Journal of International Business Studies (2007) 38, 357–360

www.jibs.net

COMMENTARY

History in perspective: comment on Jones and Khanna 'Bringing history (back) into international business'

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Abstract
Commenting on Jones and Khanna, we suggest that international business (IB) needs simultaneously maturing theory and on-going rigorous empirical work. We advocate careful data collection and develop solid theory based on the rich empirical information. The difficulty in the process is the deciphering of causality, and that is where historical studies play a significant role. Historical documents shed light on motivations and exogenous incidents. Historical studies and large-scale data analyses can complement each other in revealing causality. While path dependence constraints causal inferences using cross-sectional data, historical studies can identify differences and similarities in the paths of events across geography and time. This advantage is particularly attractive in understanding the relationship between institutions and business behaviour, which lies inside the proper domain of IB studies.
Journal of International Business Studies (2007) **38,** 357–360.
doi:10.1057/palgrave.jibs.8400258

Keywords: historical studies; methodology; long-term view and integrated approach

International business (IB) is a very new discipline, and many IB scholars have made useful contributions with first-pass studies that expose its broad issues. In pressing the importance of historical studies, Jones and Khanna (2006) echo others in arguing (rightly, we think) that the field is ready for a shift from breadth to depth. Deeper empirical investigation, including historical studies, requires careful design. In neighboring fields, this design derives from theories of various sorts that guide empirical work and are, in turn, either falsified or left standing. Economics, psychology, sociology, and other elder social sciences provide both good and bad examples of how to integrate theory, statistical correlations, and historical investigation.

Like all academics, social scientists have trouble with pendulums – perhaps because they have properties of both matter and waves. For example, economics has swung back and forth in its appreciation of history. Its founders, like Adam Smith, clearly saw history as important, but by the late nineteenth century Walras and his fellow Marginalists gained legitimacy by imitating the mathematics of classical physics. This exposed the unpleasant logical nudity of romantic Victorian historical economists, such as Marx, but left economics an austere abstraction. This seemed too

Received: 19 November 2006

pallid a reflection of the real world to Schumpeter, who cast economics back into a historical light, but lamented his inability to do so in the language of mathematics. Keynes had no such linguistic modesty, and Samuelson's pupils found post-Keynesian economics in need of a swift kick back towards precise thinking. The current generation of economists, schooled in Samuelson's wake, rejects anew the dry austerity that so disturbed Schumpeter, and so the pendulum swings on.

We suspect these undulations retard the development of economics, and hope IB might avoid them. We agree wholeheartedly with Jones and Khanna (2006) on the importance of historical investigation, but suggest that IB also needs simultaneously maturing theory and ongoing rigorous empirical work of all sorts to guide such investigation. By theory, we do not necessarily mean economists' adventures in calculus, which might too often follow the form of science without its substance. Physics is a science not because it uses complicated formulae, but because those formulae faithfully reflect centuries of careful observation. We hazard yet another science metaphor and suggest that, although physics shows waves as integral to any matter, the particular subject matter of IB might be better appreciated if we avoid swinging back and forth too much. We should get to work on the careful data collection, and develop solid theory as we go.

Fortunately, students of IB are better posed to do this than many of their intellectual elders. We are now gaining electronic access to historical records and better data from more countries spanning more years than were ever available before.[1] A broadly integrated research agenda is now a genuine possibility. Models, statistical analyses of large data sets, and historical investigations can now interact in real time, and each should pay attention to the constraints the others impose. Our goal should be a theory fully consistent with observation, with contemporary and historical.

Like anything worthwhile, all this is hard work. Architects of models would have to take the empirical literature seriously, rejecting assumptions inconsistent with correlations observed in the data. Empirical studies are sometimes contradictory, and even irreproducible. The journals would have to start sorting this out by forcing researchers to make their data freely available and by publishing replications of results as well as failures to replicate results. Even were this done, standard statistical analysis, with the notable exception of event studies, is admittedly deficient in ascertaining the direction of causality. This is because statisticians have yet to devise a validity test for instrumental variables. Fortunately, historical investigation comes to our aid precisely where we most need help, for history is largely about 'what causes what'. In this sense, we applaud advocates of historical studies, such as Jones and Khanna (2006). The patient examination of historical documents can shed light on what motivated key decision-makers and thus on the direction of causality. This too should constrain the fancies of modelers.

To see how such an interplay might work, consider Porter (1992) arguing that shareholders are myopic – fixated on the next quarter's dividends – and that this undermines long-term investment by listed firms in countries that assign strong corporate governance rights to shareholders. This is a *scientifically valid* theory because it is *falsifiable*. Hall (1993) finds that R&D spending, the longest of long-term investments, is correlated with elevated share prices. This seems inconsistent with shareholder myopia, but reverse causation provides an 'out'. Might firms with low share prices avoid long-term investment because they are especially vulnerable to shareholders' wrath? Certainly, Porter presents case studies consistent with this. But Chan *et al*. (1990) assemble a large sample of cases where US firms announce sudden hikes in R&D spending, and find that their share prices rise significantly on the news. Clearly, US shareholders rush to buy stock in firms that invest for the long term. This reflects well on Porter for proposing a scientifically valid (falsifiable) hypothesis, and shows how empirical work can prune theory.

Such feedback between theory and observation, formerly rare, is increasingly practicable. Thus recent work by La Porta *et al*. (1997, 1998) finds countries' legal system characteristics correlated with their financial systems' depth. Neither the direction of causality nor the precise legal system feature(s) in play is entirely clear, for a set of historical studies in Morck (2005) show that features such as shareholders' rights and the importance of business groups or oligarchic families change substantially through time in various individual countries. But any model of financial development that fails to connect with the legal system somehow ought now to be inadmissible. At this point, causality might run either way, or from a latent factor affecting both the legal and financial systems. Historical investigation can narrow this scope even further, for any causal

explanation must be consistent with both time series and cross-sectional observed variation. And human behavior is teleological, so key economic agents might even leave records about why they did what they did. A well-documented case suggests a direction of causality. Numerous consistent cases across a panel of data can let us induce a direction of causality with more certainty. A large enough sample of well-defined cases becomes an event study, and permits statistical tests, as in Chan *et al*. (1990).

Jones and Khanna usefully develop the example of business groups – structures commonplace in many countries, but rare in the United States – and argue that they are adaptations to permit economic growth despite weak legal systems. Jones' historical work on the United Kingdom, with its detailed descriptions of how and why businesses were organized, is precisely the sort of historical investigation we need. But much more remains undone. Why do business groups persist in some countries with seemingly well-developed legal systems, like Canada and Sweden? Theory tells us that stable second best optimum can prevent movement towards the first best. Business groups may well be adaptations to predatory governments, under developed capital markets, and a scarcity of talent, but do they also prolong such institutional deficiencies to prolong their competitive advantage? Does this then, as Morck *et al*. (2005) argue, impede further development? Elders' longevity is not always a blessing to youth.

These arguments highlight the importance of careful and comprehensive historical studies – they identify differences and similarities in the paths of events across geography and time. These observations settle as well as create intellectual puzzles.

IB is especially well positioned to take advantage of a more integrated approach to research. Data on multiple dimensions of institutional development over many years for many countries are now possible, if not already available, and digital archives make historical investigation more feasible than ever before. Students of IB can study how behavior varies with institutions more generally than is possible for scholars constrained by time and geography. Indeed, from this perspective, a continued preference for articles using US data and theories pertinent mainly to the US, as still characterizes many leading journals, is increasingly indefensible.

The importance of path dependence gives students of IB a further edge. Path dependence means that cross-sectional studies need not reproduce time series correlations. This non-ergodicity often contains information about causality. If different countries follow different paths to the comparable endpoints, contrasting their paths can distinguish critical institutional antecedents from institutional appendices. For example, no country has grown rich without universal education, but universal banks can perhaps substitute for active stock markets (Beck and Levine, 2004).

Short decades ago, such questions were essentially irresolvable in a fog of dirty data and untestable models. Proponents and opponents could argue endlessly in the journals and hurl rhetoric at each other in the popular press. No more! Growing archives of digitized historical records and ever better overall data mean we can actually expect to falsify more theories and, by the process of elimination, focus with increasing precision on a residue of scientifically defensible explanations.

A developing coherent theory of IB that is fully consistent with observation, both contemporary and historical, no longer seems out of the question. An ongoing interplay of theory and observation distinguishes a mature science, while the undulation of methodological singularity seems a hormonal excess of intellectual adolescence. Physics and biology have their pendulums too, but more mature disciplines swing less. Jones and Khanna tell us IB can, perhaps, achieve a maturity beyond its years if we are diligent in pursuing the convergence of theory and comprehensive empirics.

Note

[1]Project Gutenberg, at www.gutenberg.org, houses electronic copies of 170,000 books and documents whose copyrights have expired. The Harvard-Google Project promises eventually to digitize 15 million more such titles from Harvard University's libraries: see http://hul.harvard.edu/hgproject/index.html. Jean-Noël Jeanneney, Président de la Bibliothèque nationale de France, recently called for a European 'counter-attack' against the Harvard-Google project, and the French government is currently organizing EU funding: see *France to Develop Google 'Rival'* posted 17 March 2005 at www.bbc.com.

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Accepted by Arie Y Lewin, Editor-in-Chief. This paper has been with the author for two revisions.