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Comments from the Editor

Tamir Agmon
Editor

For the last five years I have had the privilege of editing *AIB Insights*. It has been a privilege for a number of reasons; first, through the contribution of many members, I have encountered the scholarly work of many International Business researchers that I would not have encountered otherwise. Second, it gave me the opportunity to approach scholars in other fields like history, psychology, and political science and ask them to contribute their work to the publication. In the segmented world of research it gave me and the readers of *AIB Insights* real insights into the scope of international business as seen by people who come from a different disciplines and research traditions. But the most valuable privilege for me was that I could do what I want "out of the box". As a graduate of a fairly conservative economic research oriented institution (the Graduate School of Business at the University of Chicago) and as a person who has been engaged in applied economic research for more than 30 years, I found it invigorating to seek out and publish what I thought was right in the research of International Business.

The 16 issues of *AIB Insights* with more than 45 articles by a wide variety of veteran and young researchers ranging in academic association and location all over the world present a double image: an image of the really multidisciplinary nature of research in International Business and a no less multicultural, multidisciplinary and simply fascinating image of a group of people who do research in an original and passionate way.

The current issue, the last that I have the privilege to edit, is in some way a closing and a beginning. The three contributions by Jean Boddewyn and a group of AIB Fellows, by Christos Pitelis, and by me look at the interrelated question of the changes of the global environment and the future of research in International Business. One thing is certain: the world is changing, and the post-crisis world will differ from the world as we knew it before. For me personally this is a closing of more than five years in which *AIB Insights* was an important part of my professional life. In closing the period of being an editor I took the liberty of contributing an article for this last issue.

Research is both a personal engagement and a group activity. In many ways I have served in the last five years as a conduit for original and exciting research in International Business. Looking back at the array of several scores of personal contributions I hope that an interesting, complex, varied but yet complete picture of what International Business research is arises. For this I am thankful for all who have contributed to *AIB Insights* and made my work rewarding and interesting.

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IB at 50 – A Semi-Celebration, Challenges and Ways Ahead

I. Introduction

My aim in this "Insight" is to re-visit IB's quasi-existential drama and provide reasons for celebrating its achievements on occasion of its 50th birthday (Section II) as well as to discuss some old and new challenges and possible ways forward (Section III). Section IV provides concluding remarks.

II. An IB Paradox: Existential Drama and Reasons for Celebration

Despite early interest in issues pertaining to international production, FDI and the MNE, see for example Penrose (1956), Dunning (1958), and Jones (2000) for a historical account, it was Hymer (1960/1976), that helped found IB as a new field (see Dunning and Pitelis, 2008). Hymer claimed that the pursuit of profits by growing firms established in developed nations will eventually motivate them to undertake 'foreign operations' such as exports, licensing, franchising and FDI. All these modalities had their own advantages and disadvantages, and FDI was superior in terms of the control it afforded to the firms. This superior control allowed firms to reduce international Rivalry (R) and exploit better their monopolistic Advantages (A), by leveraging them in-house, instead of using the open market.

The benefits from in-house leveraging of advantages related to factors such as fear of appropriation by rivals, the high costs of market transactions, the speed and efficiency of intra-firm (versus inter-firm) transfer of advantages which had the characteristic of a 'public good' (non excludability), and/or involved tacit knowledge (see Pitelis, 2002). FDI also had the benefit of risk diversification (D). Overall the Rivalry reduction, Advantages, and risk Diversification (RAD) benefits of FDI explained both its existence (and thus the MNE) and why MNEs were able to out-compete locally-based rivals in foreign countries, despite the inherent disadvantages of being foreign (Hymer, 1976, p46).

Subsequent developments in the theory of FDI and the MNE built on the ideas of Hymer, but focussed almost exclusively on the A aspects of his work. In particular, the classic contributions of Buckley and Casson (1976), Rugman (1980), Teece (1977), Williamson (1981), Dunning (1998) and Kogut and Zander (1993) explored the reasons why intra-firm exploitation of advantages may be preferable to inter-firm (market) ones. The R has been downgraded, except in works such as Vernon (1966, 1979) and Graham (1990). The D has not been very influential, partly because of a widespread idea that shareholders can diversify risk themselves, therefore there is no need for firms as such to do this (see for example Porter, 1987, but see also Delios and Beamish, 1999).

In terms of the explanandum, 'internalisation' scholars focussed on explicating FDI and the MNE. Vernon's 'Product-life-cycle' approach and Dunning's OLI, instead, purported to explain international production. The last mentioned involves broader considerations than the internalisation of advantages, hence Dunning's focus on location and Vernon's emphasis on inter-firm rivalry, intra- and inter- nationally. In addition to the above, Vernon's two variants (1966, 1979) involve an element of dynamics, as he aimed to explain the process of internationalisation. This was mostly absent in internalisation theories, as well as the OLI. Subsequently, Dunning aimed to rectify this, by developing the concept of the investment development path (see Cantwell, 2000). A stages approach has been developed by the Scandinavian school (see for example, Steen and Liesch, 2007), which explains the choice of location by MNEs partly in terms of familiarity and 'psychic distance' of markets. More recently, the idea of the 'liability of foreignness' emerged as an important sub-theme of IB scholarship (see Zaheer, 1995).

The issues of knowledge and learning were addressed by Hymer (1972), Buckley and Casson (1976) and Kogut and Zander (1993). They became more popular following the emergence of the resource-based-view (RBV) and knowledge-based views of the firm; see Penrose (1959), Teece (1977, 1982), Wernerfelt (1984), Barney (1991), Peteraf (1993), Foss (1996) and Mahoney (2005) for a critical survey. RBV and learning-based-ideas have been used to provide more dynamic interpretations and updating of Dunning's OLI as an envelope of other theories (see for example Dunning, 2001; Pitelis, 2007), as well as to explore linkages between theories, such as Penrose and the Scandinavian approach (see Steen and Liesch, 2007). Recent interest in institutions and development, moreover, led to cross-fertilisation between IB and development economics (see Dunning, 2006). Much more has taken place, not least work on heterarchies (Hedlund, 1986) and leveraging subsidiary skills (see Birkinshaw and Hood, 1998).

Despite this impressive journey, it is arguable that there is little specifically 'I' about 'IB'. All three elements of Hymer's triad, R, A and D, apply equally well to diversified firms within a nation. This is true concerning Hymer's own explanation as to why FDI, and Hymer's and subsequent 'internalisation'-type theories. It is not even clear whether the term 'internalisation' itself adds much to the more established and widely used term 'integration' at least when applied to firms (see Boddewyn, 2008, for a different, wider application of the term). The ideas of Buckley and Casson, Williamson, and Kogut and Zander apply equally well to intra-national diversified firms as to inter-nationally diversified ones. The same is true for more general theories, such as Dunning's OLI. In OLI, for example, O, L and I apply equally well at the N-level as to the I-level. To drive the point home, there is little specifically I in the literature of the theory of FDI and the MNE (see Pitelis and Boddewyn, 2009).

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Unique about 'F' and 'MN' is the existence of Borders, the existence of different sovereign Nations, all with a legal monopoly, of violence over their legal subjects (individuals and firms), to include the legal monopoly to TAX them (see North, 1994). Accordingly, a specifically I theory of IB needs to explore the differential costs and benefits of the existence of different sovereign legal jurisdictions, or, differently put, the scope to leverage any net benefits from the absence of flatness, or the presence of semi-integration (Ghemawat, 2007).

Our provocation and challenge above should not make one forget that IB as a field has good reason to celebrate its achievements. Hymer's RAD framework introduced explicitly for the very first time in the literature the concept and importance of profiting from firm Advantages, and differential benefits from exploiting them intra-firm versus inter-firm. The answers Hymer provided included the very first post-Coasean transaction-costs-analysis (Hymer, 1968) well before Williamson (1975). The work of Buckley and Casson (1976) on the

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'public goods' nature of knowledge, which engenders 'internalisation,' was a momentous insight that helps inform not just the MNE and FDI, but also intra-national integration/internalisation strategies of firms. The same is true for the works of Kogut and Zander (1993). Dunning's pursuit and generalisation of the Advantages thesis is path breaking, not just for IB. Today, general management scholars move increasingly towards the appreciation of the profiting from Advantages approach that Hymer established and Dunning developed and completed – see Teece (1986), Research Policy (2006), Academy of Management Review (2007), especially Lepak et al. (2007) and Pitelis and Teece (2009).

The above are few instances where IB scholarship has led general (strategic) management, rather than simply applying extant concepts to issues with an I focus. This raises the question whether the I focus is of importance in developing ideas, which may well end up being of more general use and applicability, but which might well have not appeared, delayed to appear, or remained underconceptualised, in the absence of the I focus of IB scholars. The answer is hard, as it involves a counterfactual. However, we feel it is likely to be in the affirmative. Dealing with inter-national firms helps sharpen focus and zeroing-down on essential issues which can be lost in the context of a national focus. While a US firm can license in other US firms too, at the time of Hymer's writing licensing by American firms to Japanese ones, and/or undertaking FDI instead in Europe, were far more pressing and visible issues than licensing to a US firm. The nature of knowledge (whether a public good tacit, or something else) is arguably more pressing when dealing with overseas operations, not least because the downside risk of failure can be higher than in intra-national investments. Leveraging subsidiary skills is more pressing for an MNE rather than for a (M-form) national firm.

III. Some Challenges and a Way Forward

In order to develop the I part of IB as a determinant of FDI, the MNE and IP, more resources need to be leveraged on the issues of national business cycles; different stages in national development; different macroeconomic, institutional regulatory and cultural regimes. All these are under-researched. We are aware of no published econometric test of the impact of the business cycle on FDI. The possibility of 'decoupling' (that is that emerging markets now gradually sever their links with developed markets and therefore may manage to offset economic downturns in the developed world), see Akin and Kose (2008), is extremely topical among financial economists, practitioners and journalists; see *The Economist* (2008, pp. 91-92). We are aware of no econometric analysis of the impact of decoupling on FDI flows, despite an increasing awareness of the potential role of emerging markets on FDI; see Paul and Wooster (2008). The role of different regulatory contexts on FDI so far concerns economists more than IB scholars; see Culem (1988) and Wheeler and Mody (1992). Much more work is needed on these fronts to help delineate and sharpen the distinction between B and IB. Such work can build, among others, on the contributions or the liability of foreignness literature, uneven development (Eden, 1991), Bartlett and Ghoshal's (1989) integration/national adaptation contribution, Dunning's work on the investment development cycle, the risk diversification afforded by investing in different countries (Jones, 2000), with its related links to finance (Agmon, 2006), existing literature on institutional, cultural and regulatory differences among nations (Pitelis, 2009).

In addition to delineating the I part, there exist significant IB innovations that require improved conceptual foundations to be better appreciated and integrated within IB scholarship: the move from internalisation to externalisation and their combined use; the move from closed to open innovation by MNEs and/or the combination of the two; the leveraging of the advantages of others; foreignness as an asset, not a liability; MNEs as 'global optimizers' and orchestrators of the global wealth creation process; and tensions between global value capture and the sustainability of the global wealth creation process and the challenge of 'global governance.'

US firms grew through integration. That was the world described by Chandler (1962), Penrose (1959), Schumpeter (1942) and Hymer (1976). Hymer predicted externalisation through subcontracting, but externalisation and outsourcing only acquired significance in the past fifteen years or so (see Teece, 2006a). There is nothing inevitable about growth through integration. Firms can grow by combining integration with dis-integration, internalisation with externalisation, specialisation with diversification (see Jones, 2000). We need a better appreciation on the role of I in this context. For example, could it be that increased global integration helps engender specialisation alongside the outsourcing of some activities? Which activities do (should) firms externalize and which ones should (do) they keep in-house?

One major activity that firms, especially MNEs, used to internalise is R&D. These days many firms move to open innovation, or combine 'closed' with 'open' innovation (see Chesbrough, 2003). Often this involves keeping sufficient in-house R&D to create the 'absorptive capacity' to identify (or even develop) 'open' innovation opportunities created by others, or in collaboration with others (such as universities) that can be captured by the MNEs (see *Research Policy*, 2006; Panagopoulos and Pitelis, 2009). Can IB scholarship help us understand this better? In particular, does being an MNE help explicate the move from closed to open innovation, or their combined use? Hymer and the subsequent literature on the Liability of Foreignness usually underplay the possibility of foreignness being an asset. Foreignness can be an asset per-se, when foreign is perceived as novel or better (even when it is not); when it can allow one to be forgiven for making errors (not allowed to locals); when it can provide a requisite 'distance' that can help morals become more loose, allowing some more grey acts than what a local politician, for example, might be prepared to consummate with a local firm-entrepreneur. Firms could try over time, moreover, to turn foreignness from a liability to an asset. These issues need much more exploration than currently available in IB.

In trying to capture value from their value-creating advantages, but also those of others, MNEs become increasingly more alert to the systemic benefits of overall value creation. They can help the creation of value by funding universities, collaborating with rivals, encouraging their employees to set-up their own firms (sometimes competitors), and helping competitors to innovate. Large companies, like Siemens and Microsoft, do this. Many others, like IBM and Apple, focus on their complementary integration, design and marketing capabilities to package extant knowledge in attractive new products. Gradually, MNEs tend to become orchestrators of the global value creation process—a role traditionally being the prerogative of nation states. This can be good and a challenge. Good, because it makes MNEs interested in global value creation, so as to capture as large a part of it as possible. A challenge, because value capture may undermine the sustainability of global wealth creation (see Mahoney et al., 2009).

An easy way to visualize this possibility is by considering the world as fully integrated-flat. In such a world any restrictive practices by large firms would tend to lead to monopolistic imperfections, in terms of allocative and intertemporal efficiency (see Baumol, 1991). If large firms are tempted to pursue such practices in order to capture value, and if nation states try to help them through strategic trade policies and protectionism, this would tend to undermine global efficiency and wealth creation in a similar way. Time inconsistency issues may have similar outcomes, even in the absence of conflicting interests. It may not be advisable to rely on enlightened interest by MNEs, especially given the 'free-rider' problem (see Olson, 1971). In this context, the issue of how to institute global governance structures which aim at enhancing the sustainability of global wealth creation is

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critical. Some work in this direction has been done by Moran and Ghoshal (1999). More is needed on this front, especially in an IB context.

The question of how MNEs may internalise the potentially harmful externalities of some of their operations and the type of global governance most conductive to the sustainability of the global value creation process is in our mind the single most important issue on which future IB scholarship should focus. By its nature this requires more IPE-related competence than is currently available among IB scholars.

IV. Concluding Remarks

IB is currently more B than I. Despite this, or because of it, IB scholars made very important generic contributions which might not have taken place were they not working in an I context; extant IB theories and envelopes require better delineation of the I; current IB competences may fall short of the requisite task and that a shift of focus is required from B to I-related competences, notably IPE.

The above is strengthened when one considers other innovations in IB practice, theory and strategy, such as a combined approach to internalisation and externalisation; a combination of closed and open innovation; the potential benefits of foreignness; the fact that the ability of MNEs to capture value from the advantages of others may turn them gradually into orchestrators of the global value creation process. Yet, tensions between value capture and value creation may undermine the global value creation process, rendering in our view the question of how to effect best practice global governance for sustainability the single most critical concern for the future of IB scholarship.

The policy challenge for MNEs and government policy matters alike is to identify ways through which mutual long-term benefit can be effected through sustainability. For MNEs this would involve the avoidance of restrictive practices; for policy makers the setting-up of institutions, regulations and policies that promote innovation and sustainability. These could include international organisation(s) with sustainability at its/ their core agenda.

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International Business in a Time of a Crisis: A New Paradigm for Research in the Economics of International Business

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The crisis as a "big question"

In a paper published in JIBS in 2004, Buckley and Ghauri argue that international business research succeeds when it responds to the need to answer a series of what they call "big questions" in the world economy. They proceed to say that such a "big question" is the changing strategy of multinational enterprises (MNEs) and the way it affects globalization and its geographical expression of through the location of the activities of MNEs. Buckley and Ghauri then review the literature linking ownership and location strategies to economic geography and theories of globalization to explore new areas of research in international business.

In this brief article I follow Buckley and Ghauri in the basic premise that research in the economics of international business depends on defining a "big question" and applying an economic model to gain insights into possible answers to the question. The current global financial and economic crisis is such a "big question". The current financial and economic crisis is a global phenomenon in its roots, its expressions, and in its potential resolution. Therefore it is a uniquely appropriate topic for research in the economics of international business. Moreover, the processes that have led to the crisis and the nature of the crisis will affect the future research in international business in as profound a way as the rise of the MNE has affected the development and the method of the research in international business since its beginning in the 1960's.

Following "big questions" was the way that research in the economics of international business developed from the beginning. The research in the economics of international business in the US began in the late 1950's with the question why do US firms like Ford Motor Company invest in acquiring companies outside the US and setting up manufacturing operations rather than exporting from the US? In order to answer this "big question" economists like Hymer, Kindleberger, Caves, and others have used models of industrial organization that are still the basis of what is known as Dunning's OLI model of international business. The well-known internalization model of Buckley and Casson is another successful attempt to deal with the "big question" of globalization and the famous "horse race" between markets and organizations (firms) as conduits of international trade in goods, services, and factors of production.

The nature of the questions asked implicitly and explicitly by the research in the economics of international business since the 1960's and all through the second half of the 20th century led researchers to use models that look at the problems from a North-South perspective. The focus of the research was on the multinational enterprise (MNE). For most of the second half of the 20th century the MNE was an organization dominated by owners and managers of the North, the developed countries. The MNE provides a very efficient conduit between the North and the South countries. It supports changes in competitive advantage between the MNE in the North and its suppliers in the South through the well-known process of outsourcing. The developments at the corporate level generate changes in comparative advantage among countries that change the balance between North and

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South. Foreign direct investment, a cornerstone of international business, was partially replaced by what can be called "financial foreign direct investment" (FFDI) where private equity mega buyout funds and other financial intermediaries replaced the traditional MNEs as conduits for international business. In this world the North-based MNE lost its dominance over international business, and it is in the process of being replaced by other business organizations. A fascinating question for research is who is going to replace the MNE of the 20th century as the leading power of international business? The interest in this question is heightened due to the current crisis. A crisis is always an opportunity for a change. It is reasonable to expect a change in the nature of international business as a result of the current crisis and its resolution.

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To present and discuss the change and its implication for current and future research in the economics of international business a simplified description of the developments in international business and how they contribute to the crisis is presented. This is followed by a discussion how we as researchers in international business can gain insights into the nature of the change using the economic tool box of research.

What went wrong is the success

The reason for the current crisis was an overvaluation of current assets based on inflationary expectations regarding future cash flows. This statement makes sense in the context of financial economics. In their seminal 1958 paper on the cost of capital, Modigliani and Miller defined the value of an asset as the discounted present value of the cash flows to be derived from this asset in the future. It follows that value is anchored in the expectations for the future. As the expectations for the future rise and everybody in the market expects future cash flows to increase, so does the current value of assets. This is the reason why lenders were willing to lend more than 100% of the current price of a house to sub-prime lenders. In the world of international business the same process means that the value of companies (assets) in emerging markets (South) went up, and consumption (and production) went up. To explain this process and how it led to the crisis consider the following extremely simplified example. Suppose that we have a manufacturing company in China that produces consumption goods as a supplier of a US-based MNE that adds services to the goods. There are two groups of people: Americans who are employed by the MNE in the US and Chinese who are employed by the Chinese supplier. The American and the Chinese are making consumption decisions, how much to save and how much to consume. They save by investing in the liabilities shares and bonds of the US MNE and/or the Chinese supplier. It is assumed that Americans have higher past savings (investment) than Chinese. Assume further a two-period world and that the only goods and services are those produced by the MNE and its Chinese supplier. At the first period there are certain expectations concerning both the future cash flows from the assets of the US and the Chinese companies and their associated risk. This determines the value of the assets and the liabilities of the two companies. The liabilities include the current value of wages and the return to the various financial assets like bonds and shares issued by the two companies. As all the people, American and Chinese, hold all the liabilities issued by the two companies and consume all the consumption goods and services manufactured by the two companies, we have a close system at equilibrium. As the Americans have higher past savings they are consuming more of their current value than the Chinese. Now assume that the expectations for an increase in future cash flows from the assets were proven right and as a result the people have revised the risk of the future cash flows from the US and the Chinese assets downward. This will increase the current value of the assets held by both Americans and Chinese and therefore they will increase their current consumption, the Americans more than the Chinese, which will drive the value of the asset that produces the consumption goods and services even higher. We as a profession do not know when this process will reverse itself. What we do know is that it cannot continue forever, and historically it always ends with a crisis of some sort. In addition, as long as the process continues, the balance of value is changing between the Americans (North) and the Chinese (South) as well as between the US MNE and the Chinese supplier. This is so because the Americans in this example consumed more based on the current value of their assets that went up because the inflated expectations. Therefore they are affected more by the global crisis. The use of the terms "American" and "Chinese" implies that different people in different countries behave differently. Different cultures, governance systems, and economic policies between Americans and Chinese make even this simplified model complex.

The success of the South companies in increasing production and moving into higher added value activities, the success of American households in revising the value of their assets upward and the following increase in their consumption of goods from the Chinese company (and the US MNE), and the increase in the value of the assets and therefore the liabilities of the Chinese company created a bubble in the market and change the equilibrium between the North and the South and the major processes of international business as they were discussed and presented in the research in the economics of international business since the 1960's. Admittedly, the current financial and economic crisis is in its beginning, and we do not know when and how it will be resolved. Yet, it is a safe bet to say that it will change the global world and the relations between the developed (North) countries and the emerging market (South) countries.

Useful economic toolbox

As was pointed out earlier, the nature of the "big questions" of international business as a subject of research led the profession to adopt models of Industrial Organization in the US in the 1960's and the 1970's, to give them an English interpretation in the 1970's and the 1980's and to move into strategy in the 1990's and beyond. The focus was on the MNE, the North MNE during most of the time with an increasing interest in the South MNEs in recent years. I think that the current crisis and the changes in the nature of the world of international business that will follow require a different economic tool box.

The crisis and its roots point to three major areas where most of the action is taking place. These are the role of expectations regarding the expected value and the risk of future cash flows, the role of the government as a lender of last resort and as the responsible agency in an economy (maybe representing the future generations), and the role of corporate and institutional governance. Economists developed research methods in all three areas. Financial economics deals with the first area, development economics and to some extent international trade deal with the second area, and the relatively new literature on corporate governance deals with issues of regulation and other aspects of corporate and institutional governance.

It is an irony of fate that among the major victims of the 2008 crisis are the big 3 US auto companies who were at the root of the beginning of the research in the economics of international business. In a more general way the crisis is likely to change the focus in the research in the economics of international business from the MNE as the driver of international business to a combination of government and firm as dual drivers of the world economy. The decline in the value of the US vis-à-vis the emerging markets will make the world view of international business more balanced, and the cost of excessive greed will swing the pendulum back to more regulation and government intervention. This "big question" will change the way that we do research in the economics of international business.

We are living in a time of massive changes. The financial crisis and the economic recession that began in 2008 were

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the painful end of a process that began at the beginning of the 21st century. Like in other cases in history the crisis followed a period of boom. The subprime lending in the US was an extreme case of high current consumption based on the assumption that future income, including the implicit income from the increase in the value of the homes that were the basis for the mortgage, will continue to go up. The expectations for an ever increasing future income were partially based on a global market where a growing part of the production and the savings come from the emerging markets, China in particular. This process makes it possible for both increasing consumption and for the expectations for an increase in the value of assets in the developed economies. The relation between the increased consumption and the high level of production and savings in the emerging markets is rather straight forward. The effect on the expectations for increased value for assets requires an explanation.

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Six AIB Fellows in Search of the Future of IB Research

Nancy J. Adler
Jean J. Boddewyn
Farok J. Contractor
Yair Aharoni
John H. Dunning
Stefanie A. Lenway

It was two years in the making, based on a suggestion by Alan Rugman (recent AIB President) to Jean Boddewyn, then Dean of the AIB Fellows (2005-2008), who enlisted 23 Fellows to write or co-write a chapter and 35 of them to review the chapter drafts. And so, we now have a new book on *International Business Scholarship: AIB Fellows on the First 50 Years and Beyond* published by the Emerald Group Publishing[1] in 2008 and aimed at celebrating the first 50 years of the Academy of International Business and guiding its future research paths.

Six of the 23 AIB Fellows writers were invited to present their views about *the future of IB research,* based on their chapter but ranging well beyond it, at a session at the 2008 AIB conference in Milan. Individual presentations were followed by mini-group discussions that generated much interactive buzz at the invaluable suggestion of Nancy Adler, one of the two co-chairs (with Jean Boddewyn) of the session. What did the authors say that generated this productive interaction?

Nancy J. Adler (McGill University) reminded us that our primary role is to support scholarship that addresses the complex questions that matter most to society by marrying rigor with relevance (Adler, 2008; Adler & Harzing, 2009) **Could it be that, with the right mix of innovation and entrepreneurship, the creation of knowledge of sustainable value will be the business opportunity of the 21st century?** As former UN Secretary-General Kofi Annan said in 1999 at the World Economic Forum in Davos: "Let us choose to unite the power of markets with the strength of universal ideas…Let us choose to reconcile the creative forces of private enterprise with the needs of the disadvantaged and the requirements of future generations!"

Do we believe that rising to Kofi Annan's challenge is possible—not just for some companies but for business in general (Adler, 2006)? As international-management scholars and educators, do we believe that we have a crucial role to play in shaping society's future, its success or demise? As we face the array of world crises—including poverty, war, ecological devastation and social injustice—do we believe that what we do really matters? In this regard, former U.S. Secretary of State Madeleine Albright stated that each of us has "a responsibility in our time, as others have had in theirs, not to be prisoners of history but to shape history." **The very nature of the discussions in which our IB field is now engaged is to embrace the ways each of us, individually and collectively, can choose to shape history – the ways in which each of us has chosen to take responsibility for the world.**

Therefore, I urge all of us to focus on research issues and questions that matter—such as: How can more companies do well by doing good? How can the best practices—literally, the positive deviants in the field—be scaled up to create epidemics of best practice? How can we involve ourselves in anticipatory scholarship—that is, studying the behavior that executives and companies aspire to, even before they achieve such behavior? How can the IB field develop and support the types of leadership that 21st century society and business most need?

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Jean J. Boddewyn (Baruch College, CUNY) stressed that IB researchers must sharpen and sustain their competitive advantages vis-à-vis economists and strategists by selecting research topics where their distinctive expertise truly lies.

Most of our competition comes from researchers who test **universal** theories applied to cross-border samples rather than to domestic ones—for example, the proposition that "The greater the cultural differences between countries, the greater the decentralization of decision-making" does *not* a truly address an **international** problem because it could as well have been tested between two U.S. states (say, California and Mississippi). Therefore, *it is not enough to include two or more countries to make a study "international" because the determining factor should be nature of the key variables rather than the research locales themselves.*

Instead, **a truly international topic has dependent and independent variables that are both distinctively international**—for example, that "The existence, volume and forms of cross-border direct investment (the international dependent variable) are functions of the transaction costs related to foreign culture and regulation (the international independent variables)."

Conducting this second type of research is much more difficult because it requires a profound understanding of both types of variables—something that amateur IB researchers do not have and would find it hard to get. In other words, **IB researchers need to know not only dominant theories such as transaction-cost economics—something our competitors also know—but mainly foreign environments in depth** (for example, the local cultures and regulations in the above example).

This competitive advantage of IB research requires major time and money investments to acquire foreign knowledge through readings, visits and interviews of locals and international experts—but how else can we beat the growing legion of economists, strategists, sociologists, psychologists, etc. who claim "international" research competence when, in fact they are "universalists" in disguise?

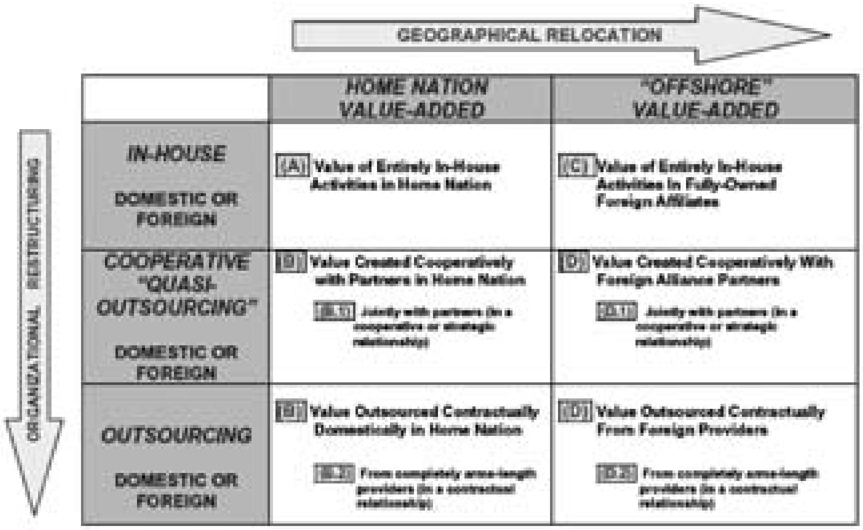
Farok J. Contractor (Rutgers University, Newark and New Brunswick) emphasized the historical evolution of interpersonal and (more recently) interfirm cooperation as mankind's necessary path toward a global civilization now manifested through in-country and between-country **alliances** because the technological, political, economic, spatial and cultural complexities of global operations are simply too great in some areas to be handled by a single company. Therefore, **cooperation is mankind's manifest destiny and will constitute a major thrust of future IB research through the spatial and organizational disaggregation of the value chain.**

We are still in the middle of an economic transformation where the value chain is being disaggregated and spread over several locations and nations, and under a multiplicity of organizational arrangements. Historically, we have seen a shift from a single producer or craftsman to the specialization of labor under hierarchical direction and under a single roof (*à la* Adam Smith's pin factory) to the sourcing of components or materials from other locations in the same country, and to the relocation of portions of the value chain in foreign nations.

The outward spatial or geographical fragmentation of the value chain has also been accompanied by an organization reconfiguration whereby the firm adds value internally only to selected portions of its value chain while leaving other selected bits of this chain (and support services) to external providers. The latter, in turn, are linked to the focal firm in a spectrum of organizational relationships ranging from highly cooperative or relational to mostly contractual or arm's-length ones.

Therefore, the overarching strategic questions for a firm are: (1) how finely do we wish to slice the value chain—that is, into how many pieces or slices, and (2) how do we wish to spread these pieces over the six categories A, C, B.1, B.2, D.1 and D.2 in the figure below?

Consequently, designing and managing this joint organizational and spatial dispersion of the multinational company is the central issue in the practice of



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international management and in our teaching of the subject.

The decision factors—that is, to determine which piece of the value chain will be performed where and under what organizational arrangements—go beyond the well-worn arguments of transaction-costs theory or of simply country-location determinants to include dynamic capabilities, knowledge management, network theory and an emerging theory of optimal alliance-governance structure. Therefore, major research opportunities lie in tracing the decision factors within companies that: (1) determine the optimum division of the value chain (that is, "What is the optimum number of discrete strategic pieces for our operation's value chain?"), and (2) ascertain the allocation and location of each piece (a) geographically (that is, "located in which nation?") and (b) organizationally (that is, which organizational form to use, ranging from "in-house" to "quasi-outsourced" and to "outsourced" on an arm's-length or contractual basis?).

For **Yair Aharoni** (Tel-Aviv University), it is impossible to predict the future of IB research, but one can highlight what IB scholars are expected to do. Thus, IB theory is about how firms behave and how their managers decide as they operate within different local environments. If we have learned anything from the experience of the last fifty years it is that all these factors change and at a faster rate. **Therefore, we should assume even more changes at even a faster rate in the next fifty years, and some of these changes may lead to fundamental shifts in the relationships between governments and MNEs.**

Take the MNE. Originally, most firms were not operating outside their home market and only very few were multinationals. Many of them came from Europe, then from the United States and Japan. Today, there are nearly 80,000 of these firms, and many of them have their headquarters in developing countries. These firms do not necessarily transfer technology from HQ but often absorb knowledge developed within a network. They attempt to develop a unique strategy against other firms so that it is often futile to study large samples with sophisticated statistical-analysis methods since most successes are based on the exploitation of firm-specific advantages that are generally hidden when one studies a large population of, say, alliances. Moreover, in the next fifty years, because of fundamental changes in the environment and accelerated rates of learning within firms, many theories may well become obsolete.

There have been fundamental changes in the population of MNEs, in the strategies of these firms and in their behaviors. Therefore, the firms we study should not be only the largest ones nor should they be only from the developed world. Besides, it is of great importance to define clearly the population from which results are assembled

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as well as all the terms used. Indeed, much of the debates today (e.g., on globalization vs. regionalization) boil down to differences in definitions. Therefore I urge researchers to be very clear about the population they study.

I further recommend studies that would reveal how firms from different countries, including developing ones, are becoming MNEs. Another important research topic is how small firms become MNEs as well as the impediments to becoming multinationals (as in the case of national airlines). Furthermore, much more study and insight are needed on the intersection of markets and national interests as in the case of military and military-related trade, culturally-sensitive areas, items that are still not traded in the world today but may become tradable tomorrow, the movement of human beings, and global agreements on labor and intellectual-property rights and their protection.

Besides, the current worldwide financial crisis will most likely affect capital movements across borders and the financing of international trade and investment, with a significant increase in their regulation as well as changes in the taxation of foreign profits. These new problems will absorb our energies for many years to come.

John H. Dunning (Reading and Rutgers Universities) thinks these are exciting times for international business teachers and researchers. It may not be too much to say that our field of study is at a watershed in its evolution. **Far from running out of steam, the future agenda of international business is set to be one of the most intellectually challenging and potentially fruitful of all the social-science disciplines in the next two or three decades.** It is in a privileged and unique position to explore the interaction between corporations and the changing cross-border physical and human environments in which they operate as well as this interaction's implications for global economic welfare.

These rather fundamental questions are now being extended as new components of the present and future human landscapes—such as security, reducing poverty, climate change, the challenges of an aging population and the increasing importance of services—and are demanding attention. The impact of these elements needs to be considered—not only how they interface with the **physical environment** but also the **human environment** in which firms and especially multinational firms operate (Dunning & Lundan, 2008).

Each of these questions suggests that such aspects of IB scholarship as culture, human-resource management, international relations, political science, neuro-economics and social psychology need to be better incorporated into mainstream paradigms, theories, empirical research and policy appraisals. It is the integration of these disciplines and fields as well as their respective methodologies into the human environment which I believe should be one of the main thrusts of IB scholars over the next decade or more.

In conclusion, **Stefanie A. Lenway** (University of Illinois in Chicago, and Past President of the AIB [2006-2008]) stated that these are exciting prospects for all of us IB researchers and educators. Some of us may be able and willing to comment on the above ideas and proposals twenty-five or even fifty years from now. Meanwhile, there is plenty of meaningful work to be done, and we hope to have whetted your research appetite!

End Notes

[1] Emerald bought the properties belonging to Elsevier/JAI Press and added them to their sizeable stable of IB and non-IB books and journals. The AIB Fellows Book is Volume 14 of *Research in Global Strategic Management* under the editorship of Professor Alan Rugman.

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