Manag Int Rev (2011) 51:735–754
DOI 10.1007/s11575-011-0106-z

FOCUSED ISSUE: 50 YEARS OF MIR

50 Years of *Management International Review* and IB/IM Research

An Inventory and Some Suggestions for the Field's Development

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Abstract:

- The evolution of MIR during the last fifty years shows remarkable similarity with the quantitative and qualitative development of IB/IM research in general. This is not only because the field of IB/IM started about fifty years ago but also because MIR, like the field itself, has been international in its orientation right from the beginning and has played in some aspects a pioneering and influential role in sharpening the profile of IB/IM research.
- Following its influential role during the past half-century, MIR will help develop future avenues promising further improvements in both the quality and scope of IB/IM research.
- To promote this process, we critically analyze the current state of the field in terms of its dominating research style and generate suggestions how identified weaknesses can be overcome.

Keywords: History of MIR · Past and future developments of IB/IM research · Further qualitative and quantitative empirical studies

Published online: 04.11.2011

The authors would like to thank Professor Jean J. Boddewyn for his valuable comments on an earlier version of this paper. We recommend to the readers the article "International Business Research: Beyond Déjà vu" that Jean Boddewyn and Gopal Iyer published in MIR in 1999.

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The Development of *Management International Review* in the Context of IB/IM Research

According to Richard W. Wright, the academic fields of International Business and International Management (IB/IM) originated in the United States by the mid 1950s and research was done almost exclusively by US scholars (Wright 1970). A few years later, in 1961, the first issue of *Management International Review* (MIR) was published. This is remarkable in the light of two facts. First, MIR appeared as one of the very early academic journals that dealt with problems and solutions in International Business and International Management. Although MIR, during its early years, was not exclusively dedicated to those core problems of IB/IM that took shape a few years later (Wright 1970), every issue contained papers with a clear international focus. Second, MIR was and is not based in the United States but in Germany. In other words, a research outlet was created in a world region where IB/IM research had been largely neglected up to then.

From a general point of view, the journal's strategy with its initially limited focus on IB/IM was a truly pioneering one, and the corresponding risks of failure were high. However, in a German or even European perspective, research on IB/IM problems was not totally new. By the beginning of the 20th century, a rich stream of European and especially German research and publications on IB/IM evolved (Macharzina and Welge 1989) in parallel with the first big wave of real business internationalization. It lasted until WWI, followed by a short period of recovery in the twenties and early thirties. Afterwards, it took decades to overcome the setbacks of the Nazi regime and WWII also in this dimension. Institutionalized research and teaching in Germany and other continental European countries through appointing IB/IM professors at universities developed incrementally only in the late seventies (Engelhard et al. 1996; Macharzina 2008) although European scholars of the post-war years were strongly driven by the notion of internationalization. Political and economic developments such as the 1957/1958 Treaty of Rome and the significant increase of the FDI activities of many continental European firms demonstrated the need and advantages of taking a cross-border view. Besides, some academics did still remember the beginnings of European and especially German IB/IM research by the turn of the twentieth century.

MIR's entrepreneurial role was not as risky as a superficial view of the general conditions could suggest. Besides being aware of internationalization's advantages, the first editorial team of MIR was a truly international one with members from several European countries and the United States. Among them, Louis Perridon, who became, a few years after MIR's inauguration, its Editor-in-Chief, was himself an example of internationalization at its best. Born in 1918 in the Netherlands, he studied Law and Economics in Paris, received his PhD (Law) at the University of Bordeaux, also in France, and his second doctorate (Habilitation) at the Saarland University in Germany. From the late 1950s to the mid 1960s, he worked as a Professor in France and Italy, and, from 1965 on, was Full Professor (Comparative Management) at the University of Munich and, from 1970 on, the Founding President and Full Professor (Finance and Banking) at the University of Augsburg, Germany. As a consequence of both the individual experiences and cross-border contacts of Perridon and of a few German scholars doing research in the IB/IM field, MIR relied right from the beginning on a variety of foreign authors. No other way existed to gather a critical mass of authors being not only interested in IB/IM problems but also having the freedom and the resources to undertake relevant research. A purely German-oriented way would not have been successful.

MIR's second Editor-in-Chief, Klaus Macharzina, took over the editorial office in 1980 after having worked since 1968 as an Assistant Editor and, starting in 1973, as the Managing Editor of the journal. As a doctoral student of Louis Perridon, he was already, during an early stage of his academic career, in close touch with the research and daily-life challenges of internationalization. A few years after receiving his PhD at the University of Munich in 1968, he left Germany and worked from 1973 to 1976 first as Senior Lecturer and then as Professor at the University of Lancaster (United Kingdom). On account of these forces that had formed and shaped his global horizon (Aharoni 1966), he made MIR not only a globally accepted journal but saw also the advantages of further professionalization in managing it. The early internationalization of Macharzina and, especially, his experiences with the Anglo-American region were a great help in establishing MIR as one of the world's leading journals in the field of IB/IM. For us, successors of Klaus Macharzina in the role of Editor-in-Chief, the position that MIR had reached by 2006—the year we took over—was both a great help and a large challenge, and we hope to be able to continue contributing to MIR's successful development. The fact that MIR has been re-listed in the SSCI since 2008 is promising in this regard.

MIR's pioneering and leading role as a research outlet in the early days of IB/IM still prevails in several manners. According to a recent study by Ellis and Zhan (2011) on the question "How international are the international business journals?", the journal's authors come from nearly all regions of the world, leading to a top-position in terms of non-US authors and cultural diversity—together with the *International Business Review* and the *Journal of World Business.* Further, its Editorial Board and its Editorial Review Board are one of the most internationalized ones, compared with other major journals in the IB/IM fields (Harzing 2012, forthcoming) although MIR is interested not only in mainstream research and methodology (for instance, MIR's editorial policy welcomes path-breaking theoretical and qualitative research).

While the internationalization of scholars is an important factor because diversity can lead to higher degrees of innovativeness and productivity, it is only one source of positive outcomes. What is also necessary is a lived plurality of perspectives on IB/IM problems and methods of how to research the latter. In this respect, the openness of MIR's editorial policy, partially based on the independence of the journal from academic institutions, offers new opportunities not only for the journal but also for the entire field. This evaluation is based on a critical and even provocative approach toward the evolution of IB/IM research problems published in highly-ranked journals during the past fifty years. At a first glance, we can identify at least three major developments:

- The enormous growth of research output,
- the internationalization of scholars playing the research game and, thereby, the loss of a purely quantitatively defined dominance of US-based research, and
- the shift towards empirical-quantitative research.

Critical Developments in the IB/IM Community and Its Research Approaches

Regarding the first points Wright and Ricks already made in 1994 (p. 695) in their overview of trends in IB/IM research, "it is astonishing to realize that as recently as twenty-five-years ago, the entire universe of published and ongoing research projects in international business could be counted, summarized and annotated in a single volume! Today, research in many of the dozens of sub-areas of international business research is as extensive as the entire field was then." An indication of the growing interest, if not necessarily of the research in the field, is the membership in the Academy of International Business (AIB) which grew from 237 in 1968 (in what was then the Association for Education in International Business) to 3493 members located in 79 different countries in 2011. As the papers published in the major journals of our field impressively show, the share of empirical papers employing more and more sophisticated quantitative methods has steadily grown. This fact is in line with a statement of Ali (1998, no pages) that "in management, the broad trend has been to follow a quantitative approach."

Besides, looking at underlying factors and resulting outcomes of the research—that is, knowledge—, the positive overall development of IB/IM turns out to be somewhat paradoxical in two respects. The first paradox consists of a growing heterogeneity of authors and the parallel standardization of research cultures. As business nearly all over the world has been internationalizing for decades, the internationalization of scholars in our discipline reflects the growing importance of the IB/IM field in country-specific scientific communities. Compared to the very beginnings of IB/IM research (Wright 1970), it is no longer only done by US scholars.

Moreover, the predominance of North-American and British academicians, which could be observed from the 1970s until the early 1990s (Thomas et al. 1994; Ellis and Zhan 2011), seems to have been overcome. Nowadays, the community is a really international one (Ellis and Zhan 2011) although, given the huge size of the US community, it is not surprising that most authors publishing in high-ranked IB/IM journals are still affiliated with the United States. However, despite the growing internationalization of authors, the rules of the research game (which itself is no longer a multi-domestic but a global one) seem to converge towards basic elements that are still US-dominated (Thomas et al. 1994).

Quantitative empirical research work is preferred because it fosters the impression of producing exact outcomes comparable to those of the physical sciences. Mastering statistical methods may also be viewed as proof of real scientific work. Since the roots of our general discipline of "Business Administration" trace back to non-university origins like commercial schools or colleges, the first steps at university level—done by the end of the 19th century—were accompanied by critical voices from the older traditional sciences, generating strong efforts by our pioneering colleagues to demonstrate the scientific potential of Business Administration. The growing tendency towards quantitative empirical research which occurred first in the United States after WWII may be an indirect heritage of older days because being competent in statistical methods promotes the image of real science.

Furthermore, as statistical methods or mathematical terms substitute partially for verbal argumentations, it is not surprising that non-native speakers of the English language have concentrated on getting quantitative empirical papers published instead of conceptual ones. As Ellis and Zhan (2011) concluded in their paper on "How international are the international business journals?": "The production of international business theory is dominated by English-speaking scholars in general and authors affiliated with US institutions in particular. Authors from countries where English is not the primary language … were more likely to be involved in the production of empirical research" (p. 108).

However, if we overlook the above possible motives for preferring the quantitative empirical approach and go back instead to the very final goal of any scientific work—that is, the search for truth (Popper 1972)—, there is only one question left—namely, *is such an approach suitable for producing truth*? As soon as we ask this question, we have to remember that there are basic differences between science and our field: "Unlike the position that exists in the physical sciences, in economics and other disciplines that deal with essentially complex phenomena, the aspects of the events to be accounted for about which one can get quantitative data are necessarily limited and may not include the important ones" (Hayek 1989, p. 3). This does not mean that quantitative empirical research cannot lead to truth but it should be made clear that not all dimensions or elements of truth can be identified via quantitative data. This is even more important when single/individual studies are not integrated into a general body of knowledge.

Besides the current preference for quantitative empirical studies, the impression of a US-dominated research style is fostered by a second phenomenon. To get an empirical-quantitative paper published, it has to be structured in a generally accepted US-specific manner that includes an introduction, theory and hypotheses, research method, results, discussion, conclusion, and limitations. Moreover, the methods employed have to reflect the theoretical and statistical mainstream approaches defined by the US scientific community. Hence, methods that could potentially also be suitable to maintain the standard of theoretical and methodological rigor (Thomas et al. 1994), e. g., qualitative empirical research and/or triangulation (Denzin and Lincoln 2005), are likely to be ignored. However, this is not diversity which, especially in IB/IM research, is an important success factor. Instead, it means overlooking the fact that research methods may be culture-bound and that different methods can also lead to good results and constitute a step towards truth. However, the US model of IB/IM research was and is very successful while the initial ethnocentric strategy has changed into a global one (Porter 1985) serving via standardized instruments the world market of IB/IM research.

The paradoxical consequence of these two developments is that, although the percentage of US-affiliated authors is decreasing, the US model of research and its diffusion have become the dominating one in our field. Regarding the hope of Wright and Ricks (1994) and Daniels (1991) that our field could benefit from the growing cultural diversity of researchers and their collaboration, it has not come (fully) true so far.

Since the internationalization of research means also a growing number of research outputs, another paradox is closely related to the two previous ones. Although there is more and more research employing more and more sophisticated statistical methods, thereby demonstrating rigor (see also Brown 2011), this approach has not yet led to a deeper understanding of major IB/IM problems. In other words, research questions, variables, instruments, and data are tending to be individual ones, i.e., author-respectively project-bound, thereby hindering their combination and interpretation as part of a big story. Such a development may be partially affected by a shift from searching truth—the ultimate goal of science—to viewing research as a mean to promote individual careers.

As most careers depend nowadays on a pure quantitatively defined research output, it seems to be consequent from an individualistic point of view to increase the number of publications in higher-ranked journals. Most of these journals, however, emphasize quantitative empirical research. Against this background, the output productivity of research can be positively influenced by several approaches. A first and simple method is to rely on creating and using both small, narrow (covering only a small part of reality) and non-longitudinal samples. This happens because it takes less energy and time to gather the data, and even those samples may demonstrate the mastering of complex sophisticated methods interpreted as proof of real science.

Another frequently applied method is the multiple utilization of samples originally created for a specific research question. After having gathered data, this set enables researchers to squeeze out a number of different papers focusing on similar but in fact different topics. However, a disadvantage of this approach is that, if other and thereby distinct research questions are tried to be answered by employing the original set of data, a tendency towards a growing misfit between the new research question, its conceptualization and the data does occur. The methods do not match the questions, leading to a decrease of the subsequent research's quality.

Finally, the chances of getting a submitted paper accepted are improved when significant results are reported. This relationship can lead researchers into the temptation to search for a study-specific measurement of their variables—this is, for example, a major weakness of the Internationalization-Performance research (see Glaum and Oesterle 2007)—that produces significant results. Yet, comparing the results of different studies based on an (almost) identical research question will be hindered by such a significance-oriented approach.

All possible methods of increasing productivity can be labelled as types of research individualism because either individual (personal) objectives are dominating the research process or study-specific methods are chosen as a means of increasing research productivity. The collective effects of research individualism, however, tend to be negative because this approach transforms the sum of research outputs and, therefore, our body of knowledge into a system of loosely-coupled fragments of knowledge. Therefore, the individualism used in approaching even big questions (Buckley 2002; Buckley and Lessard 2005; Peng 2004; Shenkar 2004) leaves almost no chance of linking the single-type produced knowledge with other ones and of integrating it into a "grand theory."

This paradox could be described as "growth without quality." A brief look at the core approaches and theories of our field as well as a corresponding analysis of the content of major IB/IM textbooks supports the argument that such a paradox exists. These approaches and theories have been developed using either a conceptual approach or a huge empirical basis—that is, a really large sample. This means that the very "foundations and pillars" of our field do not come from the currently dominating type of small-scale empirical research employing sophisticated quantitative methods but from dealing with big problems either in a conceptual or big-sample-based empirical way.

Such big problems are treated in studies on the reasons or motives for internationalization, strategies of internationalization, internationalization processes, and instruments of coordinating international business. The following approaches and theories may serve as

Range of research questions		*Empirical*	*Conceptual*
	Broad/holistic/ integrative (big questions)	Big samples are preferable (Hofstede, PIMS) ("big problems need big tools") but small-scale research is suitable if results can be linked and coordinated into a "big answer." If not, there is a misfit of problem and method	This approach was successful in the past. Its major prerequisites are a broad knowledge of the field and of neighbouring disciplines, and a "feeling for real business"
	Narrow/isolated	Individualistic research. The main motive is promoting one's academic career, not producing truth	No suitable approach exists since conceptual work leads not to detailed answers to detailed questions so that there is a misfit of problem and method
		Research method	

Fig. 1: Range of research questions and corresponding research methods

examples: Perlmutter's EPG Model (1965, 1969), Hymer and Kindleberger's Monopolistic Advantage Theory (Hymer 1960/1976; Kindleberger 1969), Dunning's OLI-Paradigm (1973, 1977), the Uppsala Model of Johanson and Vahlne (1977, 1990), Johanson and Wiedersheim-Paul (1975), Buckley and Casson (1976) as well as Rugman (1981) and Teece (1981) with their Internalization Theory, Prahalad and Doz' HQs-Subsidiary Relationships (1981), Hofstede's Culture's Consequences (1982), Levitt's Globalization Thesis (1983), Hedlund's Concept of an MNC as a Heterarchy (1986), and Bartlett and Ghoshal's Concept of the Transnational Firm (1988).

Figure 1 presents an overview of the major forces currently driving and influencing research in our field. Its message is a simple one and was already mentioned above—namely, that if we accept that there is a need to research big IB/IM problems, it would be naïve to assume that such questions can be solved by using small instruments in an individual-researcher-centric way.

Preferred methods for studying big problems should be *either:*

1. Quantitative empirical research based on large samples since this type of research will enlarge the chances of producing holistic, integrative knowledge,
2. conceptual research via transcending problems. Using this approach as a single one can lead to new methods and concepts of how to deal with the challenges of internationalization while empirical research can only identify what has been done before although case studies can help identify big problems (exploration), *or*
3. empirical small-scale research. This type of research can help solve big questions if the studies are coordinated (using the same variables and measuring phenomena in comparable settings).

The currently successful model of research is really based on an empirical quantitative approach but it seems to be too individualistic in nature. Such an impression is promoted by the fact that this approach has not yet led to a broader and well-structured knowledge. However, research that has heavily influenced our field in the past is either conceptual in nature, a big-sample-based empirical endeavor, or a qualitative empirical one. As a result, a mismatch exists between the calls for researching big questions and currently predominating methods.

In such a situation, journal editors can try or even ought to reduce the gap between the needs of our field and ill-fitting actions to serve such needs. *Management International Review* will emphasize more strongly than in the past the publication of conceptual research, large-scale (sample) research, and such (small-scale) empirical studies that explicitly explain in which ways their results enlarge and enrich our knowledge of IB/IM problems and solutions. To meet those expectations, such studies should not only add new results and insights to the existing ones but should either integrate their findings into the already existing body of knowledge or—if possible—should show how their results are able to correct or even replace those of older studies. Realizing this publication strategy could help lower the production rate of isolated knowledge and could foster the harmonization of still partially fragmented elements of knowledge. Such a policy is in line with the call of Wright and Ricks who, already in 1994, identified a need “to synthesize what we are learning into broader, more integrative frameworks” (pp. 700 et seq.).

Suggestions for the Future Development of IB/IM Research

We will now outline some suggestions about how, according to our opinion, IB/IM research should develop in the future so that it can be more fruitful both for the scientific system and for business practitioners. When developing these suggestions, our ideas were guided by the fact that IB/IM, like business administration in general, is a professionally-oriented academic field. Business administration research occurs in professional vocational schools. This means that the IB/IM research must cover the decision-making processes of business practitioners. Besides, we think that the necessary reorientation of IB/IM research should not only refer to the institutional context (e.g., the incentive system) existing in business schools. Instead, the individual scholar herself/himself is being asked to change her/his research behavior. Hence, most of our suggestions refer to changes which the individual scholar can make during her/his regular research work. We organize these suggestions according to the phases a scholar typically goes through when conducting a research project. Of course, several of our suggestions do not exclusively apply to the field of IB/IM but also to other areas of business and management research or to it in general. Yet, we will show that, for the field of IB/IM, these suggestions are even more important than for other fields within business administration.

Adjusting Research Topics more to Practitioners’ Challenges

We first want to argue that too many of the current IB/IM research-oriented publications do not sufficiently address those issues which are most important and interesting for business practitioners nowadays. For example, it is amazing that the scandals that have surfaced in the business world during the last few years or the ongoing financial and economic crisis have been very rarely covered by IB/IM researchers. The same holds true for subjects such as “virtualization of the business world,” “corporate restructuring” and “terrorism and business.” Surveys such as the one by Czinkota and Ronkainen (2009) on future trends on IB/IM show that such topics have played a much larger role in IB/IM practice than in research-oriented journals.

Besides, a review of a large number of research-oriented journals gives us the impression that this discrepancy between academic- and practitioner-oriented topics is more pronounced in the more prestigious research-oriented journals. Although this phenomenon can be partly explained by the fact that top-tier journals publish papers reporting on very sophisticated and time-intensive research projects, we think that this discrepancy has considerably contributed to the fact that practitioners read scientific journals only rarely (Oesterle 2006). Thus, in the future, it will be necessary for business practitioners to be more involved in the first conceptual phases of IB/IM research projects (Mohrman et al. 2001). Such an involvement of practitioners should not result in a jointly conducted research project of scholars and practitioners since there are reports showing that such collaborations are very difficult to conduct (Amabile et al. 2001). Instead, in the conceptual phase, scholars should collect information on practitioners’ views on what are important research topics. This suggestion, which is in line with Van de Ven and Johnson’s (2006) call for an “engaged scholarship,” seems necessary because empirical studies (Rynes et al. 2001) have shown that, in less than 20% of the articles published in a top-tier journal, were practitioners involved in the conceptual phase of the research projects about which the publications reported.

An important precondition for IB/IM research to provide insights important and interesting for business practitioners will be that the scholars turn away from the currently dominant tendency to conduct a descriptive type of research. Most models currently developed and tested by IB/IM scholars refer to variables and data describing the *de-facto* behavior which business firms have already applied. As a consequence, most IB/IM scholars are good at documenting and explaining actual behavior but are not as good in fulfilling the “utopic function” which should also be an important element of any scholarly work. If we ask the “average person on the street” what a scholar belonging to a professional school at the university level should do, she/he would probably answer that it is not sufficient for scholars to determine the relationships already existing in the real word. She/he would further answer that a scholar in such a discipline should have the capability to contribute innovative ideas to the practical world. We think that this important “think tank function” has been carelessly neglected by most IB/IM scholars during the last decades. Among the thousands of IB/IM research articles, there are very few that lean in this direction. There can be no doubt that the seminal articles of Perlmutter (1969) or Hedlund (1986) belong to this rare species but also that this species is much too small.

Orientation of Research Towards Variables Which Can be Influenced by Practitioners

Second, related to the above point, we want to argue that more IB/IM research should refer to variables that can be influenced by business practitioners. A stronger orientation of IB/IM research towards such managerial-action issues is necessary because business administration—more than psychology or sociology, for example—is a practice-oriented academic field. Business-administration research refers to a field of reality in which managers have to make decisions ensuring the success of their firms. Hence, it is not sufficient for business-administration research to be solely striving for fundamental, pure knowledge. For instance, it is not enough for business-administration scholars to develop insights describing the risk orientation of managers or the determinants of employee turnover. Beside such insights, there is the need to develop knowledge of how to influence the managerial levers able to lead to an appropriate level of managerial risk management and employee commitment. This need to develop action-oriented knowledge is even higher within business administration's functional subfields, including IB/IM, because in these subfields there is a higher specificity of research topics than in the general parts of the discipline and because the subfields can relate their insights to the basic concepts provided by general business-administration research.

It can be shown that, during recent years, IB/IM research has not always followed this need to refer to practical decision-making but has instead developed in the opposite direction. A cursory inspection led us to the assumption that many recent IB/IM studies have focused on variables which are beyond the influence of business practitioners. Thus, many IB/IM studies refer to non-operational concepts such as "psychic distance" or "liability of foreignness" or they refer to general business-administration concepts of the same abstract kind like "absorptive capacity" or "dynamic capabilities." Such concepts are difficult for managers to visualize and operationalize.

This orientation of recent IB/IM research to focus on non-manageable variables can be illustrated by the development of research on headquarters-subsidiary relationships and the organization of MNCs. Between the 1970s and the early 1990s, most studies of this area focused on such coordination instruments as "centralization of decisions," "standardization of decisions," "formalization of decisions", and "manager transfer." The studies of this period dealt with design variables which can be handled by MNC managers. For instance, headquarters' managers can determine which decisions should be kept at the headquarters and which can be delegated to foreign subsidiaries, and they can develop decision rules which have to be applied by the subsidiary managers in their decision-making (=standardization of decisions). For instance, research by Brooke and Remmers (1973), Hulbert and Brandt (1980), Welge (1980), Gates and Egelhoff (1986), or Macharzina (1993) focused on these types of variables.

However, since the early 1990s, especially influenced by Bartlett and Ghoshal's (1989) seminal work, the focus of research studies on organizational aspects of MNCs has changed. In their influential publication, intensive knowledge flows among MNC subunits—especially direct knowledge flows from subsidiary to subsidiary—were assumed to be the most important drivers of MNC success. As a consequence, many subsequent studies explored the intensity of different types and patterns of knowledge flows within MNCs. Most of these studies were/are mainly descriptive in nature or their goal was/is to identify whether there is a relationship between the intensity of knowledge flows, on the one hand, and the performance of MNCs, on the other. What is missing in most of this research is an analysis of the instruments (media) which MNC managers can apply in order to intensify or reduce the volume and type of knowledge flowing among MNC subunits. Given this shift from coordination instruments to knowledge flows, recent research on the organization of MNCs seems to be much less practically helpful than those conducted some decades ago.

A major reason for the orientation of many IB/IM studies to the world of non-manageable variables lies in the fact that, during the past few years, IB/IM journals, like other business-administration ones, have increased the requirements for their contributions in terms of methodological rigor. As a consequence, to permit a solid testing of hypotheses, scholars have to use relatively large datasets which allow them to apply advanced statistical testing procedures. Further, in order to avoid questions referring to the data's reliability, more and more scholars prefer to work with publicly available databases containing "official data." Yet, such databases usually do not paraphrase internal managerial aspects—and especially not the parameters controllable by managers—but, in most cases, only "surface characteristics" of the business firms.

This shift of the IB/IM research agenda may also be caused by the fact that many business-administration scholars (especially, *vis-à-vis* economists) are plagued by a sense of inferiority. They think that their own field should have the high level of rigor many economists claim. This feeling on the part of IB/IM scholars is strengthened by the fact that, within business administration, many representatives of the functional subfields (e.g., marketing, finance, and operations) question the scholarly rigor existing in cross-sectional fields (e.g., IB/IM). Hence, nowadays, not only research-oriented business schools but, especially, their IB/IM departments maximize values typical of the base academic disciplines (e.g., sociology or psychology) rather than those typical of a vocational school (Donaldson 1985, 1995).

Greater Implementation Emphasis

Research projects specifying under which conditions particular market-entry forms are most appropriate always was, is, and will be at the very heart of IB research. Over the years, more than thousand empirical research projects were executed specifying, under which contextual constellations, may exports, international contracts, or foreign direct investment lead to the highest level of performance. Over time, these studies also became more and more fine-grained, both with respect to the market-entry forms studied, the contextual factors used, and the empirical methods applied. Of course, many of these studies are important inputs for the development and extension of IB/IM theory. Yet, we think that it is only half of the story that IB/IM scholars should be telling. The success of MNCs' international-business activities is not only dependent upon the selection of market-entry strategies fitting the respective contextual factors but, at the same high level of importance, their knowledge referring to the implementation of the selected market-entry form in a particular host-country market. It is our impression that the number of studies addressing the implementation of market-entry strategies is much lower than that of the first kind. This practice has to be questioned because nearly each market-entry form requires specific subsequent leadership, managerial, and organizational decisions which are not very well explored by IB/IM scholars. Thus, we need more studies focusing on the implementation of market-entry strategies.

The reasons which have led to the underdevelopment of IB/IM research referring to the successful implementation of selected market-entry strategies are broadly the same as those mentioned within our previous suggestions. Whilst IB/IM scholars are able to draw statistical data on the frequency/intensity of the market-entry forms used by MNCs from public sources, data on the managerial implementation of market-entry forms are usually not publicly available. Scholars interested in empirically studying such implementation issues usually have to use the cumbersome way of collecting the data by themselves. However, this way is not only time- and cost-consuming but also risky. During journals' reviewing processes, many reviewers tend to be sceptical with respect to data collected by the scholars themselves because they assume a lower level of data quality than those available from public-data sources. Here, journal editors have to weigh the pros and cons of the theoretical conceptualization and the empirical material developed/used by the respective scholars. Yet, in any case, it should be clear that objectivity and reliability are not the only quality criteria of empirical data because validity is another important issue which should not be ignored. What is the benefit of objective and reliable data if they refer to an irrelevant or an uninteresting question?

In this regard, we think there is a great need to extend the number of studies referring to the organizational architecture of MNCs. We substantiate this view by the fact that IB/IM theory since its very beginning (e.g., Hymer 1960/1976; Kindleberger 1969) has argued that MNCs' competitive advantages *vis-à-vis* domestic firms stem from their superiority in extending their economies-of-scale advantages across countries in transferring their technological, marketing, management, and financial knowledge from one country to another, and in their ability to systematically exploit imperfect factor and product markets. Such monopolistic advantages enable MNCs to operate their foreign subsidiaries more profitably than their local competitors can (Shenkar and Luo 2008). Yet, a systematic use of such monopolistic advantages requires a sufficient level of integration of the MNC's managerial and operational processes across countries, and this, in turn, necessitates a careful design of the MNC's organization structures. It is such structures and other organizational-design elements which provide the cross-border information-processing capacity needed to develop and extend an MNC's monopolistic advantages (Egelhoff 1988). If we compare this need with the present distribution of IB/IM studies across research topics, we think that we currently have too few studies focusing on the organizational dimension of international business. Whilst, in the 1970s and 1980s, there were many studies on organizational matters, interest in this topic has unfortuntaely waned during the last two decades.

Qualitative Research Emphasis

Regarding research methods, we want to encourage IB/IM scholars to use qualitative-research methods more frequently (Piekkari and Welch 2006; Boddewyn and Iyer 1999; Bettis 1991). In the IB/IM fields, more qualitative research is necessary because many peculiarities of IB/IM *vis-à-vis* domestic business/management cannot be sufficiently captured by quantitative data. In particular, differences in the cultural and institutional environments of countries are difficult to measure quantitatively. Another major reason for an extended use of qualitative-research methods rests on the insight that large-scale quantitative empirical research, which can be used to test models and hypotheses, seldom detects totally new phenomena and relationships. This happens because the latter usually occur in single or few cases which do not lend themselves to an application of quantitative-testing research methods. Yet, especially for IB/IM research, the discovery of the new is an important task because the research object develops dynamically.

Qualitative research focuses on such single or few cases so that it seems to be imperative to use it in fields of study characterized by highly dynamic changes where knowledge structures are often valid for only a very short period of time. If the models suggested and tested by IB/IM scholars lag behind developments occurring in business practice, they will be neither appreciated by the members of the academic community nor by business practitioners. In this respect, Siggelkow (2007) argued that qualitative research is able to contribute in three ways: (1) it can be used to generate research questions, (2) it can inspire new ideas, and (3) it can be employed as illustrations. Besides, it has to be expected that a more frequent use of qualitative research methods will also lead to an increase in the originality of the research results generated by IB/IM scholars.

Our call for the intensified use of qualitative-research methods in IB/IM is supported by a number of further arguments, First, the exploratory nature of this research type helps extend and specify the research models documented in the literature. Second, case studies emphasize the rich, real-world context in which the phenomena occur (Eisenhardt and Graebner 2007). In particular, these methods promise a more accurate understanding of the causal mechanisms that work in the real world. Third, this type of research corresponds better to the methods practitioners use to obtain the required knowledge for their daily operations. Fourth, qualitative research may be a method for exploring outstanding types of IB/IM action. Fifth, it must be noted that, among the most influential articles in management research, qualitative ones can be found to predominate in relative terms. As examples from the IB/IM field, we can mention the studies of Perlmutter (1969), Edström and Galbraith (1977), and Bartlett and Ghoshal (1989)—all resting on qualitative-research approaches. Indeed, publications that build theory from cases are often regarded as the most interesting type of research (Bartunek et al. 2006).

One objection frequently raised with respect to case-study research is that it would lead to highly situational, individual, and idiosyncratic findings. This objection is correct to the extent that an individual case cannot prove a theory (Siggelkow 2007). Yet, scholars have developed avenues to reduce the problem of idiosyncrasy in case-study research. Central to these suggestions is to work with several cases so that the development of a replication logic will be possible. That is, each case serves as a distinct experiment that stands on its own as an analytic unit. Like a series of related laboratory experiments, multiple cases are discrete trials that serve as replications, contrasts, and extensions to the emerging theory. The theory-building process occurs via recursive cycling among the case data, the emerging theory and, later, the extant literature (Eisenhardt and Graebner 2007).

However, our plea for more qualitative IB/IM research should not be understood to be an appeal to move away from quantitative, large-scale empirical research. Of course, we are aware of the specific limitations of qualitative-research methods (Eisenhardt 1989; Siggelkow 2007). For instance, their limited representativeness and the increased requirements regarding scholars' information-processing capabilities must by no means be underestimated. Besides, it should be noted that the qualitative mode includes a wide spectrum of quality criteria of empirical research (Yin 2003). Nevertheless, the well-known opportunities to create valuable knowledge through qualitative-research methods and their better fit with practitioners' ways of analyzing situations seem to be large enough to call for an increase in their application.

Another behavioral mode we would like to see used more often is executing qualitative studies *before* conducting quantitatively-oriented IB/IM studies. Far too often, topic A is studied qualitatively by scholar 1 and topic B is studied quantitatively by scholar 2. Hence, it seems necessary that, more frequently, the same topic should be first studied qualitatively and with quantitative-research methods afterwards. Of course, it does not have to be necessarily the same scholar conducting both types of research. This is another aspect where management research has not yet reached the high level of disciplinary maturity being promoted in previous sections.

We think that there is a need to position this call for more qualitative research in this introductory paper because, during the past few years, only very few papers were submitted to MIR, which rested on qualitative-research methods. This is astonishing since MIR's Editorial Policy effective since 2007 explicitly encourages scholars to submit qualitative research work to it. Discussions with the editors of other IB/IM journals revealed that they too have trouble receiving enough high-quality papers resting on qualitative empirical research methods.

Greater Coherence and Integration of Research

Worldwide, there are more than 20 research-oriented academic journals focusing on the IB/IM field. It is likely that a similar number of IB/IM journals exists, which are published in languages other than English but, unfortunately, their content is widely ignored by most IB/IM scholars. If we assume that, on average, each of these 20 journals publishes 30 IB/IM papers per year, some 600 IB/IM-oriented research reports appear yearly in English-language journals devoted to this field. This large number is significantly extended by the thousands of IB/IM-oriented papers presented at international academic meetings (e.g., the AIB-, EIBA-, and AOM-conferences) but which do not succeed at becoming published in an academic journal.

Further, it has to be considered that one of the first things young scholars are taught is to identify a research gap which is not yet covered in other research projects. Given the tendency to study "unworked fields," there is not only a large number but also an extreme heterogeneity of topics and conceptualizations among IB/IM research papers. We think that, during the last decades, IB/IM scholars—like the business-administration community in general—have overemphasized this goal of scientific diversity and innovativeness over the goal of developing a body of knowledge which is sufficiently coherent and integrated. Thus, during the forthcoming years and decades, it will be a key challenge for the IB/IM community of scholars to increase the level of integration existing among its research projects and findings (Boddewyn and Iyer 1999). There are several ways to approach such a higher level of integration. In this regard, one key instrument are meta-analyses which do not necessarily have to be conducted in a quantitative manner. What is also needed are more re-testings of existing research findings in order to equip the IB/IM field with a more robust stock of knowledge.

Greater Theoretical Emphasis

It is widely accepted that scientific disciplines need a solid theoretical basis. One might even argue that theories are the constitutive elements of any scientific discipline, and academic research field lacking a specific theoretical basis do not have the status of being considered as disciplines. The term "theory" may be defined as an overarching system of general arguments used by the scholars in a specific field of research. Theories are important for scientific disciplines because they bundle the key argumentation used by their members, they integrate the manifold findings existing within a particular discipline, and they can serve as reference points for future scholarly research activities. If we look at the theoretical basis of the IB/IM academic field, it becomes obvious that:

- Most theories used by IB/IM scholars are borrowed from other subfields of business adminstration such as strategy, organization, and finance—or even from disciplines outside business administration (e.g., economics and sociology). For instance, this is true for the resource-based view (strategy), real-options theory (finance), information-processing theory (organization), and neo-institutionalism (sociology).
- Many theories central to IB/IM research were developed decades ago—for instance, the theory of monopolistic advantage emerged in the 1960s, and internalization theory dates of the 1970s.

Of course, with respect to the first peculiarity of IB/IM theory, one might argue that for such a cross-sectional field as IB/IM, it will be always difficult to develop a system of general thoughts which are highly independent from those of other business-administration fields. One may go even further and say that IB/IM's use of theories developed elsewhere helps it interlock with these other research fields and disciplines, and that it is therefore an advantage if IB/IM lacks a self-contained theoretical stock. Yet, the second peculiarity of IB/IM theory has to be evaluated differently. Here, it is important to notice that the number of research papers has exploded during the last decades and even years but that the number of IB/IM publications that represent important contributions to theory has declined. Therefore, it seems that the IB/IM field has generated a growing number of marginal insights while lacking timely overarching concepts which would comprehensively help understand current developments in international business and draw together the manifold studies into a significant stock of arguments. Thus, in the future, more publications are needed which try to develop new theories for the IB/IM field.

We think that the lack of fundamental theoretical contributions in recent IB/IM publications is mainly caused by the academic incentive system as it developed during the past decades. First, scholars have to publish their work in the format of journal articles which usually cannot go beyond 25–30 pages. Second, the academic incentive system forces them to deliver every year a specific number of academic publications. Both requirements are in conflict with the approach needed to develop a new theoretical framework, and they need much more room to be elaborated. Remember, for instance, that internalization theory was introduced by Buckley and Casson (1976) and by Rugman (1981) in a *book* form. The same is true for instance for Aharoni's (1966) behavioral internationalization theory.

Greater Critical Emphasis

Finally, we would like to see more IB/IM publications with a critical tone. We think that IB/IM scholars should be more critical both with respect to research results—their own and those of other scholars—and to the behaviors and other developments occurring in business practice. For instance, it is interesting to notice that only very few IB/IM scholars have analyzed and commented on the 2008/2009 global economic crisis—a phenomenon having a pronounced international-business dimension. In this respect, we would like to make the following suggestions:

- IB/IM scholars should take a more critical distance *vis-à-vis* their research findings because Karl Popper's (1959) critical rationalism not only argues that scholars should always be very sceptical with respect to the truth of the hypotheses they developed but that they should permanently strive for a falsification of their own hypotheses. There are indicators that many current scholars and journal editors do not follow this postulation at all. For instance, after analyzing several hundred top-tier management publications, Fung (2010) found that: (1) in none of them was the number of rejected hypotheses higher than that of confirmed ones, and (2) many hypotheses were confirmed exactly at the statistical threshold values of $p=0.05$ or $p=0.01$. The latter finding is an indicator that many scholars collect data until they are able to confirm their hypotheses at a statistically sufficient level but this result has nothing to do with Popper's idea of being self-critical.
- There are also signs that the IB/IM field has become more uncritical over time. Take as an example the research on the organization of MNCs. In the 1970s and 1980s, there were many studies which, based on serious research work, tried to identify which organizational form fits which MNC strategy (e.g., Stopford and Wells 1972; Egelhoff 1982; Daniels et al. 1984), and these studies led to a differentiated, situation-specific stock of knowledge. However, induced by Bartlett and Ghoshal's (1989) key publication, this knowledge was replaced by the naïve and escapist view that the transnational solution is the only possible organizational form fitting the strategies of all MNCs.
- We think that the IB/IM community of scholars should regain its ability to comment on developments in business practice. In earlier times, scholars perceived it as a privilege to take a firm stand with respect to developments occuring in the field they were studying. This was a constitutive privilege for any social scientist. It is regrettable that many IB/IM scholars have lost the ability or willingness to behave in such a manner, because the IB/IM field is riddled with highly political questions—for example, the transfer of jobs to foreign countries, the ethical tenability of wage differentiations, and the appropriate handling of dubious business behaviors at particular foreign locations. It could be again argued that the increasing specialization and mechanization of business-administration research has led to this unfortunate development.

In this MIR Issue

Given the above critique of the current status of IB/IM research, this focused issue is dedicated not only to celebrate 50 years of MIR but also to serve as a signal that there is a growing need for change in our discipline. This anniversary issue is driven by the spirit of re-emphasising conceptual and large-scale research and the accelerated development of real IB/IM calls for new paradigms, theories, concepts and models in this academic field.

In the opening paper, Alan M. Rugman, Alain Verbeke and Quyen T. K. Nguyen analyze IB/IM research during the past 50 years. After identifying three key units of analysis that dominated the history of IB theory—the country (as origin and target of trade and FDI), the firm (MNE) and the subsidiary—, they suggest that the most promising future research directions for IB/IM theory will be the study of the interactions among these three parameters, with an emphasis on the subsidiary as the key building block.

Considering the fact that various IB/IM scholars have recently raised concerns about the field's decreasing output in knowledge creation, Joseph L. C. Cheng, Wenxin Guo and Bradley Skousen try to re-engage IB/IM scholars into seeking new knowledge creation designed to help IB/IM remain a relevant and fruitful field of study. Therefore, they propose an investigative approach to guide IB/IM scholars in developing new groundbreaking theories.

The other papers in this issue aim at particular research fields within IB/IM. Gabriel R. G. Benito, Bent Petersen and Lawrence S. Welch deal with a basic IB/IM-topic—namely, market-entry modes. They analyze six Norwegian companies in three key markets (China, the UK and the United States) as the basis for an examination of how and why companies combine different foreign operation modes.

Concentrating on one specific entry mode, Arjen Slangen, Sjoerd Beugelsdijk and Jean-François Hennart investigate the influences of cultural distance on exports. In contrast to prior studies that argue for a generally negative relationship, they provide a more precise view by distinguishing bilateral exports at arm's length from intra-firm exports, and offering different results regarding the influence of cultural distance.

In search for new insights into the increasingly important field of knowledge management, Patrick Regnér and Udo Zander focus on knowledge creation inside the multinational company. Developing a new perspective, they suggest that the agglomeration of a variety of diverse social-identity frames nested inside a MNC do shape an environment in which useful knowledge can be created.

Peter W. Liesch, Lawrence S. Welch and Peter J. Buckley explore another basic topic in IB/IM research—namely, the role of risk and uncertainty. They state that there is a need for a more nuanced treatment of these variables in the international development of firms. To accomplish this mission, the authors introduce dynamic concepts of uncertainty acclimatization and risk accommodation based on co-evolution theory. This approach will allow a better recognition of how uncertainty and risk may evolve over time.

The final paper of this anniversary issue deals with the problem of conceptualizing expatriates' return on investment. Yvonne M. McNulty and Helen De Cieri take on the challenge of developing a conceptual framework which should help determine the value gained from long-term international assignments. Furthermore, they identify elementary questions to guide future research on this issue.

We hope that the papers presented in this issue, along with the suggestions made in our introduction, will help to stimulate further IB/IM research and that the readers will enjoy them. Special thanks go to the colleagues who invested time and efforts to review the papers submitted to this focused issue. Their reviews helped to select topic related papers and to improve their quality. And finally we want to thank very much not only the large number of authors, editorial board members, and ad-hoc reviewers but also the readers who have supported MIR during the last five decades. Without their generous support MIR would not be what it is: One of the leading academic journals in the area of IB/IM.

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