CHAPTER 27

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# METHODOLOGICAL CONTRIBUTIONS IN INTERNATIONAL BUSINESS AND THE DIRECTION OF ACADEMIC RESEARCH ACTIVITY

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It is one of the best-kept secrets of research that a methodological contribution is the most powerful engine for the replication and diffusion of an idea. Radical organizational innovations are far more influential than technological ones for the same reasons. Because they serve as complements to a wide array

of industrial techniques, they can diffuse rapidly across the boundaries of a sector. Similarly, a major innovation in research design is far more powerful than a single technique or isolated idea.

The development of the field of international business has been strongly driven by innovations in research design and methodologies. I would like to emphasize this role in this concluding chapter in order to suggest to researchers, and to our doctoral students, that progress is engaged when a community collectively is able to ride upon common methods, schemas, and templates. The tendency in some fields, such as strategy, has been to suffer from the incentives for a proliferation of terms that represent the same idea. We thus have terms, such as 'multidomestic', 'multinational', and 'polycentric', that mean roughly the same thing. This proliferation is exacerbated by the applied mission of the strategy field and its proximity to the textbook and consulting industries. Yet in all fields, there is value for career advancement in generating and proliferating terms, a problem hardly unique to the managerial sciences.

Given these incentives, what then constitutes a good innovation in academic research? There are many answers to this question, but I prefer, as someone who likes historical sociology, a simple one. Good innovative research is what a community decides is interesting. However, this relativistic orientation is disciplined by an important caveat in the following form. The best measurement of this interest is its sustained allocation of subsequent scientific effort to produce, confirm, and extend the cumulative results associated with the innovation.

If we accept the definition of a 'good' research innovation as a contribution that can be verified by a community (much in the spirit of Robert Merton's emphasis on the importance of the cumulation of knowledge), then it follows that tools of research design can have powerful effects on the direction of academic activity. We often study the direction of research activity among industrial firms and ask why certain technologies have evolved, and others not. By now, we know the answer is not simply market demand. Some technologies provide richer avenues for development; they are 'generatively' more fecund and act as broad technological platforms into new uses.[1]

Not all research methods and designs are equal in this regard. Take for example the research on transaction costs. Ronald Coase established in 1937 the observation that the boundaries of the firm are determined by a comparison of the costs of using the external market relative to the costs of internal

[1] See the seminal NBER volume on the direction of inventive activity (Universities-National Bureau Committee for Economic Research 1962), from which the title of this chapter derives.

organization. This work laid dormant until the 1970s when Oliver Williamson (1975) forced a reconsideration of these ideas. Surely, there was a market for this reconsideration, for Williamson wanted to explain initially why anti-trust authorities should not break up large firms. His earlier efforts in this regard relied upon a brilliant, yet conventional, welfare analysis that compared the monopoly loss of a process innovation relative to the gains in efficiency. He subsequently developed a more radical approach, first expounded at length in his 1975 book *Markets and Hierarchies*.

Yet, this book was not clearly different than other approaches developed at the same time that stressed the importance of market imperfections, especially for knowledge and information. In the international field, Peter Buckley and Mark Casson (1976), two young scholars, offered a broader explanation for the existence of the firm (in the form of the multinational corporation) by analyzing the different sources of market imperfections and the incentives for 'internalization' of market activities (see also Dunning 1977). Their approach was also more general, for unlike Williamson, they did not have the goal of showing that the multinational corporation was 'globally' the most efficient way to organize production. They sought to provide a positive theory for why it exists. In addition to Buckley and Casson, there were other efforts, including that of McManus (1972) who contributed the important notion of a continuum of governance solutions and 'relational' contracting. Not long after, Jean-François Hennart (1982) offered a theory of the multinational corporation that relied heavily upon market imperfections for the sale of information *and* incentive conflicts among principal and agents. Rugman (1981) similarly suggested the efficiency explanations for the multinational corporation.

All of these contributions invoked subsequent lines of research. (In fact, I fall prey to a 'selection' bias in failing to investigate those many ideas of the mid-1970s that essentially died without further development—or, remembering the dormancy of Coase's early work, at least have yet to be developed.) Why has the term of 'transaction costs' come, however, to be the dominant construct in the discussions on the boundary of the firm? Why do we not have studies clearly focused on 'internalization' as opposed to transaction costs, especially outside the field of international business?

Of the many answers to this question, I would like to highlight one: Williamson (1979) provided a schema by which empirical research could proceed to test his ideas. This article proposed that transaction costs have three dimensions: asset specificity, frequency, and uncertainty. Up until then, transaction costs appeared as an unobservable construct that required data on production and market costs under two different governance regimes (i.e. within the firm and from the market). How could this be done? These dimensions, especially the first of asset specificity, provided the manifest constructs for subsequent researchers to investigate the relationship between

an observable decision, e.g. to make rather than source a component, and the latent factor, called transactions costs. With this methodological and research design contribution, research in transaction costs established its industry. However, it is important to underline that the collection of such data, when using Williamson's prescription for 'microanalytic' detail, can be very difficult and demanding.

Research in international business has contributed its own methodological designs that serve as templates for subsequent efforts. I would like to document briefly three contributions: Raymond Vernon's multinational data base, foreign direct investment studies, and the choice of foreign entry mode. I turn then to two current areas of research (i.e. organizational ecology and comparative national systems) that might benefit from agreement on design and method. In focusing on these contributions, I neglect other major contributions to international business research, especially that of business history (as seen in Chandler 1990; Wilkins 1970, 1974, 1988) that has indisputably created successful research programs with defined methodologies; this topic is well treated in Wilkins' chapter in this volume.

## 27.1 THE HARVARD MULTINATIONAL ENTERPRISE DATABASE

Raymond Vernon, following a successful career in government service and a series of influential studies on the economic geography of American cities and their loss of industry, became intrigued by the question: why does the United States dominate foreign direct investment flows. It is often forgotten that Vernon posed essentially a historical question regarding *country* patterns in direct investment. Of course, he knew that other firms from other countries invested overseas. But what explained the dominance of the United States in the current flows? By posing the product life cycle theory of international investment that emphasized the *home bias of innovation*, he could subsequently provide a research template that sought to correlate the life cycle of innovations to the pattern of investment in that industry (Vernon 1966).[2]

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[2] This theme was picked up and amplified by Porter (1990).

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oligopolistic gaming and defensive investments on the basis of this research data. Stopford and Wells (1972) published the most important book on strategy and structure in the multinational business field using these data. Subsequent students, particularly William Davidson (1976, 1980), further developed the database and analyzed the entry patterns of multinational corporations. Karen Hladik (1985) and Ben Gomes-Casseres (1985) wrote their theses on alliances using these data. Gatignon and Anderson (1988), discussed below, also used these data. In 1993, Delacroix published a brilliant chapter on the demography of entry by American firms into Europe.

The above list is hardly exhaustive, but serves the purpose to show how a research design and project can diffuse far from its original intention. Did the design itself diffuse? Did other authors not use the data for testing other theories? Of course, but they did so within the constraints of the sampling design established by Raymond Vernon in collecting the data. He established therefore not only the raw data, but also a research program encoded in the decisions how and what to sample. The impressive and stunning contribution of Alfred Chandler to international business research, especially his 1990 book that compared the large firms of the US, the UK, and Germany, (*Scale and Scope: The Dynamics of Industrial Capitalism*), essentially established the same research framework of deriving statements about efficiency and advantage from comparisons among the largest firms.

In fact, when we sample on the side of host countries, we find that foreign direct investment has historically consisted of investments by many small and medium size firms. It is easy to forget this, and to conclude that size itself drove the success of multinational firms. The contrary hypothesis is that, in many cases, firms grew big because they had the knowledge of doing things better. As they grew, they invested overseas. In looking at foreign direct investment by American firms historically, I argued, with suggestive statistical analysis, that the growth and expansion of the US multinational rode upon the diffusion of better practices by US companies (Kogut 1992). The relationship of size and competitive advantage is partly a spurious result due to the omission of the underlying factors that drove the growth of the firm in the first place. More recent work in organizational demography has redressed this omission of analyzing growth, as we will see below.

One of the lessons of the Harvard Multinational Database project is that successful research templates bias subsequent studies away from other avenues of inquiry.

# 27.2 FOREIGN DIRECT INVESTMENT STUDIES

As discussed in many chapters in this volume, Hymer transformed the theory of direct investment by logically showing that competitive advantage at the firm level, not macroeconomic rates of return, explains the motives for outward investment by firms. Since then, direct investment and the multinational corporation have been indelibly linked. He proposed that since foreign firms were always at a disadvantage, they had to bring competitive advantages, e.g. scale economies, to overseas markets (Hymer 1960). However, he only tested this idea by correlating outward investment flows and concentration levels at the industry level (with the common belief of the time that industry concentration proxies for firm advantages). His thesis adviser Charles Kindleberger (1969) fought for the recognition of Hymer's contribution, which was not published until after the latter's death in a motor vehicle accident, by using Hymer's insight as the basis of a series of influential lectures on the current history of the multinational corporation.

In 1971, Richard Caves advanced this line of work by emphasizing the importance of the multinational firm for the transfer of 'intangible assets' across borders. This issue of 'transferability' of advantages, to use the current even if clumsy parlance, lies at the heart of the multinational corporation, for the costs of the transfer of the advantage must be less than the revenues derived from the investment. These ideas were far ahead of the debate at that time and were largely neglected. Instead, the ideas of entry barriers as derived from intangible assets appeared to be a succinct formulation of the Hymer line of analysis.

This article itself may have been remembered and footnoted as an early and intelligent treatment of the industrial economics of direct investment. Caves (1974), however, solidified his contribution by subsequently proposing a way to test the economic motivations of direct investment. His design consisted simply of regressing industry data on direct investment on measures of firm advantage, such as scale economies, R&D, advertising. These measures of firm advantage relied, in other words, on industry-level proxies that could be attained, with some difficulty, from government and public sources for some countries, in particular for Canada and the United States.

Having provided a statistical specification and a sampling methodology that relied on public data, Caves contributed a template that has been a standard design in direct investment research up to now. Of course, there have been some important modifications to this approach as well. One modification concerns the measurement of the dependent variable. Many studies, especially those that look at macroeconomic flows, rely on balance of

s published by the International Monetary Fund and by the
snational Corporations. The balance of payments method
le investments, for example, that are made by borrowing
country to be invested there. Some studies have used, as a
of plant and equipment when available (the US government
lata for a fee); others have used sales data, sometimes net of
studies have used entry counts, which have been shown to be
d with plant and equipment purchase data.

s of modifications regards the right hand variables. Because
provide R&D or advertising data, some studies have used US
looking at investments by non-American firms. For example,
publishes very skimpy R&D data, as collected by the OECD,
ot give figures for electronics without revealing private infor-
. Philips, a Dutch electronics firm. Since R&D data, as well as
lata such as scale economies and advertising, are correlated
s, the use of American data as a proxy appears as reasonable.
riously, the use of American data is not reasonable when we are
n differences in patterns across countries. Perhaps oligopolistic
not the same across all countries. Can Japanese technological
ully lead to investments in American oligopolies? A possibility
rican industry is not an oligopolistic in the same sector and the
lack competitive advantage. (Of course, we also might be
American sector of technological rivalry with the large firm
racteristic of Japan.) If we use only American data for describ-
nd Japan, we obviously cannot evaluate this possibility.[4]
a more subtle point. The Hymer and Caves studies also shared
ne bias assumption, namely, that firms go overseas on the basis
advantage. The home advantage pushes out investment. Yet,
if foreign countries pull investment because of location advan-
nally, we thought of location advantages as differences in factor
ect investment is primarily among rich countries, we know that
be the explanatory cause for location decisions. Clearly then,
access and technological sourcing (inclusive of productivity
re factors that can pull investment.

ell (1989) was one of the first to explore the influence of
pull on investment. In doing so, he relied upon a new source
form of patent registrations. Unlike direct investment data,
tions exist over a long period of time. Increasingly, they are

point was not, however, resolved in the empirical literature until Hideki
ned his study on Japanese exporting behavior using different observations on
ited States (1988). Sea-Jin Chang and I (Kogut and Chang 1991) followed this
yze Japanese direct investment in the United States.

accessible remotely through electronic databases, a process that can be easily automated. The ease of these data have already spawned an industry of studies. He also endorsed a particular measure of revealed technological advantage that indicates a country's relative propensity to innovate in a particular sector normalized for its overall innovative propensity in all sectors.

However, there is, as always, a serious bias in using data for proxy purposes. For example, investment into a country is pulled not only by technology in the strict definition, but by superior productivity in carrying out particular activities. The sources of this superior productivity might be organizational or institutional. An organizational source is the local diffusion of a particular practice within the borders of a nation, such as the diffusion of mass production in automobile manufacturing in the early part of the last century.[5] An institutional source is, for example, the corporate governance practices supported by the legal and financial institutions of a country. In either case, organizational and institutional sources do not easily diffuse across borders of countries. Consequently, a country might have a sustained location advantage in attracting investment and such advantages need not be reflected in patent data.

Again, we have the case that the success of a research strategy can steer a field in particular paths. One possible response from researchers in the area of patents is that technological output is complementary to innovative organizational practices. Thus, patents are a proxy of publicly registered technological outputs plus the tacit knowledge of how to innovate and, less obviously, of how to organize in general. We don't know how good, in general, an assumption this is, but it is possible to think of cases where it is a very bad one. Perhaps the clearest example is an industry where patents play a minor role, and yet direct investment flows show a country pattern. Advertising is a good case, where the US and the United Kingdom have long dominated and have attracted a great deal of foreign investment. There are other prominent industries as well, from cinema to industrial design to consulting. Patents are a bad measure of some important kinds of knowledge. However, given the template and the importance of studies on technology, location, and diffusion, studies using patents are riding currently a powerful trajectory that will, and should, expand further.

Are there proxies for knowledge in general? Lieberman and Demeester (1999) suggest that inventory levels proxy for the Japanese practice of just-in-time inventories and on this basis, they trace the diffusion of these

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[5] For a full explanation, we have to explain why such organizational practices stay within the borders of a country despite the ability of firms within a country to imitate each other. An early attempt to address this question, and to propose a country explanation for firm advantage, is given in Kogut (1991).

practices within Japan. This study has the benefit of looking at an economic outcome, inventories, to measure presumed adoption. In this sense, it is a more direct measure of an economic effect that patents that are presumed, with several studies that validate this claim, to measure technological inputs. Some studies look at adoption of practices, such as divisional structures or quality control, without knowing the 'fidelity' of adoption or the economic consequence.

The alternative is the expensive but rewarding research design to collect data at the level of the practice itself. The MIT motor vehicle program, especially the research of John Paul MacDuffie (see MacDuffie 1993 for example), has collected data at the plant level across countries for organizational practices. However, because these data are limited to a single industry and are at the plant level, they cannot serve as a template for quantitative studies on relating the effect of organizational innovations on pulling or pushing direct investment. Such studies do serve as useful templates for research in national comparisons, a point to which we return later.

## 27.3 FOREIGN ENTRY MODE

The choice of the mode of entry by a multinational corporation has interested researchers in international business from the start of the field. Most of the early studies were based on case studies that provided rich insights into the choice. However, with the creation of the Harvard Multinational Enterprise Database, it became possible to explore more systematically the factors that influenced the choice of entry. Stopford and Wells (1972) looked at correlations to make predictions about the positive relationship between strategies and the degree of equity ownership, arguing that technology encouraged higher control relative to advertising. Fagre and Wells (1982) later asked: how did bargaining power between host government and foreign firm influence the degree of equity held in a local subsidiary? Wilson (1980) used the Harvard Multinational Database to look at the choice to acquire, using correlational analysis.

This concern with equity control gradually became relatively less interesting than the question of entry mode choice, that is, by wholly-owned, acquisition, joint venture, licensing, or exporting. It has been a common confusion in this literature to confuse mode with the choice between a greenfield versus brownfield investment, that is a new investment versus

buying into an existing facility. Clearly, wholly-owned entry can also be conducted by an acquisition. However, the above framing has become the currency of research in this area.

An important breakthrough in the studies on entry mode choice occurred when Richard Caves and his student Mehra (1986) published their chapter in a book on international competition that included, among other things, a logit analysis of the entry choice. This contribution was inevitable given that the entry choice was discrete, but it was nevertheless Caves who gave the methodological template once again. This template consisted of collecting data on entry mode as the dependent variable and then regressing it on reasonable predictors. Again, an industry was launched, with no clear signs that its energies have yet been sapped.

Given the methodology, the scramble was now to refine the method and to name its theoretical implications. Gatignon and Anderson (1988), in this regard, made both of these fundamental contributions. Unlike an ordinary least squares regression, a qualitative-choice model does not provide a measure of explained variance. It does provide a goodness of fit test in the form of a Chi-square estimated from the likelihood function. This test has low power, which has usually not troubled researchers in management (or economics) who rarely conduct power tests. Gatignon and Anderson were troubled, however, that their data—to which we come in a second—consisted primarily of wholly-owned entries. They thus partitioned the data into several sets, ran their estimates, and then tested for the robustness of the results. This paper has become a standard fare in doctoral courses as a result of this rigor alone. In addition, the paper introduced multinomial choice models to the literature at a time when the software was not available in a package.[6]

The other contribution of the paper was to establish a template for future research emphasizing transaction cost explanations for entry choice. Stopford and Wells had argued that strategic motivations explained entry choice, such as the desire to control technology. They supported their point by correlating R&D expenditures with entry choice. Anderson and Gatignon (1986) argued that these motives are best seen as relating to transaction costs concerns. They reinterpreted, for example, the measures of R&D expenditures as a proxy for the hazard of transacting information in the market place. (Buckley and Casson 1976 made the same argument and empirical claims for the internalization of investment flows in their seminal study.) Effectively, they

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[6] Singh and I (Kogut and Singh 1988a), in fact, used their program for our paper published in the same year. Our paper introduced a measure of 'cultural distance' that by itself served as a vehicle for replication in other studies, another example of a methodological innovation allowing a field to develop, with some negative consequences in biasing attention away from other kinds of cultural studies. The success of this measure is due to riding the coat-tails of a more sweeping research program of Geert Hofstede (1990) on culture, which I do not review in this chapter though deserving of attention.

highjacked the direct investment and Harvard Multinational Enterprise templates for the purposes of ushering in the plethora of transaction costs studies that were to follow.

Given that we knew that the correlations between their proxy variables and control were already explored by Stopford and Wells for the same data, the results of their qualitative-choice models were not surprising, but surely were reassuring of the robustness of the earlier analysis. They noted, however, that it was difficult to predict the degree of equity control as opposed to the choice of entry. This finding suggested that firm considers the equity control decision subsequent to entry choice, with equity control influenced by bargaining and by other means to exercise control. Over all, the assumption of a monotonic relationship between control and equity share has been a difficult one to maintain in the empirical work on entry mode choice.

This finding of a two stage process was basically ignored until a paper by Myles Shaver (1998) exploited this insight to suggest a radical rethinking of the entry and strategy relationship. Shaver was perturbed by the finding of Hennart and Park (1993) that entry by acquisition lead to more stable entries by foreign firms. This claim troubled him, as it should. For if doing X is better, then we should see all firms doing X in the absence of barriers to imitation. Whereas we can construct scenarios whereby a firm is unable to imitate an acquisition, this construction is a far more sophisticated interpretation than justified by the original estimation.

Shaver proposes to separate out the entry decision of foreign firms into the United States along the lines of Gatignon and Anderson. First, we should understand the strategy choice to enter a country and second, we should test for the performance impact of this choice. After all, it is possible that acquisitions are encouraged in more profitable and fast growing industries, and it is this unobservable, called industry conditions, that also then influences the survival and performance of the entry.[7] There is, in other words, a potential Hausmann selection bias (Hausmann 1978). How do we then separate out the unobservable influence of industry conditions?

Shaver notes that we can, in fact, say something about this unobservable because we know already that firms have chosen to enter the United States by a particular entry choice. So step one is to estimate the coefficients to the predictors for choosing an entry mode, acquisitions or wholly-owned. These coefficients can then be used, in combination with the data for each firm, to generate the estimate for the likelihood that a firm will choose acquisition over wholly-owned entry.

Step two consists of taking this value and treating it as an independent variable in the equation that specifies the survival chances of a given entry. He shows acquisition, without this selection correction, is positively related to

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[7] Kogut and Singh (1988b), in fact, find this relationship to hold for their sample.

enhancing survival, but that this effect goes away when the selection bias is controlled. To drive home his point even further, he then conducts a counterfactual exercise, showing how mortality should rise if the entering firms chose the 'wrong' entry mode.

There are other examples in management studies where it is claimed that doing X has a monotonic effect on performance. Such claims, based on single equation and linear estimates, are vulnerable usually to the line of inquiry posed by Shaver; hence his contribution has opened a wide terrain of research in the strategy and management literatures in general. It is not surprising that other researchers are already jumping onto this moving train.

It should be noted that the Hausman selection bias is equivalent to the bias of incomplete sampling discussed above in the context of the Harvard Multinational Enterprise Database. Without correcting for selection, the bias is incurred by specifying the model as 'unconditioned'. In fact, the estimates are conditioned on the underlying sampling methodology. In the case of the Harvard data, small firms were left out and the multinationals that died by the time of the 1966 cross-section were left out. In the case of the Shaver study, the performance model was conditioned already on the choice of firms to choose an entry mode. In both cases, the error was failing to see that the estimations were conditioned on the sampling methodology.

# 27.4 ORGANIZATIONAL DEMOGRAPHY AND OPTION VALUATION

The article by Shaver belongs also to another research tradition called organizational demography. Vernon had, as we noted, started this line of work in international studies, but his choice of sampling on survivors entered a bias into the data. This bias is called a left-hand censoring bias and it arises whenever we reconstruct histories based on survivors. We have obviously lost data on multinational corporations that are no long surviving or no longer qualify as multinationals under Vernon's definition.

The adjacent field of organizational ecology and demography, particularly associated with the early studies of Hannon and Freeman (1977) and Carroll and Delacroix (1982), has heavily emphasized the importance of this kind of

bias. They have made an important and remarkable, contribution in establishing a clear template for demographic research regarding the importance of sampling on founders rather than survivors and of the effects of age on mortality. For example, many of the early studies on joint venture survival ignored that ventures may die faster when young, or that older ventures are already proven 'survivors'. It is simply unacceptable to compare organizations of different ages and make inferences on survival without correcting for these effects.

Of course, organizational ecology is more than the methods of demography. Ecology has also proposed a number of theoretical ideas, some no longer in repute, some that are still in development in the international area. One idea, though of less concern today, was the liability of newness, derived from Stinchcombe (1965), that suggested that young firms' die more rapidly than older ones because they have yet to establish legitimacy and to learn the ropes. Another theoretical claim is that firms' entry into markets rises and then declines with the density in that industry, and similarly mortality has also a quadratic relationship with density. Density is the number of firms in the industry.

The diffusion of these methods into the direct investment performance literature happened with a decade or more delay. In many ways, these methods and ideas resonated well with international business that had been preoccupied with entry and death from the time of Vernon and Burenstam Linder. The latter, in his remarkable study that launched the field of inter-industry trade (Burenstam Linder 1961), noted that international export is the extension of the home network overseas. In other words, international expansion is part of the theory of the growth of the firm.

These ideas were not actively explored, outside of the fascinating studies in business and economic history, by international business researchers until the 1990s. Many of these studies adopted population ecology theories along with the method, thus leading to a rich, sometimes overly complex theoretical discussion for the reasons for the growth and death of subsidiaries. The early studies were by Jiao-ti Li (1995) who looked, using the new methods, at the survival of foreign subsidiaries. They thus sampled on firms that already entered. This approach, with increasing sophistication, informed also the studies of Shaver and his two thesis advisers, Will Mitchell and Bernie Yeung, in their work (Mitchell *et al.* 1993; Shaver *et al.* 1997). The article by Shaver (1998) on selection bias, discussed above, was the outcome of this line of research.

Srilata Zaheer's studies on trading rooms represent a sophisticated treatment of this line of work. In her article on currency trading rooms, she looks at whether multinational financial companies that have foreign subsidiaries working as currency traders suffer from the Hymer disadvantage, which she labels the 'liability of foreignness' (Zaheer 1995). She finds evidence for the

effect of a liability of foreignness, even for an industry where information diffuses rapidly and all traders have, in theory, equal access. Of course, we once again have Shaver's possibility, namely, firms that have decided to operate overseas already are a selected bunch.

The Zaheer study poses, however, the larger question if the importation of ecological ideas, as apart from the method, have been corroborated. In part, the problem is that ecology has itself not fully addressed issues in its own literature regarding the liability of newness and the reliance on reduced-equation models of entry and death. Both of these issues raise larger questions regarding whether demographic forces are lawful, independent of context, a larger issue we pick up in the next section.

The liability of newness hypothesis has not been satisfactorily tested in the literature. The early efforts imposed a parametric specification of the hazard model and then asked seemingly simple questions, such as is the slope negative. A parametric specification gives the hazard rate as a specific functional form; early choices were the Gomperz or Weibull. If the slope was negative, then this implies that young firms die at a higher rate than older firms. Of course, there are many problems with this estimate of the slope. First of all, most of the specifications were log-linear, whereas the data almost showed a rather non-linear trend, even when logged. Even when the slopes were estimated to be negative, the plots of the data suggested a far more complicated relationship. In particular, deaths usually picked up after an initial period. Efforts to fix this problem resulted in partitioning the sample into early and later periods or discussing complicated relationships between intercept and slope estimates.

But there is a more devastating objection to this hypothesis other than the resistance of the data to conform. All demographic samples of organizations consist of heterogeneous firms. This heterogeneity consists of observables such as size or product diversity. It also consists of unobservables, such as the inherent quality of the firm. Now here is the problem, and the beauty of a liability of newness prediction. It is mathematically true that any heterogeneous population will evidence a liability of newness. (See Heckman and Singer 1985 for a proof.)

It is simple to show why this would be true. Consider two populations, one consisting of dogs, the other of humans. Both have exponential hazard rates, that is, their hazard rates are constant regardless of age. (Now this is extreme, but it serves our purpose.) In Figure 27.1, we graph their constant hazard rates and assume that the hazard rate for dogs is higher. Now what would happen if we mix these two populations? It takes a moment of reflection to realize that we would begin at a point in between the two populations and then that the decline would asymptotically decline to the constant rate for the humans. Since the dogs are dying more rapidly, their contribution to the mixed population declines relatively faster; at some points, there are only
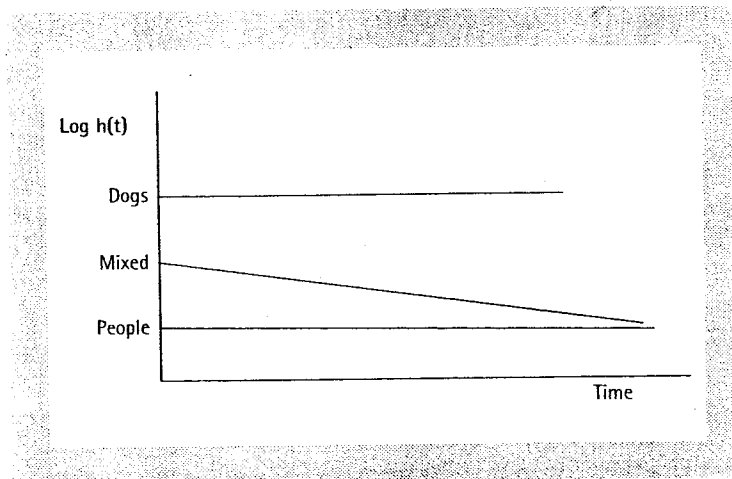
Fig. 27.1  Constant hazard rates and yet a liability of newness

people left. Heterogeneity always generates a liability of newness, even when the true populations have constant hazard rates.

Of course, there are some reasonable sources of heterogeneity. Size is an obvious one; we don't expect large firms to die as fast as small ones. We can try to control for these influences in a parametric model, but it is unlikely that heterogeneity can be controlled. (Efforts to specify mixed distributions, such as considering the happy conjugate of the gamma as a description of the unobserved error, have not lead to satisfactory resolutions.) The alternative is to avoid the claim of a liability of newness, specify exactly what are the causal sources that should cause young firms to fail, and to rely upon semi-parametric or non-parametric specifications to test their relationships. Implicitly, the literature has taken this direction without formally disclaiming the earlier claims to have found a liability of newness.

The weakness in trying to infer a law from a reduced equation raises the more important issue of whether it might be more interesting to think about the underlying mechanisms instead. The international literature began, as we noted, with an emphasis that international expansion is a consequence of the growth of a successful firm. This emphasis leads to a research design that samples the firms in a country and then analyzes their expansion overseas and the consequences of this expansion on firm mortality. Mitchell *et al.* (forthcoming) follow this strategy, arguing that international expansion actually increases the mortality (hazard) rate. This surprising result is consistent with earlier findings that suggest firms who are not succeeding at home often expand overseas first; successful ones would rather expand at home. (See Mascarenhas 1992; Yu and Ito 1988 for some evidence.) In other words, it

raises Shaver's selection bias issue in trying to sort out the direct effect of expansion from the unobserved factors that might promote overseas expansion in the first place.[8]

These are complex issues, which are not helped by the plethora of organizational and economic theories for why firms grow in general and why they grow across national boundaries in particular. As in all cases, it is sometimes useful to remember a baseline case. This case consists of a firm that grows randomly but proportionately. In other words, growth is log-normal and is governed by the following process:

$$dS/S = a(t) + \sigma dZ$$

where the proportionate growth of the firm is equal to an expected mean growth (or drift) plus random variation drawn from a standard normal distribution and scaled by the variance of the firm.[9] Let's assume that a firm expands overseas once it has reached a certain size. (This size could be the volume of exports.) The hazard of expansion overseas is the probability that the size of the firm will hit a lower boundary given its size at a certain point in time. A hazard rate is, in other words, derived from a more fundamental structural model that expresses a theory of the growth of the firm.

This observation implies that every growth process can be understood as facing certain hazards. Sometimes, this hazard is a negative outcome, such as death; other times, it is positive, as when large size gives the possibility to expand overseas. We might then ask the question if changes in size allows a firm to go overseas, what is the value of this probability at a certain point of time? In other words, what is the value to the option that a firm expand in the future?

There is, in other words, a mathematic relationship between hazard rates and option values that also suggests a theoretical point. Firms have the potential to enter a country, or to withdraw from a country. We can under-

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[8] For those interested in understanding the technical details in many of these models, it is useful to see the repetition in mathematic specification. The primary observation is thank the heavens for the exponential function. The second is that the treatment of selection, censoring, and truncation bias leads to the same methodology of calculating the probability density functions conditional on the remainder of the cumulative distribution function. A lot of the magic goes away when we see how the same tricks solve so many different problems.

[9] The specification of the hazard rate implies, in consequence, particular forms for the underlying structural process. I have never found a text or discussion that describes, for example, the implied process for a Weibull or Gomperz model. However, for the random walk, the conditional probability of hitting a boundary is the Inverse Gaussian, a solution given first by Edwin Schroeder for the so-called first passage problem. However, it is important to note that the random walk is a reasonable model for firm growth only if the process is scaled, that is if the process is log-normal. Otherwise, we are assuming that a firm has the same dollar variance when it is worth a billion dollars as when it is worth 10 dollars. This would greatly overstate the chances that large firms will survive and greatly exaggerates the (correct) point that size matters to understanding hazard rates.

stand this potential as a probability, or hazard rate. We can also, if we are taking a more economic approach, evaluate the value of this potential. In other words, organizational demography leads directly to a consideration of option values, once we move from reduced forms (hazard rates) to considering the underlying stochastic process governing the value of the firm.[10]

Understanding the sources of the stochastic growth of the firm has proven to be a difficult and complex undertaking. Economics suggests that it might be more appealing to work with profit or cost functions, and let stochastic elements enter via exchange rates or other price movements. It is this approach explored by Dixit (1987) in his model on exports and hysteresis (see also Campa 1993) and by Kogut and Kulatilaka (1994) in their treatment of the multinational network as consisting of the options to respond flexibly.

While these models seem far removed from the concerns of organizational demographers, they are in fact mathematically related and substantively related in trying to understand the stochastic value of the firm to grow and coordinate its activities across borders. Thus, in empirical work on the relationship between exchange rates and foreign investment, Kogut and Chang (1996) relied principally upon the methods of organizational demography. They constructed a population of Japanese firms in the electronics industry listed on the Japanese stock market. They then looked at the influences of exchange rates, previous entries, and firm characteristics on the hazard of 'exercising the option' to enter the United States.[11] Thus, the template of organizational demography was brought to bear upon a problem of the economic analysis of foreign entry.

## 27.5 COMPARATIVE MANAGEMENT AND NATIONAL SYSTEMS

One of the most promising directions of organizational demography in recent years is its work on tracing the entry and exit of firms in a given industry

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[10] Tuma and Hannon (1984) analyze models derived from stochastic growth as Ito processes; Ito processes are a standard in the options literature. Yet, the technical similarity between the approaches has been ignored, because the two literatures do not know each other, but also because organizational demography has not theoretically been disposed to the idea of 'exercising options', that is, to the idea of choice.

[11] Rangan 1998 offers a more direct test of the multinational switching hypothesis.

across borders.[12] As noted earlier, the chapter by Delacroix (1993) is a fascinating application of organizational ecology to foreign entry, using the Vernon database, which also is sensitive to country context. A good deal of this work is focused on the question if founding and death rates are density dependent. For the reasons given above on the liability of newness, this question is likely to be very sensitive to controlling for other factors, such as the size of firms and their concentration of sales in the local markets. But these efforts represent the most extensive efforts to collect comparable data since the Harvard Multinational Enterprise Database.

One of the principal questions that derived by looking at the rates of entry and exit across countries is the effect of institutions on economic behavior. Work in this area varies widely in approach and in conceptual rigor. Some approaches consist of the claim that countries are different in their business systems and offer a list of factors by which to make comparisons. Or sometimes an approach simply claims that institutions matter and countries differ in their political ideologies and economy in how they achieve social consensus. More ambitious approaches ride upon notions of institutional equilibria. In these approaches, a country is characterized by a number of actors who are institutionally represented. They achieve a political balance that also permits effective coordination in the workplace.

A good example of this latter approach is the work of David Soskice on Germany. Soskice (1990) proposes that Germany, as an ideal type of 'corporatist' solutions to social cooperation, is characterized by the institutions of labor, enterprise hierarchies, banking, and business associations. Each of these institutions are represented by formal institutions or dominant actors. (In other articles, the Central Bank and governments also play a role.) These institutional actors bargain to create a 'high equilibrium' that supports the coordination of work to produce quality export-oriented products.

This is a vast field that is rapidly expanding. Are there templates of methodology and research in this area?

There are certainly a number of possible avenues for researching institutions. The crude yet effective way is to collect data across countries, and then use country dummies to check for differences. There are variations on this method, ranging from random effects to log-linear models. However, these approaches do not provide deep insights into how institutions matter; the power of these tests, one suspects, is not in any event strong.

Comparative studies often, as a consequence, consist of rich country studies that try to isolate an effect by quasi-experimental design. For example, Bendix (1956), in his study on the relationship of authority systems to national development, chose Russia, the UK, Germany, and the United States as four countries, each which occupied a single cell in his two-dimensional

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[12] See e.g. Delacroix and Carroll (1983) and Hannon et al. (1995).

causality as delineated by Simon (1957). This problem is easily treated by Ragin's comparative Boolean methodology. In particular, we will utilize the following Boolean rule. A cause A can be present (denoted as A) or absent (denoted as 'a'). In one case AB are two factors that are both present and are associated with a truth condition of high performance. In a second case, factor A is still present but factor B is not, and yet the outcome is still high performance. By Boolean algebra, given that the truth condition is the same, AB + Ab = A. In other words, only cause A is causally related to the outcome of high performance. Indeed, for this comparison, A is *sufficient* to cause high performance. In the absence of other causes, A also appears as a *necessary* factor.

Lets reconsider the Aoki formulation more carefully. Using now our binary symbols of 1 and 0 rather than upper and lower case letters, we can represent Aoki's claim, as we saw, as mapping the combination of {1 1 1} to the condition of high performance. What if we found a second country, say Korea, that had only two of these conditions but still had high performance. Comparing the two cases, we have:

Japan: 1 1 1
Korea: 1 0 1

We can now conclude that only the first and third causes are causal; condition two is eliminated by Boolean algebra.

The second problem falls under the label of functional equivalence, as first analyzed and studied by Merton (1949). To illustrate this problem, consider the case that we compare Korea to a third country, say France, and both are high performing. (If it seems unfair to compare rich France to moderately rich Korea, keep in mind that there are much poorer countries than Korea. Sample selection issues remain critical to this methodology too!) By assumption, these two countries have the following configurations:

Korea: 1 0 1
France: 0 1 0

We cannot reduce these expressions further. They represent functionally equivalent institutional configurations to achieve high performance.

Now this conclusion might be troubling to our penchant to want matters to be more precise, such as there is one configuration that dominates all others. But as we have learned from the literature on the varieties of capitalism (e.g. Berger and Dore 1996), there are many ways by which countries can achieve similar performance outcomes despite different institutional configurations.

The problems of spuriousness and functional equivalence are easily dispatched by the application of a Boolean methodology. However, our third and fourth problems are not fully resolvable. The third is the problem of insufficient variety in the empirical data. As noted earlier, Aoki's three

factorial design implies 8 distinct configurations. These are {0 0 0} {1 0 0} {1 1 0} {0 1 0} {0 0 1} {1 0 1} {0 1 1} {1 1 1}. What if history does not provide all these experiments? Or what if the research design did not generate a fully saturated model by the force of its sampling methodology?

It is easy to see that we can make an error. Consider the case in which we have sampled Japan and Korea as before in the first two rows and then subsequently consider the case of a third rich country called the US:

Japan:  1 1 1
Korea:  1 0 1
US:  0 1 0

Clearly, we can no longer decide to eliminate the second cause. In fact, this second cause appears as sufficient, but not necessary in order to have a rich outcome. We have now two configurations that are suggested by Boolean reduction. This analysis reduced the complexity of the three cases to two configurations.

Of course, we can in some cases generate 'what if' cases based on the empirical data. We know by de Morgan's law that these two statements are equivalent: both it rains and Johnny comes, and either it does not rain or Johnny does not come. We can use this rule on the empirically observed configurations to generate the counter factual cases.

The last problem is the perennial obstacle of omitted variables. It is always the case in empirical research (and in theorizing) that we have neglected variables that do not only matter but they also interact with the variables we have chosen. In econometrics, there are statistical treatments to eliminate unobserved heterogeneity (as discussed earlier), but these treatments are themselves guesses about the distribution of the unobserved error; they do not handle issues of complex interactions.

The effect of the problem of complexity is easily represented by Ragin's Boolean approach. Consider a comparison of Japan and Chad; Chad looks the same as Korea but is poor. We could conclude that the absence of the second factor is causally responsible for poverty by analyzing the following configuration.

Japan:  1 1 1
Chad:  1 0 1

But we already know that Korea has the same configuration as Chad and it is relatively rich. The contradiction indicates that there is a problem due to an omitted variable and an incomplete theory. If we add in a fourth condition, we might have:

Japan:  1 1 1 1
Korea:  1 0 1 1
Chad:  1 0 1 0

Now we see that the fourth variable is causally responsible. But we discovered this only because there was a logical contradiction, and because we expanded our theory to look at a variable in which Japan and Korea agree, but Chad differs. It almost looks as if we cooked the books. Theorizing country differences that do not permit testing is indeed an exercise in exotic and imaginary cuisines. Yet, theorizing is indisputably required in order to guide the choice of variables and to prevent the list-like presentation of country differences that is often to be found in the literature.

The national system literature presents countries as independent experiments, sometimes precariously balanced in an equilibrium in which all actors must continue to agree to perform. However, it is also clear that practices and institutions diffuse across borders. How can we understand the study of Eleanor Westney (1987) on the importation of organizational forms into Japan if we contend that organizational effectiveness is contingent upon rigid institutional configurations? Similarly, the work of John Paul MacDuffie (1993), discussed above, indicates that American factories can adopt Japanese practices (in configurations) and achieve high performance productivity in the US despite different institutions?

Diffusion presents thorny issues to national systems. The problem is that there has been a failure in theory at two levels. The first is to separate the effects of genesis from diffusion. It is perhaps true that certain institutional configurations gave rise to particular practices; this is historical causality. But once such practices are known, they may diffuse to other institutional conditions.

The second level is a failure to understand that actors are far more adaptive than implied by these comparisons and that these practices themselves undergo radical re-interpretations. Within the corporatist balance of Germany, practices at the firm or factory level may change, sometimes by diffusion, but in the context of a discursive search among actors to adapt these practices. It is surely more complex to adapt when practices challenge existing categorizations of work encoded in an existing division of labor, such as skill categories that are tied to prestige and to wages. Yet, even here, the political balance among corporatist actors at the macroeconomic level need not be tightly coupled with the changes in work practice adaptations at the microlevel of work and industrial organization.[15]

Work on diffusion is still pretty much trapped in fairly crude analyses that once characterized national comparisons. That is, studies look at how a practice diffuses and by what channels. There is a growing literature on the difficulty of adopting certain kinds of knowledge (e.g. Zander 1991) and a fledgling body on institutional and political resistance to the adoption of

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[15] See Kogut and Parkinson (1998) for this argument, and also Kogut (1999) for an analysis of the adaptation of US work practices in Weimar Germany.

these practices. These studies need to be joined, but they are theoretically quite distant. And closer to our thesis, there is no clear research template by which to drive this research farther along. It is in need of a methodological innovation.

As a first pass, let's consider the case of the adoption of a practice by a European firm that is institutionally neutral. In this case, the problem is largely cognitive, for the adopting firm needs now only to understand the right causal combination and to adopt the various elements. Easier said than done! The European firm is organized as a hierarchy, with banking investors, and internal recruitment of top managers. We can characterize this system then:

| Hierarchy | Internal Recruiting | Truth Values: Performance and Quality | |
|---|---|---|---|
| 1 | 0 | 1 | 0 |

Consider now an American firm that has a different organizational structure of

| | | | |
|---|---|---|---|
| 0 | 0 | 1 | 1 |

In other words, the American firm produces at the same level of efficiency as the French firm but at better quality. What can the French firm do? It has control, subject to its negotiations with its managers, over the degree of hierarchy and its internal recruitment. (Indeed, the 1980s revolution in American corporate life saw flatter hierarchies and more external recruiting (Useem 1996)). Given this, the French firm can make three changes.[16]

| Hierarchy | Internal Recruiting | Truth Values: Performance and Quality | |
|---|---|---|---|
| 1 | 0 | ? | ? |
| 0 | 1 | ? | ? |
| 0 | 0 | ? | ? |

It becomes transparent right away that there are twelve experiments to run, as each configuration can take on four different combinations of truth values. This is complicated, so we can make our lives easy by assuming that productivity does not decline. The first two cases represent hybridization by recombining American and French practices. They represent two feasible paths from the French system. It is possible, of course, that these hybrids are superior to the American configuration, in which case there might be reverse diffusion. The third case (in bold) is Americanization, with the wholesale adoption of the American configuration. If the two first cases both lead to high quality and high productivity, then they are functionally equivalent. If neither work, then the firm is constrained to choose the American configuration.

[16] With two elements to perturb, there are 4 possible configurations; the firm's current configuration occupies one possibility.

It should be clear that even in this easy case, the finding of the right solution is not easy. We have restricted the choice to two factors. Of course, there might be more elements. In addition, a firm might be unable to run all these experiments. As a consequence, it might observe other firms that have experimented, or it might hire consultants. Even then, borrowing might be too inclusive and practices might be borrowed that have little to do with performance. They are like the hitching-hiking genes in genetics, bits of practices that have no clear causal outcome.

It is possible that even in the third case, the new configuration will not do well because of interactions with French institutions. In this case, the initial configuration might have to include the influence of the type of external financial market. In this extending consideration, the initial configuration is the following:

| Hierarchy | Internal Recruiting | Bank Finance | *Truth Values: Performance and Quality* |
|---|---|---|---|
| 1 | 1 | 1 | $\underline{1}$   $\underline{0}$ |

Here, a value of 0 for bank finance means that the firm relies upon equity markets. If the external financial institution is a fixed constraint, there are three possibilities:

(1) how a firm finances does not matter to performance;
(2) finance matters in conjunction with some configurations but not all;
(3) how a firm finances is a *necessary* condition (with equity finance required in order to achieve quality).

The second possibility is the most interesting, for it suggests that French firms can adopt hybrid forms that suit the national conditions. For different countries, there will be different configurations of practices that generate both high productivity and high quality. An obvious point is that there is not a single best set of practices. But the more important point is what gets diffused, or should be diffused, from a source country (e.g. the US) varies from country to country. In Italy, given its small firm structure, the adoption of American corporate organization might well decrease productivity. At the same time, external recruiting of managers might help performance. The idea of *core practices* is, then, possibly wrong, for it presumes that there are *necessary* practices. As we have seen, diffusion of practices from one country to the next can be compatible with multiple configurations.

The last possibility poses the problem of institutional change. For if French firms wish to achieve both quality and productivity, then there will have to be changes in financial markets. Institutional change is different than adopting practices, for it concerns the social and political agreements among diverse actors. In this wider consideration, not only is cognition a point, or the

internal politics of the firm, but also the credible commitments made by various parties to institutions.

Institutional change poses, then, particular problems for diffusion of practices. Of course, the example of superior American quality might seem anachronistic—if it was ever valid. But if we switch the truth value to radical innovation and the financial system under consideration to be venture capital (or its absence), we have indeed posed precisely the contentions in the popular press that financial markets are critical for the new economy success of the United States. And in fact, France, Germany, and other countries have introduced new stock markets to provide incentives (through initial public offerings) for venture capitalists.

Yet, even the simple idea of introducing a stock market for small firms can pose complex institutional interactions. We have not, therefore, entirely treated the problem of institutional change. For the problem is rarely simply altering a single institution, but rather the consideration of a change on the ensembly of interacting institutions.

Again, an illustration might help. If we return to Soskice's description of Germany, the corporatist economy is a fragile balance between competing interests. German financial institutions interact with central bank powers and with national and sectoral unions who bargain for wages. The logic of adopting new practices might require changes in an institution. However, changing a given institution might itself cause national agreements to decay.

Thus, it is not simply an issue of whether a firm should equity finance, but how this affects the strategic behaviors of other economic actors. The Eichel Plan to forgive taxing German companies for restructuring their holding companies has the appearance of creating more American capital market pressure on firms. However, the external shareholder representatives of American financial institutions still must sit on supervisory boards consisting of 50 per cent worker and managerial delegates. Whether such piecewise institutional changes are possible have yet to be fully observed. Clearly, such proposals are rejoined with an active debate among the institutional actors.

Boolean algebra does not eliminate all inherent complexity in causal relationships. It does provide, however, a methodology by which to sample countries (i.e. saturate the design) and to characterize the factorial combinations as necessary, sufficient, or causally unrelated. This approach has always been implicit in country comparisons, but yet rarely explicit as a template for conducting comparative research.

## 27.6 CONCLUSIONS

The field of international business has engaged the efforts of hundreds, if not thousands, of researchers over the decades. Despite these extensive efforts, doctoral students are not given clear definitions of methodological templates that can permit them to address important theoretical and empirical issues in a timely fashion. *Market demand* alone is not sufficient to create timely and high quality research.

International business has sometimes failed to lead in research on important international questions, such as transition in formerly communist countries or globalization. Other disciplines have made important contributions in these areas, even disciplines lacking the relatively homogeneous agreement in paradigm that characterizes economics. For example, in the area of transition economics, David Stark (1992) and Victor Nee (1989) made early contributions to understanding the importance of institutions and labor markets; Stark relied upon ethnographic and network analysis and Nee upon demographic techniques. Annalee Saxenian (1996) and Saskia Sassen (1998) used ethnographic methods to analyze differences in the performance of regions or the impact of globalization on different social strata. More recent studies, such as those by Ghemawat and Khanna (1998) and Hansen (1999), are attentive to context—be it country or company—and yet are branching into exciting new areas of research that are part of the legacy of the international business discipline.

It is important to consider, then, the value of emphasizing the role of coherent research methodologies that can speed research yet assure a high quality. International business, like many of its allied fields in management, has often resolved this issue by turning toward other disciplines, e.g. economics. We have failed to realize that international business' own history reveals that it has been most successful when research efforts have been able to rely upon broad methodological templates.

There is, in fact, a very simple implication in this discussion. The training of Ph.D. students should not be organized around timely topics but around research programs with defined methodological definitions. These definitions should engage students in understanding and utilizing frontier methods in statistical, comparative, historical, and ethnographic research. As illustrations of important methodological techniques that yet define broad theoretical research programs, I have cited above the examples of selection bias and Boolean comparative analysis, among others.

There is, of course, a broader implication regarding the desirability of international business to foster greater efforts to provide the equivalent of Vernon's multinational enterprise database. To a large extent, organizational

demography has seized this opportunity for the comparative study across countries. These efforts should be embraced and endorsed in international business. After all, an origin of this approach is the Vernon research program that is surely the historical domain of the field of international business. We should look with caution at contributions that fail to utilize existing or create well-defined research methodologies that permit researchers, especially doctoral students, to acquire the broad skills to influence the future direction of academic research activity in international business.

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