salient aspects. The sub-system is isolated from its environment by assuming that the environment stays constant or by neglecting it altogether. One function of false idealizing assumptions is then to “help implement theoretical isolations in a controlled manner” (Mäki, 2012, p. 13); an analogy is often made with the physical controls of laboratory experiments (Morgan & Knuuttila, 2012).

Important here is also the idea that by a process of *de-idealization*, as science progresses, one can reintroduce omitted elements, bringing the model closer to the real world. In fact, much economics work consists of introducing additional complications that have been disregarded in earlier, more simplified models. Thus, reversing idealizations can be conceptualized as moving towards greater realism. In practice, however, de-idealization may be difficult for many reasons (Morgan & Knuuttila, 2012). For example, some idealizations may have been motivated by mathematical tractability. Re-introducing excluded factors may also re-introduce causal complexity in terms of non-separable causal relations that were previously assumed away. The upshot of these and several other similar issues is that it is not straightforward to move towards more realistic models through de-idealization.

An alternative to the view that models abstract, isolate or idealize essential factors or mechanisms, is that they are rather “like pure constructions or fictional entities that nevertheless license different kinds of inferences” (Morgan & Knuuttila, 2012, p. 61). In this view, models are used mainly to facilitate communication of arguments (Walliser, 2002), and are more like *caricatures* designed to illuminate some feature of the situation (Gibbard & Varian, 1978) or *fictions* which “can give us some understanding of real economic mechanisms, even though they are not interpreted as representations of real target systems” (Morgan & Knuuttila, 2012, p. 62). Morgan and Knuuttila cite Machlup’s (1967, p. 9) claim that the neoclassical firm in traditional price theory is just a “theoretical link” which should not be confused with any real firm; as well as Lucas’ (1980, p. 697) argument that

[o]ne of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost [...] A “theory” is not a collection of assertions about the behavior of

the actual economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy.

This fictional account of modelling contrasts with the realist account of among others Mäki (Morgan & Knuuttila, 2012). A limiting case of fictional models that have been labelled “absurd” models (Røgeberg, 2004; Røgeberg & Nordberg, 2005) helps make the point that models do not in all cases make claims to scientific realism. For example, in “rational addiction” models (Becker & Murphy, 1988), individuals are portrayed as rationally implementing an increasing consumption path of drugs. As interpreted by Røgeberg and Nordberg (2005), such models are not meant to be about anything real at all. Rather, given limited human cognitive abilities they are “a way to bundle facts and hypotheses in a form that exploits folk psychology”. As long as this is understood, Røgeberg and Nordberg see a useful function for such models; problems only arise when one draws inappropriate welfare or political implications (e.g., that one should not worry about drug addiction because the individuals are simply implementing their utility-maximizing path of drug consumption over time). Thus, caricatural models do not only idealize, but *add* new features (Morgan & Knuuttila, 2012), amounting to advanced story-telling or even rhetoric (McCloskey, 1983).

## **ECONOMICS AND INTERNATIONAL BUSINESS RESEARCH**

*(How) is the philosophy of science of economics relevant for IB?*

Our brief tour of the philosophy of science of economics raises several issues for economics-based IB research. First, we have seen that given essentially infalsifiable methodological assumptions, Lakatos and Kuhn seem to be more relevant for understanding economics research than Popper’s falsificationism, in which any component of the theory is, in principle, in jeopardy.<sup>16</sup> A question is to what extent the same is true for IB research descending from economics. There are some possible reasons why IB economists might be more committed to falsificationism than their colleagues in mainstream economics. Consider first the claim of Hennart (1997) that IB has had a strong policy focus. For example, the MNE posed important policy questions which were not answered by the trade theory existing at the time, and which necessitated the development of new theory (Hennart, 1997, p. 648). Convincing cases have

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<sup>16</sup> As pointed out in a footnote above, however, Popper suggested a specific methodology for the social sciences (*situational analysis*) which was very much in line with the economic approach.



been made for the importance of such “big questions” for IB research (Buckley, 2002). Or consider the statement relating to the neoclassical standard assumptions in economics that “IB scholars were better able to devise new theories because they were less reluctant to break these assumptions” (Hennart, 1997, p. 649). This suggests a greater willingness of IB economists to modify even the hard core.

Another important characteristic of IB research is the focus on firm-level heterogeneity rather than the minimalistic conceptions of the firm in general equilibrium models (Markusen, 2001).<sup>17</sup> IB does not have general and purely theoretical programs such as the theoretical general equilibrium analysis program of mainstream economics which are continually refined, with little concern for empirical testing. Even the more abstract frameworks and models such as Buckley and Hashai (2004, p. 44), aim to “compare the predictions of a synthesised model with reality”. Compared to mainstream economics, there is also relatively little use of mathematical modelling in IB. To be sure, early internalization works such as Buckley and Casson (1976), Rugman (1981) and Hennart (1982) all contain mathematical models, as do a number of later studies (Asmussen, Benito, & Petersen, 2009; Buckley & Casson, 1981). But these models are (usually) limited to specific arguments rather than being a focus in the analysis. Apparently IB scholars in general are more reluctant to leave out important factors of the main analysis to facilitate tractability of models. Hennart (1997, p. 650) notes that “[t]he need to tackle real problems has [...] privileged relevance at the expense of elegance”. We now consider whether these traits mean that the conclusions regarding economics must be modified when considering economics-based IB research.

#### *Falsificationism and scientific research programmes in economics-based IB*

The first economics-based IB contributions were largely motivated by empirical facts which did not fit into existing theoretical frameworks. According to Hennart, “The concern of IB scholars with facts and data has often exposed the inadequacies of existing theories” (1997, p. 645). Thus, Hymer (1960/1976) started with asking why a distinction is made between direct investment (involving control) and portfolio investment (not involving control), when this distinction was not reflected in the prevailing theory of foreign direct investment (FDI) which asserted that FDI, like portfolio investments, was driven by differences in interest rates

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<sup>17</sup> This does not mean that equilibrium does not play an important background role e.g. in internalization analysis. The following explicit statement is illustrative: “Our model can thus contribute to explain and predict the equilibrium size of firms across industries, between countries and through time” (Hennart 1982, p. 60).

between countries. Hymer went on to identify a number of problems with the interest-rate theory, summarizing its implausibility as follows: “if the investment is motivated by interest rates, it should move to some countries and all industries, and not, as is in fact the case, to some industries and to all countries” (1960/1976, p. 20). According to Hymer, the main shortcoming of the interest-rate theory was that it did not explain why a firm chooses to retain control over the investment object. As is well known, he went on to provide two explanations in terms of removing competition and appropriating the returns from firm-specific advantages; the latter establishing the idea that firm-specific advantages are a necessary condition for FDI to overcome liabilities of foreignness. Similarly, in their retrospective on *The Future of The Multinational Enterprise* (Buckley & Casson, 1976), Buckley and Casson (2003, p. 220) saw it as “distinguished by its strongly positivistic stance”, beginning by “summarizing the stylized facts revealed by the published statistics.” Hennart (1982) followed much of the same approach, describing stylized facts about FDI and pointing out the failures of various alternative theories to account for these facts. Hennart (1982, p. 26-27) then presented “a general model of the choice between firms and markets as organizational modes” which could account “for the industrial, geographical and historical patterns of foreign direct investment and for the ownership policies of multinational firms.”

The question here, however, is not whether there has been empirical testing and whether the theories have been broadly corroborated, but whether economics-based IB scholars have gone the additional lengths required by falsificationism; and in particular whether they have been willing to subject all parts of a theory, including core assumptions, to potential falsification. The next question is, then, whether such stricter tests have been performed as IB theory has matured. Indeed, it has been noted that with the arrival of ambitious theories such as transaction cost economics (TCE) and the resource-based view, theory testing and predictive approaches have become more of a focus in IB from the 1980s onwards (Benito, Petersen, & Welch, 2012).

Buckley’s (1988) discussion of empirical testing of the internalization theory (IT) of the MNE about a decade after *The Future* sheds some light on this. According to him, two general axioms underlying IT are that (1) Firms choose the least cost location for each activity they perform and (2) firms grow by internalizing markets up to the point where the benefits of further internalization are outweighed by the costs. Buckley refers to this general statement as “tautologous”, admitting that the general theory of the MNE cannot be tested directly. However, “restricting the theory [...] by limiting the general propositions gives rise to a

number of special theories that have empirical content and can be tested” (1988, p. 182). Linking up to Williamson’s (1975) TCE, Buckley cites IB work showing that, as expected, “the incidence of transaction costs is particularly high in vertically integrated industries, knowledge-intensive industries, quality assurance-dependent products and communication-intensive industries” (*ibid.*)<sup>18</sup>

Buckley also provides some suggestions as to what would refute IT. We here mention three: (1) a pattern of foreign direct investment contrary to the theory's predictions; (2) perfect diversification and risk avoidance, and (3) market entry behaviour at variance with predictions. Against (1), Buckley notes that “data shows that by industry and nationality there is conformity to the theory's predictions at the most general level.” (1988, p. 186). Against (2), he states that “we observe a concentration of foreign direct investment in advanced market economies where risk/return patterns exhibit close correlation one with another” (*ibid.*). Finally, discussing (3) he cites a theoretical model in Buckley and Casson (1981) predicting that “in an expanding market where two or more different modes of market servicing are used, foreign direct investment will never precede licensing, licensing will never precede exporting and foreign direct investment will never precede exporting”. Recognizing that there may be many ways that foreign market servicing can evolve, however, he adds that “tests must be formulated in a more rigorous way against data that are hard to obtain” (1988, p. 187).

Again, however, these might not be strong enough tests of the theory from a falsificationist perspective. One way of testing a theory is directing it towards new questions that it was not originally developed to explain. Thus, IT and TCE<sup>19</sup> have been applied to the questions of full or partial ownership in FDI (Anderson & Gatignon, 1986), greenfield investments versus acquisitions (Hennart & Park, 1993), strategic alliances (Chen & Chen, 2003), internal governance of MNEs (Buckley & Strange, 2011; Tomassen, et al., 2012), and de-internationalization (Benito & Welch, 1997). One particularly important application of

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<sup>18</sup> Of course, as Buckley also points out, these works have not actually measured the transaction costs of hierarchical versus market modes. Some work attempting to measure such costs has emerged recently (Tomassen, Benito, & Lunnan, 2012).

<sup>19</sup> For the moment, we lump together IT and TCE-based works, though these were to some extent developed independently (Buckley & Casson, 1976; McManus, 1972; Williamson, 1975) and there are some important differences between these streams. According to Hennart (2007, p. 4), the core similarities between internalization theory (in two variants of Buckley & Casson 1976 and Hennart 1982, respectively) and TCE are: “(i) all three theories look at the MNE as one of many potential institutions set up to organize international interdependencies; (ii) they all assume that human agents are opportunistic and boundedly rational; (iii) they all argue that agents choose institutions that maximize the gains that can be derived from the transaction, and that the rents obtained when organizing the transaction by a given institution depend on the specific characteristics of the transaction”. Some differences relate to the role of asset specificity defined as relation-specific investments as opposed to information asymmetry (see, e.g., Brouthers & Hennart, 2007, p. 401).

TCE/IT has been in entry mode research; this research has certainly not been slowed down by somewhat mixed results (Brouthers & Hennart, 2007).<sup>20</sup> IB researchers are not deterred by anomalies that apparently cast doubt on the TCE explanation. For example, while most of the predictions based on TCE are borne out empirically, Gatignon and Anderson (1988, see pp. 309 and 323) do not find support for the core idea in TCE (Williamson, 1975, 1979) that in particular the interaction between uncertainty and asset specificity promotes control through ownership. Despite this, they emphasize the overall support for the TCE perspective.<sup>21</sup> In fact, a lacking interaction effect is also found in other TCE-based studies e.g. in marketing (see John & Reve, 2010); and while TCE has allegedly received extensive empirical support, it is notable that few studies have even tested this crucial interaction effect predicted by Williamson's theory either in entry mode research (Brouthers & Hennart, 2007) or more generally (Macher & Richman, 2008).

Somewhat ironically, the celebrated paper of Kogut and Zander (1993) who posit an alternative explanation to TCE and IT for the MNE, illustrates these issues well. Kogut and Zander argue that the assumption of opportunism is superfluous to explain the existence of MNEs; MNEs are essentially social communities and "repositories of knowledge", and MNEs are explained by the fact that tacit and less codifiable information is more easily transferred within such social communities, meaning it will be associated with control through ownership. Their argument leads to many of the same predictions as TCE-based arguments, and indeed, it is unclear how their empirical analysis can effectively separate between the two explanations: Less codifiable information might be exactly the information that is most difficult to protect through market-related arrangements (e.g., patents) or licensing, increasing the risk of opportunistic behaviour (Gatignon & Anderson, 1988). Essentially, Kogut and Zander's (1993) empirical analysis can support both the TCE story and the repositories of knowledge story.<sup>22</sup>

On one account, however, IB does appear to perform better according to Popper's standards. Like most economics research, IB studies often fail to provide satisfactory tests to discriminate against competing approaches, and though IB as a field is multidisciplinary, individual contributions are rarely so (Doz, 2011). Nevertheless, exceptions exist. For

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<sup>20</sup> Though we cannot enter into details here, the differences between TCE and internalization theory may be of some importance for understanding these results (Brouthers & Hennart, 2007).

<sup>21</sup> More generally, this points to an interesting issue regarding articles that state a set of different predictions on the basis e.g. of TCE: If some hypotheses are supported and others not, to what extent can one say that the theory is really supported? Can one really speak in terms of some "average" support for the theory?

<sup>22</sup> Cf. also Brouthers and Hennart (2007) who find Kogut and Zander's (1993) results consistent with an alternative asymmetric information hypothesis.

example, Buckley et al. (2007) find that the economics-based approach performs well in specifying the choice set of foreign investment projects to be considered, but that the final choice within this set is more difficult to explain with this approach. Hashai et al. (2010) test organizational learning theory (e.g. Cohen & Levinthal, 1990) versus internalization theory (along with Kogut and Zander's (1993) theory) in explaining the diversity of foreign entry modes by MNEs. Hashai et al. (2010) in their analysis find support for the organizational learning perspective, rather than the TCE one.

Overall, however, the example of IT/TCE-based IB research seems to us to suggest that like in the case of economics, Lakatos' ideas are more applicable than those of Popper. While Buckley (1988) does not use Lakatos' terms, the fact that the axioms are essentially irrefutable might justify an interpretation of IT in terms of an SRP. This is what Swaminathan (2007) lays out in the only previous attempt of which we are aware to identify Lakatosian IB SRPs. Specifically, Swaminathan (2007) argues that IT is a progressive (the word he uses is "thriving") theoretical SRP in IB, while the "structural contingency theory" SRP is a degenerate one (he uses the word "declining"). What kind of SRP, then, is IT? Interestingly, Swaminathan argues the hard core of IT is transaction cost economics (TCE) (Williamson, 1975). Several comments can be made about this suggestion. First, it is interesting that this hard core makes no explicit reference to the international dimension (as is the case for Buckley's (1988) axioms for IT). Indeed, the main interest of many internalization scholars has been the growth of the firm in general, not only *qua MNE* (e.g., Buckley & Casson, 1976). For example Hennart (1982, pp. 60-61) considers the theory as "perfectly general", not distinguishing between "expansion of the firm within or across national boundaries". In a Lakatosian interpretation, this means that any reference to the international dimension belongs in the protective belt, and thus is secondary and not essential to the theory. Some of the challenges for IB in developing a distinct identity might be related to the fact that according to one of its most influential perspectives, IT, international expansion is simply seen as a special case of a firm's expansion. There is thus no link e.g. to the view of Grosse and Behrman (1992) that accounting for the effects of country borders and thus government policies is an important part of IB, and the view of Boddewyn that "[w]ithout political boundaries, there would be no *international business*" (1997, p. 56).

Second, specifying TCE as the hard core of IT raises some interesting issues related to the definition of SRPs. In fact, it has been argued elsewhere that TCE (Williamson, 1975, 1979) is itself an SRP. Specifically, Knudsen (1993) argues that the hard core of TCE consists of the

assumptions of bounded rationality and opportunism, along with the assumption that governance structures are chosen to economize on transaction costs; while the protective belt includes the characteristics of transactions such as uncertainty and asset specificity influencing economic organization. Some IT scholars might object to Swaminathan's (2007) view of the relationship between IT and TCE. For example, outlining the contributions of IB research to TCE, Hennart (2010) has argued that TCE is incomplete in the sense that it focuses too much on asset specificity for explaining the choice between internalization and licensing. Hennart argues that since licensing also would be expected to involve transaction-specific investments, information asymmetry is a more compelling explanation for internalization. Still, according to Knudsen's (1993) interpretation the contribution of IT, since it does not reject TCE's hard core assumptions, still takes place in the protective belt.

Whatever one's view of the above issue, Swaminathan's (2007) suggestion also raises the question of whether there are essentially SRPs at different levels; and, indeed, if we might go even further to explore the relationship of IT to economics. Now, IT/TCE is clearly distinct from the neoclassical economics SRP in the sense that it rejects core assumptions of neoclassical economic theory of the firm (Knudsen, 1993). Specifically, TCE makes two important modifications to the neoclassical assumptions (Williamson, 1975).<sup>23</sup> First, bounded rationality represents a weakening of the rationality assumption, whereby actors are "intendedly rational" (Simon, 1961) but lack the ability to identify all possible contingencies, assign a probability to them, and process them for decision-making.<sup>24</sup> The opportunism postulate on its hand states that economic agents cannot be trusted to honour all agreements; thus departing from neoclassical economics where it is implicitly assumed that all "contracts" are kept or enforced. But what if both TCE and neoclassical economics are themselves part of the protective belt of a more general "economic approach" whose hard core consists of the minimal assumptions of maximization and equilibrium? Indeed, TCE has been cited as an example of economics modifying itself in response to criticism while retaining core

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<sup>23</sup> It does, however, retain the view of the firm as maximizing as a unitary actor. This contrasts with corporate governance theories allowing for the fact that firm decisions may not reflect profit maximization, but rather the motives of managers, etc. Hennart (1982) recognizes this and makes an argument for why transaction cost minimizing choices are nevertheless reasonable to assume. Our guess is that an emerging literature linking corporate governance and IB (Strange & Jackson, 2008), implicitly questioning the two axioms of Buckley (1988), will provide useful insights in the time to come. Besides this, it is worth pointing out that internalization and TCE are mainly concerned with comparative efficiency between institutional arrangements; however, as noted among others by Tomassen and Benito (2009) *within* a given governance structure there may still be significant differences in the actual governance costs incurred. The internalization decision thus arguably only tells part of the story about "maximization".

<sup>24</sup> Again, it is useful to think in terms of maximization under constraints. In this case, the constraints include limited resources in terms of cognitive capacity, time for making decisions, etc.

assumptions (Buckley & Casson, 1993). In Fig. 1 below we suggest such a nested view of SRPs where the hard core of the “economic approach” is the assumptions of maximization and equilibrium (Buckley & Casson, 1993), while the derived SRPs of neoclassical economics and TCE respectively combine this assumption with different sets of auxiliary assumptions to form their own hard cores. By deriving progressively more fundamental assumptions, we find nested SRPs appearing as Russian *Matryoshka* dolls.

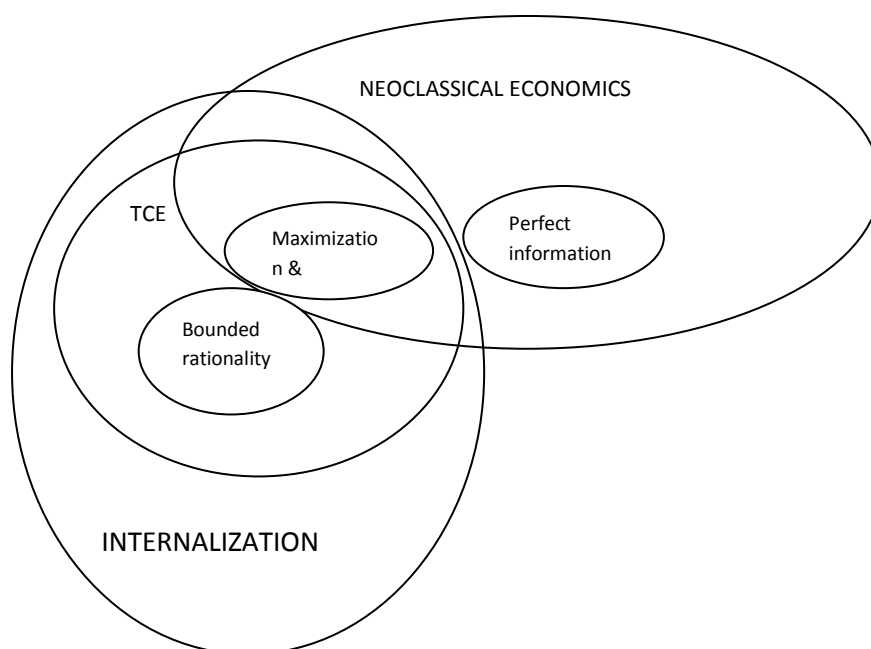


Fig. 1. Nested scientific research programmes in economics.

A discussion of economics-based IB research would not be complete without considering the OLI (Ownership-Location-Internalisation) paradigm developed by John Dunning over several decades (e.g. Dunning, 1980, 1988, 2000; Dunning & Lundan, 2008). Through this paradigm, Dunning attempted to pull together the different insights from the MNE literature into a coherent framework, positing three conditions for an FDI to take place. First, there must be *Ownership advantages* (O), firm-specific advantages that can compensate for the higher costs of operating abroad than at home. Second, there must be *Location advantages* (L) in the host country in question, providing an advantage of production there relative to production in the MNE’s home country. Third, there must be *Internalization advantages* (I) of keeping production within the firm’s boundaries, rather than relying on market transactions. It is obvious that given its generality, the OLI paradigm is difficult to falsify (Pedersen, 2003); and indeed “the OLI paradigm is not, and does not purport to be, a specific and testable theory of

the MNE” (Dunning & Lundan, 2008, p. 120). As Dunning and his associates point out, though the OLI paradigm itself is not testable, it offers a framework within which operationally testable theories can be developed.

The OLI has been interpreted previously both in terms of a “theory”, a “framework” and a “paradigm” proper (Eden, 2003). We here investigate whether it can be given an interpretation in terms of ambitions to be an IB SRP. It might seem too “obvious” to label the work as a Kuhnian “paradigm” or normal science for IB scholars; though Dunning (2000) himself hints at this. It is worth noting, however, that the main “tenets” of the paradigm in terms of the O, L and I–advantages are indeed meant to constitute fundamental assumptions for IB research (they also include a specific reference to the international dimension, which, as we have seen above, is missing in IT). Indeed, although not using these terms, Dunning (2000, p. 169) appears to have a distinction between a hard core and a protective belt of the OLI in mind:

The question at issue, then, is whether the changing character and boundaries of the O specific advantages of firms [see discussion below] can be satisfactorily incorporated into the eclectic paradigm, as it was first put forward. We would argue that as long as they do not undermine the basic tenets of the paradigm, and are not mutually inconsistent, they can be, although most certainly, they do require some modification to existing subparadigms and theories

Here, Dunning in effect proposes that modifications can be made in the “protective belt” of the OLI paradigm, where we find the actual operational theories. Can we also assess the OLI in terms of Lakatos’ criteria for scientific progress? This would essentially involve showing the success of the theories in the protective belt, but building on the core OLI assumptions, in increasing empirical content over time. Though originally intended to explain international production, the OLI has later been extended to cover also other types of activities such as cooperative ventures that do not involve ownership (Pedersen, 2003). On the other hand, one might worry that the OLI framework might risk losing its coherence or internal consistency to the extent that it must be modified to accommodate these new phenomena. This is illustrated by the fact that the OLI has itself evolved over time: Indeed, five different versions or “marks” of the paradigm have been identified (Eden & Dai, 2010; Narula, 2010), which have grown in complexity, even leading some researchers using the OLI to prefer the earlier, simpler versions (Narula, 2010). Thus, Pedersen (2003, p. 20) notes that



The positive side of the change [from considering only international production to covering all value creating activities] is that the eclectic paradigm adds generality when other entry modes than FDI are taken into consideration. The downside is that OLI is assumed to explain just about anything by merely adding an extended set of variables. It seems reasonable to pose the question whether it explains all or nothing.

While some of the changes in the OLI reflect changes in the world economy, others have arisen as a response to critics. One particularly notable criticism has come from internalization scholars arguing the O-component is superfluous. Buckley (1988, p. 182) argues that “if internalization is interpreted dynamically, the inclusion of ownership advantages is double counting”. The argument is that internalizing provides the firm with an advantage (i.e., an owner-specific advantage). Dunning and Lundan (2008, pp. 102-103) accept that from a dynamic perspective, the OLI components may be interdependent, but argue that it is still conceptually useful to consider them separately. In fact, Dunning (2000, p. 167-168) argues that the increasing interdependence of the different components of the paradigm is a strength rather than a weakness: “the value of the eclectic paradigm has increased relative to the sum of its parts, with the contribution of each becoming increasingly interdependent of each other”. Other authors objected that some firms might go abroad precisely in order to obtain certain firm-specific advantages or assets (Mathews, 2002): This was later also acknowledged in the OLI, though Dunning maintained the importance of the assets that the firm possessed prior to internalization (Eden & Dai, 2010).

Whether these and other modifications imply that there are in fact several OLI SRPs can be discussed. We would here, however, like to point out one final interesting characteristic of the OLI, which is its apparent ability to include many different research streams. Specifically, as its hard core assumptions are essentially stated as *conditions* (Boddewyn & Iyer, 1999) rather than as behavioural assumptions like in earlier reconstructions of SRPs in economics (Foss, 1994; Knudsen, 1993), it can accommodate SRPs based, for example, on perfect rationality (neoclassical economics), bounded rationality (transaction cost economics) and rules-based “procedural” rationality (evolutionary economics) (Knudsen, 1993). Indeed, one might argue that the existence of such a framework in IB itself distinguishes it from “representative” economics research (that being said, it should be pointed out that even mainstream economics work on MNEs acknowledges the OLI as a “building block” for their analytical approach; see Barba Navaretti & Venables, 2004, p. 24).

*Modelling and scientific realism in IB research*

We now ask *whether the generally greater focus of IB on descriptive realism implies a greater commitment to scientific realism*. One could pursue two lines of argument. First, it could be argued that the sparser use of mathematical modelling and unrealistic assumptions itself reflects a reluctance to sacrifice empirical detail in order to construct a tractable mathematical model. However as we saw in the discussion of economics, the relationship between descriptive realism and scientific realism is not straightforward, and in any case IB works rarely motivate why mathematical modelling was *not* used. On the other hand, a view of models as idealizations are argued by some to be more closely linked to realism than one of models as pure constructions or “fictions”. Our approach here will therefore be to briefly discuss which of these two views of modelling appears to be predominant when IB scholars actually use mathematical models. Of course, such an analysis should only be regarded as suggestive, as it will largely rely on the language used by the authors. For example, Buckley and Casson’s (1976, pp. 62-65) model of “the growth of the research-intensive firm” is simply one in which their verbal analysis of “the aggressive phase of the research-intensive firm is summarised” (1976, p. 58). Rugman (1981, p. 53) does announce a more ambitious agenda by presenting different models or “methods of determination of the optimal modality” of servicing a given market, seeking to “provide as much information as possible so that a definitive model may emerge later from these pioneering efforts at formalizing the cost conditions affecting each modality.” As the field has matured, mathematical modelling has not diffused widely. However, of the two main categories of models discussed above, the “idealizing” view of models seems to predominate. For example, Buckley and Casson (2001) repeatedly motivate modelling choices with reference to “for simplicity” and the like. Similarly, Asmussen (2007, p. 93) notes that “[w]hile a formal model of the [integration-responsiveness] framework can never replace the rich system description of the existing models, it can single out aspects of that system and show its mechanisms in a more operational way.” Thus, to the extent that the stated “idealized” approach to models can be used as a proxy, we conclude that IB scholars do appear to lean towards scientific realism.

### CONCLUDING REMARKS

This paper has drawn upon the extensive efforts of philosophers of science in economics, to try and give tentative suggestions on the philosophy of science of economics-based IB research. In the spirit of economic analysis, we have made strong simplifications. We have

restricted the focus of the analysis to suit our specific purposes; limiting our discussion to the economics-based IB-program (internalization theory and the OLI paradigm) we have “assumed” away among others the important but elusive question of the boundaries of IB (Toyne & Nigh, 1997a). Nevertheless, through this limited analysis we have arrived at a number of tentative conclusions. First, although there is generally a stronger empirical focus in IB, it appears that like its ancestor, economics-based IB research is better understood in terms of Lakatos’ scientific research programmes (SRPs) than Popper’s falsificationism. Considering the nature of IB SRPs in turn gives some interesting results. First, we have pointed out that existing interpretations of the hard core of internalization theory do not contain an explicit reference to the international context; and that the theory can essentially be traced back to a more general economics paradigm, via modified sub-programmes such as transaction cost economics. On the other hand, discussing the currently “dominant analytical framework” (Dunning, 2000, p. 163), namely Dunning’s OLI paradigm, we identified some features which justifies an interpretation in terms of ambitions to be an IB SRP; such as a hard core consisting of core tenets (and also making explicit reference to the international context), and the suggestion that the paradigm can accommodate essentially most international features through an appropriate modification of the protective belt in terms of the specific theories accommodated within the paradigm. Another characteristic feature of the OLI paradigm is the openness to incorporate sub-SRPs based on different behavioural assumptions such as perfect rationality (neoclassical economics), bounded rationality (transaction cost economics) and rules-based “procedural” rationality (evolutionary economics) (Knudsen, 1993). Finally, exploring the link between (mathematical) models, unrealistic assumptions and scientific realism, we have suggested that the approach to models of IB scholars in most cases implicitly reflects a commitment to scientific realism.

As for much economic analysis, a challenge is now to broaden our perspective and consider what the analysis has not captured. There are also other issues in the philosophy of science of economics which might be of interest for IB scholars besides those we have treated here. One such issue is the importance of quantitative statistical analysis in economics, and in IB. While we have here focussed on a relatively narrow set of economics-based studies, quantitative statistical analysis predominates in IB work more generally (Doz, 2011). Further work might for example consider the idea that qualitative analysis is “ideal” for falsificationism (Welch, Piekkari, Plakoyiannaki, & Paavilainen-Mäntymäki, 2010). Another important question we

have neglected here is the debate on positive versus normative economics; i.e. whether it is possible to have a “value-free” economics (Blaug, 1980).

Another topic which we have not treated, but which may become increasingly important in the philosophy of economics, is the implications of behavioural economics (Angner & Loewenstein, 2012), which has theorized and described a number of departures from standard assumptions on behaviour. One strand of research has demonstrated the importance of social preferences in terms of altruism and reciprocal behaviour, for example (Rabin, 1993). A second strand describes how actual decisions under uncertainty deviate from those prescribed by subjective expected utility theory. For example, *rank-dependent utility* implies that the attention given to an outcome depends not only on the probability of the outcome, but also on how favourable the outcome is compared to other outcomes (Diecidue & Wakker, 2001); while *prospect theory* holds that agents are *loss averse* as compared to risk averse (Kahneman & Tversky, 1979). Mainstream economics has long resisted the inclusion of such features in economic analysis, arguing that this would dilute the distinct strength of economics based on “rationality”. One may ask whether incorporating behavioural features strikes economics as its “Lakatosian core” of rationality, and whether behavioural economics is in this sense a new economics SRP. We limit ourselves to pointing out that “maximization” may be a more fungible concept than appears at first sight. Though relaxing assumptions on rationality, transaction cost economics still assumes “intended rationality” and in particular a sufficient foresight of potential transaction cost issues leading to the design of efficient governance structures (Williamson, 1995). Similarly, incorporating behavioural features does not necessarily imply “irrationality”: The usefulness, in general, of the various heuristics as described above becomes more plausible when we allow that both mind and time are scarce resources in decision situations; cf. the idea of “maximization subject to constraints”.

The debates presented here may also be useful in considering the future agenda of IB. For example, they may be useful for thinking about such issues as increasing the descriptive realism of IB research. Descriptive realism can be extended in various ways. One is greater realism in terms of the phenomena one seeks to explain. For example, some authors (Asmussen, et al., 2009; Benito, Petersen, & Welch, 2009) have called for recognition of the fact that actual foreign operation modes are more complex than most research assumes, and that the focus on specific mutually exclusive modes “stands in contrast to the variety of combined entry modes that can be observed in real-world firms” (Hashai, et al., 2010, p. 660). Extending existing theory (in particular internalization theory) to these relatively overlooked

aspects can also be seen as a way of further testing the theory. Other authors have called for greater realism in the assumptions regarding the explanatory factors. For example, Brouthers and Hennart (2007, p. 419) argue that

the current focus on rational models of mode choice needs to be supplemented with more realistic [strategic decision making] perspectives that take into account the actions, beliefs, and attitudes of the managers who actually make these decisions and those external factors that influence managerial choices.

As noted above, Friedman (1953/2008) argued that we should be concerned with the accuracy of predictions rather than greater descriptive realism in assumptions. However, incorporating so-called “behavioural features” has often led to improved predictions (Holden, 2012). Perhaps behavioural economics will in the years to come be one factor promoting the integration of the rational economics-based tradition and the “behavioural” tradition in IB.<sup>25</sup> So far, the dialogue between these streams has been relatively limited. However, some authors argue that the developments in behavioural economics may also affect the future application of economics to IB (Aharoni, et al., 2011); FDI studies utilizing these insights have already started to appear (Pinheiro-Alves, 2011).

Though the aim of this paper has mainly been to investigate the philosophy of science implications of IB economics research practice, we would like to end on a more normative note. While there are limitations in principle to the practice of falsificationism, it is clear that IB researchers could in many cases do more to really put theories to the test. We have already mentioned the failure of many TCE-based studies to test the crucial interaction effect between uncertainty and asset specificity predicted by Williamson (1985) (and the failure of those that do so to problematize a lacking effect), but other examples could also be given. On the other hand, one welcome development in this respect is an increasing number of studies that attempt to combine different theoretical perspectives (e.g. evolutionary economics and transaction cost economics, both of which can be seen as SRPs in their own right). In these cases researchers may need to think about how they are setting these perspectives up against other or using them in a complementary manner. Interestingly, concepts from philosophy of science may also be helpful here; for example, identifying Lakatosian hard core assumptions of the different perspectives one attempts to combine could indicate whether there are

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<sup>25</sup> Though as Angner and Loewenstein (2012) point out, the relationship between older work such as that of Simon and the behavioural theory of the firm and modern behavioural economics is not straightforward.

inconsistencies in combining the theories (e.g. in terms of behavioural assumptions), and what are the implications of this.<sup>26</sup>

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<sup>26</sup> The author has discussed this latter idea in some more detail elsewhere (Rygh, 2013).

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