RELEVANCE IN INTERNATIONAL BUSINESS RESEARCH: A NEED FOR MORE LINKAGES

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Editor's Note: This paper is the address presented by John D. Daniels, outgoing president of the Academy of International Business, in October 1990, in Toronto, Ontario.

The bi-annual presentation by the outgoing AIB president has become traditional. It is not only an honor, but also an unusual opportunity to address an audience composed of international experts in all areas relating to international business. It is also a perplexing challenge to choose a topic that should be of general interest, without departing too much from one's own field of specialized interest. Fortunately, the AIB officers agreed on a theme for this year's meeting, "Relevance in International Business Research," which presented me a topic to discuss.

Although themes for annual meetings are somewhat futile because people submit papers and panel proposals based on work they already have in progress rather than changing research directions to fit a particular meeting, I, nevertheless, supported the relevance theme. First, I thought that "relevance" was broad enough to fit almost any proposed paper or panel, because, notwithstanding, we tend to think our work is relevant, even if just within our university reward systems. Second, the last outgoing presidential presentation, "The Study of International Business: A Plea for a More Interdisciplinary Approach," focused largely on research; and it has subsequently been discussed extensively [Dunning 1989]. Thus, an exposition on research relevance allows me to amplify and elaborate on this ongoing dialogue. Third, the relevance of business school research in general is increasingly being questioned. For example, Dean Scott S. Cowen of Case Western Reserve was recently quoted as saying "As much as 80% of management research may be irrelevant" [Byrne 1990]. Fourth, our holding of this meeting in Canada, the country that gave the world the popular "Trivial Pursuit" game, is perhaps symbolic for reflecting on how even the seemingly irrelevant might be made relevant in international business research.

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.

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These divergent comments have been enlightening and analogous to an anecdote about Henry Ford. During a chance meeting of a visitor to the Ford Motor Company factory, he pointed to a finished car and proudly declared, "There are exactly four thousand seven hundred and nineteen parts in that model." The visitor was impressed that the company president should have such details at his fingertips, but was somewhat skeptical. He subsequently asked the company engineer if the statement were true, whereupon the engineer shrugged his shoulders and said, "I'm sure I don't know. I can't think of a more useless piece of information."

NEED FOR MORE LINKAGES

My opinion is that change or improvement must come out of the research process for the research to be relevant. This view is broader than it may appear at first glance. The so-called "change" may be no more than a confirmation, where there was some previous doubt. Furthermore, the timeframe can be open-ended, because historically some basic observations and/or theories (such as the rising of hot air or the spherical nature of the earth) were necessary precursors to concrete improvements that did not come until centuries later. Therefore, research relevance usually involves a linking process, such as among basic, applied, and developmental (commercialized) research, between micro and macro analysis, between theoretical and empirical examinations, and between descriptive and prescriptive examinations. To quote a line from Longfellow's "Hiawatha," they are "useless each without the other."

Most of the recent discussion on the relevance of business school research has centered on finding some balance between the theoretical and the vocational. Dean Thomas P. Gerrity of the Wharton School summarized this concern when he said, "The right answer is balance. It's not one way or the other. Business schools can't lose sight of their fundamental intellectual foundations" [Byrne 1990]. Although I do not disagree with this opinion, I find it incomplete because there is no explicit regard for a linking process.

Many of us have simply become so specialized by function, methodology, or topic area that change or improvement from our research comes slowly—if at all. Although we sometimes think that this problem is unique to international business, because of the cross-functional nature of many of the problems or issues that we study, it is not. For example, Andrew H. Van de Ven pointed out the problem for the functional area of management when he said, "We now have many theories competing with each other to explain a given phenomenon. Proponents for each theory engage in activities to make their theory better by increasing its internal consistency, often at the expense of limiting its scope. As a result, a way of seeing is not seeing. Such impeccable micro logic is creating macro nonsense!" [Van de Ven 1989].

There are, of course, numerous linkages that may be pursued—some collaborative and some not—but I shall concentrate on only four of these:

(1) the spectrum between basic and developmental research; (2) the connection between past and present research; (3) cooperation between international and non-international specialists; and (4) collaboration among scholars in different countries. Although these linkages are discussed separately, they are not always mutually exclusive. The important linkage in interdisciplinary research was covered extensively by John Dunning two years ago and was later published in *JIBS*; therefore, I shall make very few remarks on this subject. Although my emphasis is on research, it is sometimes difficult to separate research relevance from curriculum or even AIB relevance; consequently, the discussion will occasionally digress to these latter areas.

Basic to Developmental Spectrum

There has been considerable work in recent years on the technological diffusion process, particularly relating to national competitive positions and strategies. The emphasis has been on research in the so-called "hard sciences" rather than social sciences; and there is controversy surrounding almost every proposition that has been presented. Nevertheless, there are a few lessons that may apply to IB research.

First, basic research, including theory building, must usually come before applied research, which must come before developmental (commercialized) research. Some backlog must exist at each preceding stage if there is any hope of moving along the spectrum. But if the backlog becomes too large, the situation is akin to holding excess inventory; if too small, there may be, in effect, an inventory outage. The ideal situation, therefore, is similar to a just-in-time inventory system. Japan, for example, traditionally put little effort in basic research and a great deal in the applied and developmental ends of the research continuum. The demonstration of Japan's position is supported not only by anecdotal evidence but also by the low proportion of scientists in relation to engineers who have either graduated from Japanese institutions or have been employed by Japanese firms. This strategy worked well as long as there was a storehouse of basic and/or applied data that could be transferred easily into Japan. But, more recently, Japan has realized that bottlenecks in basic technology necessitated a redressing of its educational system so that its future production of scientists will be approximately the same as the number of engineers.

Second, if the diffusion process is to proceed effectively, adequate communications linkages must exist among the parties specializing in different types of research, such as basic versus applied. For example, diffusion studies often point to the United Kingdom, one of the world's biggest per capita R&D spenders, as a country that has lagged behind some other industrial countries in receipt of benefits from its own R&D for either the British economy or for British companies. For instance, Britain led in early research, perhaps through the applied stage, on such products as jet engines and television, but fared poorly in the linkage to commercialization. This situation has been attributed in part to ineffective communications linkages between scientists and engineers who generally attend different educational institutions, develop their own specialized vocabularies, and are viewed somewhat differently in terms of occupational status.

Third, positive incentive systems are necessary for the diffusion process to work successfully. The Soviet Union, for example, may have as many as a quarter of the world's scientists and engineers; however, there has heretofore been little inducement for technological developments that lead to change or improvement.

How does international business research relate to the above three generalizations on diffusion? As part of business school research in general, it is more likely to be in the applied than in the basic or developmental part of the spectrum. This is probably true, regardless of whether the research is theoretical or empirical. Within the business school, international research specialists are involved largely in the bottleneck of the diffusion process. When basic underlying theories and techniques are developed in such disciplines as psychology, political science, economics, and math, the international business specialists are seldom the first to apply the theories and methodologies to business. Instead, there is typically an adaptation and testing of theories and methods on domestic business questions before application to international ones. The intervening time lag between domestic and international research can be quite long. In fact, the domestic theories are frequently applied and modified by businesses to their international operations before the international research specialists consider applications. The international researchers may thus be in the tenuous position of following, rather than leading, the developmental process.

How might this bottleneck be better addressed? There are a number of possibilities. INSEAD ran a workshop in 1989, in which organization theorists and international management specialists explored ways either to expand theory or to apply existing theory internationally. Such an approach certainly has possible future merit. Some business schools are also hiring more discipline-oriented faculty in addition to business functional specialists to speed the overall diffusion process; however, this does not specifically address the international issue. Perhaps the AIB could consider workshops to bring discipline specialists and international business faculty together, with the specific objective of applying new theories and methodologies more quickly.

Much of the work in international business is descriptive rather than prescriptive, even at the theoretical level. The emphasis on descriptive analysis is due, in part, to the leapfrogging from academic work in a domestic context to business's own applications internationally. There are undoubtedly some other factors as well, such as impediments for researchers to gain early access to the inner workings of international companies' operations, which are the laboratory for experimentation.

The descriptive work is certainly relevant; and much may lead to recommendations, particularly when performances are compared and when a variety of approaches are discussed. But perhaps a greater emphasis should be placed on prescriptive theories. Given the rapid global changes that are taking place, businesses seem more eager than in the past to familiarize themselves with new theories and techniques, even though they realize that not every theory will have merit. Such an emphasis on business application need not reduce the scholarly value of the research. At the same time, it may further serve the students and companies, who constitute part of the business school's clientele.

Of course, the rigor of refereed academic publishing preferences empirical testing through formats and methods that are acceptable by the mainstreams of academia, an especially important concern for the international business field, which still faces some credibility problems. Nevertheless, although the paramountcy of scientific testing may remain, there is a need for more outlets for a wider range of "think pieces." These may serve to stimulate mainstream empirical research. As members of faculties, professional associations, and journal editorial boards, we are in positions to influence the placement and positive review of papers. Unless more of these think-pieces are accepted in the vehicles that are important for promotion, tenure, and reputation, there will be insufficient incentive for their undertaking. These comments do not imply that no thought-provoking qualitative-type papers are published. There are some, such as Maruyana's examinations of Western management theories in the context of other cultures [1982]; however, there may not be enough. One may even suspect that Adam Smith's specialization-production efficiency concept might not presently be accepted in some of the more prestigious journals because he neither tested his theory empirically nor built a mathematical model of it. In fact, it took almost 200 years for learning curve analysts to confirm his thesis.

Not all theses need to be tested. Hall's depiction of a silent language was a pioneering work that has endured [1960]. Although there has been a lack of testing about some of the cultural differences he discussed, testing may not be necessary. His observations, like those of other informed scholars, serve to stimulate the thinking of both our managerial and academic clientele.

An ideal compromise is a collaboration between people who are primarily theorists and people who are primarily empiricists. For example, the early work between Richard Farmer, a theorist, and Barry Richman, an empiricist, was synergistic and has survived better than the later work that each did individually. Collaborations could occur between international specialists or between an international specialist and someone from a similar or different functional or discipline area.

Past and Present Research

Some of the physical science meetings hold regular overviews on the current research state for specific areas. Presenters are usually senior scholars, and to be picked as a presenter is prestigious. A similar approach has merit for

AIB. We experimented for the Toronto meeting on a limited basis; and the AIB Fellows have adopted an annual state-of-the-art presentation as an ongoing project. These presentations should include an extensive historic overview of research in the area; and topics should be broad enough to cut across functional and disciplinary interests.

An extensive literature review serves as an update or primer for people, who although not actively researching an area, are nevertheless involved in teaching about it. The review also serves as an accelerated means for academicians to enter a new research area without "reinventing wheels." Much current research effort seems to be wasted because earlier research has somehow disappeared into the oblivion of a "black hole." The most regrettable wastage is when enthusiastically completed research is unpublishable because of being insufficiently different. Even much of the published research on certain IB questions seem déjà vu, inasmuch as the questions, methodologies, and findings are very little changed over time. This criticism does not imply that one should not reexamine questions which have been studied before. Situations may change. Data availability changes. New explanations can account for old phenomena. But if one does not realize that the road has been traveled before, then one risks overlooking the best way to traverse that road. State-of-the-art presentations may help numerous scholars to move faster into areas new to them and to ensure that their research contributes beyond the current state.

Aside from special AIB presentations, we might all consider, at least for our doctoral students, the requirement of a more thorough historical literature review on any study topic. Many of us are probably somewhat remiss in including references to earlier work in our course outlines and texts. Since we must be selective, we tend to preference the latest work so that we appear as up to date as possible.

Having state-of-the-art presentations that are broad helps overcome the "blind men and the elephant syndrome." Just as the group of blind men felt and interpreted different parts of the elephant's body and ended up with no description that resembles that animal as a whole, research sometimes does the same. The quest for knowing more about less sometimes leads to very accurate views of the parts, but obscures that these parts are of some "whole." Although there are some treatises and text chapters that pull the parts together, there is a need for more recognition, even when researching a narrow subject, that this research is attached somehow to a larger system. *JIBS* did a commendable service by soliciting and publishing papers (Spring-Summer 1981), which summarized research in particular areas. We need more of this.

International and Noninternational Specialists

Part of the "raison d'etre" for studying and researching international business has been based on contentions that (a) political and economic conditions are more subject to rapid change in the collective global environment than in any single country environment, and (b) these changes have widespread ramifications apart from where the changes take place. Recent events have provided much ammunition to persuade even the most domestically oriented thinkers of these rationale; and they have been further prodded by their public and private sector constituencies to think globally. To highlight a few of the major events, the Berlin Wall came down, and Germany is reunified. The trade embargo on Iraq is probably the most widespread in history. Privatization is occurring in places that one would not have predicted a short time ago. President Bush has proposed a western hemisphere free trade agreement which no Latin American leader has as yet rejected outright. Eastern European countries, even Albania, have increased their openness to the West. North and South Korea are planning to field a joint Olympic team. Nelson Mandela has been freed. Free elections have changed the political-economic direction of Hungary and Nicaragua. Oil prices reached an all time high. The Nikkei stock index fell 39%. Earth Day was popularly celebrated globally. Baltic independence declarations were countered by economic, as opposed to military, responses. Hundreds of thousands of foreign workers in Iraq and Kuwait are either stranded or fleeing. Italy and Japan, for perhaps the first time ever, are soliciting the entry of foreign workers. The Panama Canal closed, albeit for a very short time. Iraq and Iran ended their war.

One need not go on. But a result is new international research involvement by respected leading scholars, who heretofore took no international perspective on their particular disciplines or functional areas. Their international forays are, however, not receiving unanimous favor among many of the long-term internationalists. They talk of the "Johnnys come lately" who are involved opportunistically and who will abandon the international area when other currently glamorous research areas come along. They talk of "closet-internationalists" who did not come out to fight for the field before it became acceptable. The real concern behind many of these comments, though, is probably that one's domain might be lost if everyone is including international perspectives in courses and research.

Frankly, I don't worry about the domain issue. As an analogy, every functional area addresses legal questions and uses quantitative techniques; but we still need specialists in law and quantitative methods. Nor do I worry about whether international business should be a separate department or a part of each functional department. In offering a second analogy, multinational companies have found methods to operate successfully abroad with a variety of organizational structures to encompass their international activities. The situation may be interpreted as an enlargement of the international business group, rather than the absorption of the international area by others. We may be at an opportune time to get more support for some of our individual pet internationalization projects, such as requiring that all doctoral students understand the international nuances of their functional specialties. In other words, there is now a great chance that internationally committed faculty can take a leadership role.

Two ways of working more closely with international newcomers are through joint research, where there are undoubtedly synergies, and through serving as facilitators. Many newcomers to international have recently worked much more closely with specific theories and literatures within our functional specialties than we have. At the same time, we probably have a better perception of the international applicability of their theories and approaches. We also usually know the international databases better, have established networks with colleagues in other countries, and know better how to deal with some of the international methodological problems. A personal example should illustrate a benefit of collaboration. Two full professors in my department recently became involved for the first time in the administration of an attitudinal questionnaire in more than one country. (As an aside, I could not help but be amused when I overheard their comments that foreign data collection was more difficult.) I was able to put them in contact with academicians in Hong Kong and Japan who were pleased to administer the questionnaire to a subset matching the United States' sample. Furthermore, I gave them an article dealing with the methodological problems of interpreting scaler responses from different countries, an issue that they had not considered. The end result should be their much better appreciation of existing international business research.

Cross-National Collaboration

Most international specialists have networks of colleagues in different countries. One of AIB's strengths is in its increasingly international membership; currently 29% live outside the United States. AIB is committed to hold meetings in different parts of the world. Officers can and do come from anywhere. AIB is trying to take a truly global orientation rather than using a confederation of national or regional groups, while still holding meetings at regional levels. The amendment to permit lower dues for scholars in the world's poorest countries has passed and may be useful as an initiative to internationalize membership even further.

At the same time, AIB members are probably not fully utilizing the research potential from the international membership. For example, the Toronto meeting has approximately 125 papers, of which only six are coauthored cross-nationally.

Even among these six, the collaboration may have been more fortuitous than directed toward gaining a cross-national synergy. For a group that has been at the forefront of joint venture and strategic alliance studies, the situation is somewhat surprising. Of course, not all studies benefit by collaboration, much less internationally. Nevertheless, a number of areas of joint effort might result in a more efficient data collection and in more meaningful findings. Comparative studies easily come to mind. In addition, studies dealing with parent-subsidiary relationships might enable a researcher in one country to collect data from headquarters personnel, while a researcher in a second country collects data from subsidiary personnel. Strategic alliance studies offer possibilities whereby one researcher studies licensing or joint venture partners in one country, while a second researcher studies the partners elsewhere. To date, there has been little of this cooperation. For example, I recently completed a project on U.S. joint ventures abroad by U.S. high technology firms and talked only to U.S. parents. Had there been collaboration with colleagues abroad to collect similar data from the partners, the findings may have been much richer.

In the not too distant past, joint cross-national research efforts were problematic because of communications. Today, AIB members see each other at various meetings, are connected by fax and Bitnet networks, and know each other well enough to judge research effort integrity.

SOME THOUGHTS ON CONTENT RELEVANCE

Topical relevance is a particularly problematic subject because one must prophesize what will be important some years in the future and build research and teaching programs around the prophesies. In this respect, I have two observations. First "hot" research issues in international business tend to be cyclical. For example, strategic alliances (although not called by that name at the time) were highly topical in the late 1960s and early 1970s, became less popular, and then reemerged in popularity by the late 1980s. Studies on the effects of MNEs on host societies were in vogue during the late 1970s, are currently out of favor, but will undoubtedly make a comeback. This leads me to conclude that a broad breadth of research is permanently relevant, although not obviously so during temporary periods. Second, of the two basic philosophies on the future—that you can predict what it will be and that you cannot—the latter is gaining in significance because of the recently dynamic international environment. This leads me to conclude that in international business research and teaching, breadth is again extremely important. Furthermore, we need to make more use of relevance trees or scenarios that postulate all the foreseeable future directions so that decisionmakers may be aware of the best and worst things that can affect them. Model building is probably relevant whenever an occurrence-probability is greater than zero.

These conclusions deal only with content. We also need to address the question of content versus process, an issue that cuts across international business research and teaching. Given rapid changes, should we be expending more efforts on teaching and researching about interpersonal awareness and adaptability as opposed to content-specific international business knowledge? At AIB annual meetings, the few pedagogy sessions tend to deal primarily with content delivery. Perhaps we should foster a workshop on the more fundamental question of relevant learning objectives at least in terms of content versus process.

As I look into the near future, I am as dismayed as most of you probably are with what seems to be an unusal amount of uncertainties and anomalies. If Dickens were around and writing an introduction to "A Tale of 160 Some Countries," his earlier words of "It was the best of times, it was the worst of times, it was the age of wisdom, it was the age of foolishness," etc. would seem even more appropriate than when he wrote about early nineteenth-century France.

On one hand, we see a 1990 World Bank projection of decreased world poverty; on the other hand, we see more recent high oil prices and possible higher interest rates causing even more acute LDC problems. On one hand, we see a growing disdain for government price controls in the now transitional economies; on the other hand, we see a clamor for strategic trade policies, oil price controls, and national education systems in some of the bastions of free enterprise. On one hand, we see a growing opinion that centralized decisionmaking is inefficient at the government level; on the other hand, we see a growing opinion that almost unbridled centralization will work for MNEs. On one hand, we see almost unparalleled global cooperation vis-à-vis Iraq's invasion of Kuwait; on the other hand, we see little progress on trade liberalization through the current GATT round after four years of negotiations. On one hand, we see disillusionment with central planning; on the other hand, we see cases of central planning being used to eliminate central planning. On one hand, we extol the Japanese practice of evaluating business performance on a long-term basis; on the other hand, we try to evaluate countries' economic performance (particularly balance of payments) on ever-shorter time frames. On one hand, we see the global flow of information leveling cultures; on the other hand, we see a rise in ethnic identity and conflict in many countries.

Yes, I think that Dickens would repeat his words, "it was the epoch of incredulity."

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