The Relevance Problem of International Business Research

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1 Relevance as a Matter of Perspective

The discussion on relevant research seems to be a constant phenomenon of self-reflection within the academic field of International Business (IB). Already in 1991, the Academy of International Business organized its annual meeting as conference dedicated to the topic “Relevance in International Business Research.” In his speech as outgoing AIB President John D. Daniels shared his thoughts on the topic leading to the plea that relevance needs more linkages. Whereas nowadays relevance is mostly viewed as being equal to applied research (that can be [directly] transferred to and implemented by the business practice; see the ongoing debate on rigor vs. relevance), John D. Daniels showed that relevance can be interpreted by taking also the opposite perspective—relevance via theory building: “During the past few months I have heard enough comments to realize that AIB members have divergent views on relevance, ranging from ‘only theory building is relevant’ to equating relevance with applied research” (1991: 177).

As so far the topic “relevance” should be approached by clarifying first the question to whom IB research could be relevant. Figuring research simply as a value creating chain of units and their activities with scholars as producers, journals as outlets of research results, the real business world as source and object of research, and consumers of research like other scholars, students, and practitioners shows that the respective process consists of different elements with most likely also different interests. This means that not all participants of the process might be guided by the same understanding of relevance.

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According to Popper (1995: 4) "our aim as scientists is objective truth; more truth, more interesting truth, more intelligible truth." Especially the dimension "interesting truth" makes clear that the results of research should attract attention or should be relevant. However, in an academic discipline like IB that is dealing with real phenomena attention articulated only by other scholars is not enough. If no research outcomes are produced that attract the attention of practitioners the discipline would uncouple itself from its object endangering, e.g., the willingness of practitioners to fill out questionnaires, to be interview partners, to deliver material for case studies or to fund research projects: In the words of John Dunning ". . . the effectiveness of our scholastic efforts to study and teach international business is entirely dependent on our capability to marshal and organise the necessary human and other assets so as to supply a range of end products which are acceptable to the academic community of which we are part, our paymasters and the main purchasers of our products, viz. the business community" (1989: 411). So the question is not about rigor vs. relevance but how to produce true relevant results fostering the progress of theory and the real business world.

On the other side, younger scholars struggling for a tenured position (job) in the academic sphere might be guided also by the interest to use their research to promote their own careers. Relevance is in this case not automatically the same as in the ideal first case but dominated by that what high-ranked journals expect. To be attractive to the job market (junior) scholars have to follow the rules of the game, i.e. to run that kind of research in terms of topics, concepts, methodology, and outcome that meets the (standardized) expectations of the journals or better—the respective scientific community as occupants of the ivory tower. The journals themselves are under competitive pressure seeking to become more relevant in terms of getting higher impact factors. Relevant are therefore those papers that will be cited by members of the scientific community. Higher numbers of citations receive most likely those papers that follow as outcome of the perceived dominating dimension "rigor" the generally accepted principles of producing research results. This means, one has to deal with a topic being in fashion (new trends are normally set by prominent and leading scholars, not the young ones) and has to treat it with the established instruments—application of sophisticated, preferably new theories (Tourish, 2020), ambitious conceptual thoughts and/or ambitious empirical analyses, mostly quantitative ones.

However, that kind of research is not (always) very attractive to those that are both objects (input) and potential customers of the scientific production process—the real business world and its decision-makers. They expect research results that help them to make their business more efficient and effective or—in a more general sense—to get along better with the challenges business has to face. According to several studies (e.g., Gopinath & Hoffman, 1995; Oesterle & Laudien, 2007) and our own impression IB scholars do not always know or care for (e.g., Lewin, 2004) what problems international firms see and have to solve.

The following discussion of the question, what could be done to make research in the IB field (more) relevant will try to incorporate critically those perspectives showed above that are specific to the IB field. Some of the already described perspectives are focussing on problems that are typical of today's business administration as academic discipline in general, e.g., rigor-orientation, methodological narrowness in terms of emphasizing quantitative empirical analyses, and large distance to practitioners at least in their (potential) function as customers of research output. As so far the following sections are guided by the idea of a problem-centered discussion, taking such problems as starting point that are content-wise typical of the discipline IB. By doing so the presented thoughts will of course not be able to reflect a one-best-way solution. Their basic motivation and goal are rather to help producing a higher degree of awareness toward the problems that are likely to impair the up-grading of relevance in the IB research process. In this context relevance will be first discussed as problem within IB research; a further subsection will focus on IB research as a system that is—according to our impression—not intensively interacting with other disciplines of business administration and beyond, thereby likely to overlook already existing knowledge and not fully realizing the chance to broaden and deepen the own knowledge bases: The consequence of paradigmatically sticking to an own perspective is favoring a limited ability to explain the real world of IB, because already existing approaches of other disciplines to describe and to explain phenomena of IB are not sufficiently employed or exploited. And finally, the last section will focus on common misunderstandings of authors when it comes to submitting their research papers to relevant journals in the field of IB.

2 Some Examples of IB's Loss of Practical Relevance

In the introduction section of this essay, we have discussed the relevance question more generally as it is valid for all subfields of business administration. We argued that answering this question will always require a specification of the respective target group. It has to be defined out of which perspective the relevance problem is viewed. If the relevance question will be discussed out of a researcher's perspective, this will lead to a different answer than if it will be discussed out of the perspective of business practice. While we fully understand that non-tenured faculty members have to conduct research activities in a way so that they can help them to get tenure, we think that it is highly problematic if most business administration research and therefore also IB research is oriented toward this direction. If business administration research *as such*—and thus IB research also—will go this way then it will ignore the need to deliver research output being helpful for the decision-makers in business practice.

Furthermore, in the introduction section, we argued that this causes the need to deliver research output that practitioners perceive to be interesting. If IB researchers will provide knowledge practitioners consider to be uninteresting then IB research will not have any impact on MNCs' actions. Of course, not all IB research has to be practice-oriented. But if (nearly) none of it is practice-oriented, the IB community has a severe problem.

In the following, we will present examples showing that during the last decades IB research has lost its close linkage to the business practice.

Example 1: Research on the Management of MNCs

Until the mid of the 1990s most research on the management of MNCs was focused on MNCs' strategy and organization issues. In the strategy area, quite many studies explored MNCs' *strategic orientations* (e.g., Doz, 1980; Porter, 1986). These studies tried to specify under which firm-external and -internal conditions which type of strategic orientation (international, multi-domestic, global, blocked global/transnational) fits best. *By doing so, this strand of research corresponded heavily with decisions MNC managers were responsible for.* It helped them to formulate MNCs' strategies solidly. These days, a main aspect of the research on MNCs' *organizational issues* were coordination instruments MNCs' headquarters and subsidiaries inserted to ensure and improve the alignment between MNCs' subunits (headquarters vs. subsidiaries; subsidiaries among themselves). This research analyzed both formal and informal coordination mechanisms. On the one hand, there was a substantial and growing body of knowledge being focused on formal coordination mechanisms. Here the research on MNCs' organizational structures (e.g., Daniels et al., 1984; Egelhoff, 1982; Stopford & Wells Jr., 1972) was most prominent. This so-called international strategy-structure research clarified under which MNC strategy which organizational structure tended to fit well. *Again, by doing so, this stream of research corresponded tremendously with decision and choice questions MNC managers had to deal with.* In this period, studies on informal coordination mechanisms were also focused on approaches that could be used by MNC managers. For instance, there were quite many studies questioning in which contextual situation informal coordination instruments like manager transfers, visits, or corporate culture should be inserted. *Again, this research had a close linkage to MNC managers' decision situation*: They had to decide how intense their MNC shall use these subtle coordination instruments. Even Bartlett and Ghoshal's (1989) work on the transnational solution followed this idea since it argued that in blocked global industries, which became more and more typical at these days, an organizational form called "transnational solution" would fit best. *A commonality of all these topics of research was that the studies were focused on instruments MNC managers were able to insert.*

But then, in line with Kogut and Zander's (1992, 1993) seminal publications, this "instrument-oriented" research started to get replaced by studies having a more social-scientific, non-design-oriented nature. Some of these studies shall be mentioned here as examples for this new research epoch: Gupta and Govindarajan (1991) analyzed differences in subsidiary contexts along two dimensions: (a) the extent to which the subsidiary is a user of knowledge from the rest of the corporation and (b) the extent to which the subsidiary is a provider of such knowledge to the rest of the corporation. Based on this, subsidiary archetypes were described. A few years later, Zander and Kogut focused narrowly on selected facets of intra-MNC knowledge transfer, e.g., tacitness of know-how (Zander & Kogut, 1995). In 2000, again Gupta and Govindarajan investigated both theoretically and empirically the determinants of intra-MNC knowledge flow patterns. They conceptualized and tested hypotheses like the following: "ceteris paribus, the higher the level of the host country's economic development relative to the home country, the greater will be the knowledge outflows from that subsidiary to the parent corporation" (Gupta & Govindarajan, 2000: 478). Minbaeva et al. (2003) mainly referred on the concept of absorptive capacity as a key factor explaining the success of MNCs' knowledge flows. These authors suggested that absorptive capacity should be conceptualized as being comprised of both ability and motivation. *In comparison to the older instrument-oriented studies, the newer knowledge-transfer-oriented studies are more descriptive and academic, and they deal with more abstract constructs. By doing so, the results of these studies are more difficult to transfer to the business practice.* For instance, for MNC managers it is hard to assess the tacitness of knowledge, to estimate the volume of knowledge flowing in and out of their subsidiaries, or to identify promising ways to increase a subunit's level of absorptive capacity. *As a consequence, this type of research has a more indirect practical relevance, if any.*

Example 2: Research on Distance as a Central Explanatory Variable
During the last 25 years, many IB studies have taken "distance" as a key variable of inquiry. As a consequence, distance became a key concept in IB research. Most studies focused on the distance between MNCs' home country and the respective foreign country where the MNC is doing business. Without any question, the Kogut and Singh (1988) article on the influence of national culture on the choice of foreign entry mode was a cornerstone of the "distance research" since it suggested a frequently used method to measure cultural distance (Konara & Mohr, 2019). While over the years the conceptual focus of distance has changed and widened significantly (e.g., psychic distance, geographical distance, political distance, institutional distance), the general logic of this research stream tended to remain stable, since distance was constantly seen as a key predictor of the behavior of MNCs and their managers, and of the success of MNCs'/managers' actions. For instance, Morosini et al. (1998) rejected the standard assumption that national cultural distance hinders cross-border acquisition performance. Instead, they found a positive association between national cultural distance and cross-border acquisition performance. Brouthers and Brouthers (2001) took the inconsistent results on the relationship between national cultural distance and foreign entry mode choice (some scholars found high cultural distance associated with choosing wholly owned modes while others found high cultural distance linked to a preference for joint ventures) as a starting point for their own research. They found cultural distance to be related to both types of foreign entry modes. Given this, they theorized and tested that the level of investment risk in the target country can help to explain the choice between these entry modes. Consistent with this finding, Tihanyi et al. (2005) meta-analysis showed that cultural distance itself is not a significant predictor of entry mode choice. Instead, moderator effects were able to lead to significant results. Dow and Karunaratna (2006) focused on psychic distance measures as predictors of trade flows. They found that the most common psychic distance surrogate—a composite measure of Hofstede's cultural dimensions—is not a significant predictor of trade flows. Berry et al. (2010) disaggregated the construct of distance by proposing a set of multidimensional measures, including economic, financial, political,

administrative, cultural, demographic, knowledge, and global connectedness as well as geographic distance. Further, they suggested to use the Mahalanobis measure instead of the Euclidian distance measure. They provided evidence that the suggested distance measurement method is more powerful to explain MNCs' foreign expansion choices. Shenkar (2012) identified several conceptual illusions (illusion of symmetry, illusion of stability, illusion of causality, and illusion of discordance) and problematic methodological assumptions (assumption of corporate homogeneity, assumption of spatial homogeneity, and assumption of equivalence) in IB's research on cultural distance. Harzing and Pudelko (2016) scrutinized the explanatory power of the concept of (cultural) distance. Based on a review of 92 prior studies on entry mode choice and an own empirical investigation they concluded that the explanatory power of distance is highly limited.

Given this heterogeneity of findings and the fact that the results vary heavily by the measurement of distance, out of the current hindsight perspective it is really interesting to see how long and resistantly IB research has considered distance as a key predictor of MNCs' foreign business activities. We are pretty confident that a key cause for this is the quite easy access to distance-related data. This means that many scholars have modelled their research activity based on the availability of data and not on a careful musing on the decision process MNC managers typically go through. Or in other words: Do we really think that an MNC manager, if s(he) has to make a decision which foreign market to enter and which foreign market entry mode to use, above all analyzes types of distance existing between the home country and the host country?—No. Instead, managers being responsible for foreign market entry (decisions) mainly think in categories like market size, market potential, purchasing power, necessary investment intensity, degree of rivalry in the foreign market, or other factors characterizing the host country market as such and not the distance existing between the home and the host country.

There is a further reason why the extreme focusing of current IB research on quantitative distance measures is problematic: The more IB research has concentrated on quantitative distance measures, the less it has studied qualitative characteristics of the respective foreign country where MNCs want to do business. Unlike the current way of IB research, MNC managers, if they consider to start a business activity in a foreign country, have to perform a detailed analysis of different kinds of characteristics of the respective country. They have to analyze the country in depth. Do IB researchers, if they work with quantitative distance data (e.g., a calculation of the political distance between the home country and the foreign country with "4", the geographical distance with "2" and the social distance with "3," etc.), provide a picture of the host country, which is sufficiently multifaceted and informationally rich enough, so that this can be used as an information basis for managers to start business activities in this foreign country?—No. We think that the reductionist information provided by most of the contemporary IB research is of quite little help for business practice. By working with such highly integrated distance-oriented data, current researchers have gone away from a key strength of traditional IB research which was to deliver detailed insights into the peculiarities of foreign markets and foreign countries.

All in all, we think that the IB community has overheated the intensity of studying the distance variable. By doing so, it has created a further gap between its own research activities and the decision problems MNC managers have to deal with.

Example 3: Dominance of Non-Replicable and Weak Relationships in IB Research

Before we will start to focus on our third example, let us recap which kind of research results managers want to receive from business administration research. As mentioned in the first section of this essay, managers want to receive interesting and reliable research results which provide information what to do so that their firm's long-term success will be supported. These results shall refer to levers that have a substantial impact on the firm's success.

If we compare this kind of demand with the kind of research output contemporary IB research is providing, a noticeable gap seems to get obvious.

(a) First, without any doubt, during the last decades IB research (like other fields in business administration research), has made a development toward narrower, more focused conceptual frameworks. As a consequence, if a contemporary empirical IB researcher refrains from conceptualizing and testing moderated relationships, (s)he will receive substantial critique by journals' reviewers. As a further consequence, (s)he tends to have no chance to get her(his) work published in a decent IB journal. Of course, there are cases where a specification of moderated relationships seems to be necessary (e.g., if previous results have led to inconsistent findings), but it is quite clear that current IB research is not limiting the conceptualization and testing of moderated relationships to such special cases. Instead, the specification of moderated relationships became a fashionable trend, i.e., factual requirements have not initiated them. They are frequently used although, in the relevant field of study, no inconsistent findings are reported. Further, a large portion of moderated relationships seems to be developed in a data-driven manner and they are "made plausible" by contrived logics. Many of these relationships seem to be complemented by post-hoc logics. This fashionable trend toward the use of moderated relationships is highly problematic, since they are typically supported in the studies in which they are introduced, but they are rarely confirmed in subsequent studies. Since most moderated relationships are not well confirmed, they derogate the robustness of IB's research's body of knowledge and this, in turn, will lead to research findings MNC managers cannot trust (for further aspects of science's reproducibility and replicability crisis see Aguinis et al., 2017).

(b) A further reason for the existing gap is that empirical IB research, unlike other fields of management research, has continued to sharpen its attention to quantification (Delios, 2017). While other fields of the academic world started to question "the value of kowtowing to the 0.05 deity" (Delios, 2017: 392), the IB community has not done so yet. In the era of big data this is especially problematic since, because of huge numbers of observations, even extremely weak relationships between variables will master to skip over such threshold values. Again: Do we really think that MNC managers are interested in levers having a *minimal* influence on the efficiency and effectiveness variables they want to steer?

3 IB Research as a Closed System? A Plea for a More Intensive Look Beyond the Borderlines

Already a first, only superficial look on sources used by papers that have been published in high-ranked IB journals (JIBS, JWB, GSJ, IBR, JIM, but also in our "own" journal MIR) leads us to the impression that the majority of those sources is originated in the field itself; furthermore and to a (much) lower extent sources stemming from a closely related discipline, i.e., (international) economics seem to be used.

However, the conclusion out of this impression that there are likely only few other scientific disciplines that are interested in problems international firms have to face and solve would be wrong. This is because there is a) indeed a number of other sub-disciplines of business administration and management that are also interested in research on problems and challenges international firms are facing; and b) beyond business administration and management further scientific disciplines can easily be identified that are also—at least partially—dedicated to the research on international firms. As so far John Dunning's already in 1989 formulated plea for a more interdisciplinary approach of studying IB has still not reached the full extent of realization (Dunning, 1989: 411 et seq., especially 430).

In the following, we would like to discuss IB-specific problems originating from a too strong discipline-focused sourcing of research outcomes. In this context, we also provide examples of other scientific disciplines that are interested in research on international firms, too. Thus, they should be viewed as further sources of knowledge on the object of research, i.e. the international firm.

Research on FDI and respective location choices can be labeled as one of the core fields of IB research (Blonigen, 2005; Paul & Feliciano-Cestero, 2021; Paul & Singh, 2017; Werner, 2002). Therefore we take this field as starting point of the following thoughts. As we know from the real business world international location choices are not only guided by the availability of resources, market size, or production cost advantages, but also by taxation differences. However, taxation issues and according location choices seem to be still a not very well researched phenomenon in IB. As so far research published in journals dedicated to international taxation could be exploited and employed in a much stronger way.

However, the focus on research results stemming from the own field, i.e., IB, is as so far expectable as business administration and management are scientific disciplines that show after much more than 100 years of existence in the USA and Europe (Engwall & Zamagni, 1998; Wren & Van Fleet, 1983) a high degree of specialization. Yet, when specialization leads to a concentration of sourcing knowledge only from the knowledge pool of the own sub-discipline scientific progress can be slowed down. This is because "foreign knowledge" that has value for the problem under research in the field of IB has lower chances to be discovered and to be employed. As so far it is not surprising that core problems of IB, e.g., FDI, HQ-subsidiary-relationships, implementation of Regional HQs, or internationalization as process are approached/encountered only by a rather narrow set of theories (for internationalization see especially Surdu et al., 2021: 1047) established in the IB field and thereby not taking into account that also taxation, financing, or management accounting issues can influence the decisions of managers and the aforementioned outcomes.

The phenomenon of overlooking "foreign" knowledge seems to be even more existing when we focus our discussion on knowledge that could be sourced by IB scholars from more distant disciplines like international economic geography, international (political) relations, or sociology. Those disciplines are also treating problems of the international firm; however, they are approaching the problems most likely via perspectives being not (exactly) those of IB.

As so far problems of the complex real business world are viewed by different disciplinary perspectives (Cheng et al., 2009: 1070 et seq.), but IB up to now does not show very strong interest in employing such different perspectives. By doing so the chance to describe and explain existing IB problem in an integrative way will not be used fully, making our identification of research questions, the work for respective answers, and finally our solution-oriented offers to practitioners not that powerful as they could be. Given this our plea is to look more intensively beyond the discipline-oriented borderlines in order to scan and to employ knowledge of other disciplines to elaborate stronger and thereby for practitioners more attractive and relevant descriptions, explanations, and practice-oriented solutions of real business world problems.

4 The Basic Relevance Problem of Journals (and Potential Authors) in the Context of Submissions

All major IB journals publish well-defined Aims and Scope statements. Scholars being interested in submitting their research papers to one of these journals should know the rules of the game, i.e., they should be able to assess if their paper fits the Editorial Policy (Aims and Scope) of the respective journals. In other words: They should be able to judge if their research is relevant to the respective journal. But obviously, this is not the case. Because we have no respective data of other journals we feel free to describe in general the situation of MIR. During the last years we received not only more and more submissions (not only due to the fact, that scholars of more nations (e.g., PRC or Brazil) are now interested in the IB field). We also receive a growing number of submissions that are not relevant to MIR, since they do not meet the journal's Editorial Policy. As an IB journal, MIR is only interested in research dealing with IB problems, but not in research dedicated to other topics like the effectiveness and efficiency of quality management systems in country A.

Some years ago there was an interesting debate initiated by Jean J. Boddewyn (2016a, 2016b). This debate followed a tradition established by Nehrt, Truitt, and

Wright already in 1970 (Nehrt et al., 1970). It tried to specify which requirements should be met that research is truly IB research. But this interesting debate and its outcomes seem to have not reached many of those being interested in publishing in IB journals. This is, because beside submissions dealing definitely not with questions of IB we receive submissions that are based on international data but do not analyze for an international dependent or independent variable. Such submissions do also not fit the Editorial Policy of MIR (and most likely that of other IB journals).

As so far we would like to motivate researchers being interested in problems of IB to assure themselves first if they have a perspective on IB that is in line with the field's broadly accepted definition of (content-wise) objects of research. And second they should develop a closer, more precise look at the Aims and Scope statements of journals to ensure that a potential submission is—in a basic way—relevant to the respective journal.

5 Summary: What To Do?

In the current contribution, we have discussed the state of IB research. We have analyzed scholars' research behavior and especially IB's relevance issues being a consequence of this behavior. In the contribution's first section we saw that relevance is not an absolute concept. Instead, its meaning depends on the perspective the respective person involved actively or passively in the research process is taking. Further, we have learned that especially with respect to the research process itself a field's social structure and its social processes heavily influence the predominant meaning of relevance. In the second section, we have presented some examples indicating that IB research's practical relevance has abated over the years. While earlier IB studies were heavily focused on decisions practitioners had to make, more recent IB research has a stronger social-scientific nature. Design-oriented issues are not that important in contemporary IB research. If we wanted to be more pronounced or even provocative, we could say that recent IB research tends to be rather sociology or economics of the international firm than part of business administration as it was understood over decades. Of course, IB scholars themselves have to decide if they want to make their research more practically useful in the future. If they do so, they could consider hints as they were suggested in the literature already quite long ago (e.g., Wolf & Rosenberg, 2012). Especially, we have to intensify our contact to practitioners being responsible for international firms. Otherwise many IB scholars will continue to study phenomena being not very important to the business world and they will run the risk of conceptualizing the phenomena under study wrongly.

Yet, what is questionable and disturbing is the fact that in the IB field there are many studies that conceptualize and test relationships that never got confirmed in subsequent studies. This is in sharp conflict with science's goal to provide not only interesting but also reliable knowledge. And we also have to lament that the field's strong tendency toward quantitative analyses has led to a state where qualitative aspects of IB got underemphasized. This is problematic since, for an international manager, it always had been central to consider and to deal with qualitative peculiarities of the foreign environments. In the contribution's third section we diagnosed that the community of IB scholars is still quite closed. This is disadvantageous since in both the studied phenomenon itself as well as in adjacent academic disciplines there are quite many issues, topics, and concepts that can and should be considered more by IB scholars in the future. The fourth section provides strong hints that in IB's scientific community there seem to be quite many scholars who obviously have never carefully thought about general aspects and the boundaries of the IB field. Many seem to start IB research activities without having ever thought about such fundamental questions relating to the IB field. Otherwise, journals like MIR would not get so many submissions being clearly outside of the journal's scope. That said, we want to encourage our colleagues to solidly muse on the content of the field they are belonging to or they wish to belong to. This seems pretty easy to do, since the established literature (see our references) has delivered many contributions dealing with this question.

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