



PERSPECTIVE

# Qualitative research for international business

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## Abstract

Qualitative research in international business has been rare, the main research streams of the field relying more on quantitative methods. This paper first outlines why qualitative research has been scant. It then presents areas, such as theory building, where qualitative research could make a substantial contribution. Third, it reviews approaches to high standards of qualitative research and criteria for evaluating qualitative research. Finally, some possible research areas where qualitative research might prove fruitful are suggested.

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International business (IB) is a rich, open and complex field of study, partly because the world is intrinsically rich and complex, but also because IB is free from any single core paradigm, does not pursue a single dominant central research question and does not abide by generally accepted simplifying assumptions that would drive the choice of research methods and tightly bound areas of research relevance. In other words, many disciplines can contribute to IB, and IB is in essence multidisciplinary, benefiting from the complementary insights provided by various theories.

Yet rather than embracing the potential richness of a wide-open under-defined field, most IB researchers have shied away from such richness and relied on the comfort provided by well accepted but partial theories borrowed from various other fields and disciplines. For instance, transaction cost economics have come to dominate research on foreign investments, modes of entry and organization of multinational companies. Scales of cultural similarities and differences and measures of “distance” – essentially at the national level – have come to provide the basis for a stream of cross-cultural research. Institutional theories have been relied upon to explore different patterns of economic development, of policies toward foreign investment and multinationals, and of differences in internationalization patterns between multinational corporations (MNCs) (e.g., Rangan, 2009). Institutional theory has also shed light on the internal functioning of MNCs (see Doz & Prahalad, 1991). In some other cases, data driven inductive empiricism has prevailed and led to the *ad hoc* selection of useful theories for the research questions at hand.

Such theory borrowing and dogged empiricism have allowed the field to establish itself, grow and make progress, enabled some IB

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researchers to publish in leading journals in other fields (e.g., economics, strategy), and fostered the growing impact of *JIBS*. It has also come at a cost, though (Cheng, 2007).

IB research that is explicitly multidisciplinary – borrowing and adapting theories from multiple fields – has remained scant, probably reflecting the grounding of researchers in a single underlying theory. So while the field as a whole is richly multidisciplinary, individual contributions seldom are. This results in several limitations. First, borrowed theories all have their own strong focusing and simplifying assumptions which make them shed only a partial light on IB phenomena. The light they shed is partial both in that it does not cover the field but illuminates narrower areas, and in that its hue provides a specific theoretical perspective which highlights certain variables and shades others. Second, relying on these theories has exonerated IB researchers from the onus of building their own, thus often making IB but a “special case” or a mere application of broader disciplinary research, be it economics, organization theory or psychology. This, in turn, has made the specific identity of the IB field difficult to define, ascertain, bound and assert in scholarly terms. Research work contributed to the furthering of discipline-based theories more than to a rich understanding of phenomena of interest to IB. Institutionally, this lack of identity made the status and existence of IB departments in business schools increasingly vulnerable.<sup>1</sup> As the world economy becomes global, the specificity of IB research may fade away.

In a munificent open field defined as a phenomenon of vast scope and constant evolution one would expect qualitative research to play a significant role. Yet qualitative studies in IB have been relatively scant. Perhaps the intrinsic difficulties inherent in high quality clinical studies of multinational companies have deterred researchers from engaging in qualitative case-based research. The challenge of evaluating qualitative research and the resulting possible reluctance of top journals toward publishing such research may have further limited the visibility and recognition of qualitative research. Qualitative research is often related to hypothesis development and theory building, and building theory is difficult, slow and frustrating. It is seen as a high risk ambition by PhD students. Research projects may take years. So, qualitative research may well require such stamina and willingness to take risks that drive away some researchers. These factors may explain its relative paucity.

This paper attempts to redress the balance toward qualitative research by pointing out its potential contributions, suggesting how to improve its quality and outlining potential areas of research in IB where qualitative research may be needed. It starts by summarizing potential contributions of qualitative research to IB. Qualitative research, of course, can be defined in various ways. Here I adopt a somewhat constraining definition: qualitative analysis (such as narratives and conceptual development) of qualitative data (such as semi-structured interview data, qualitative case studies, ethnographic studies, and so on).<sup>2</sup>

This paper then offers a brief discussion of criteria and activities for ensuring high quality qualitative research. I close by suggesting a few areas where qualitative research might be particularly useful and productive.

### **WHAT CAN QUALITATIVE RESEARCH CONTRIBUTE TO IB?**

Overcoming the limits of borrowed theories and quantitative empiricism is where qualitative research can play a determining role. As we all know, qualitative research makes a central contribution to theory building in management (Eisenhardt, 1989; Weick, 1989; Yin, 1989, 1994). Indeed, the IB field itself benefited from the early qualitative work of business historians (e.g., Wilkins, 1970, 1974). Qualitative research is uniquely suited to “opening the black box” of organizational processes, the “how”, “who” and “why” of individual and collective organized action as it unfolds over time in context (such as Aharoni [1966] did for US investments in Israel). Managers in action are its predilection. Event and time series analysis (e.g., Mintzberg & Waters, 1985; Poole, Van de Ven, Dooley, & Holmes, 2000) leading to quantitative analysis of qualitative data and other quantitative methods such as surveys (e.g., Brannen & Peterson, 2009) can complement, extend and very effectively triangulate qualitative methods.

Rich, “thick” process descriptions provide a guarantee against the temptation to rely on a single theoretical lens because they make obvious to the researcher that any single lens will shed only partial light on the phenomenon being researched. Inductive theorizing is more faithful to the richness of the phenomenon being researched than deductive theorizing could be. Confronting various theories can lead to new conceptualizations and allow a leap from a multidisciplinary approach to a truly interdisciplinary one. In the latter, contributions

from various theories are melded into new conceptualizations rather than merely juxtaposed. Only rich, thick descriptions can provide the basis for the use and possible synthesis of multiple theories into new conceptual development. While true theory-agnostic grounded theorizing (Glaser & Strauss, 1967) is probably impossible, given the cognitive frames researchers carry with them, the richer the qualitative research the greater the chances of being free from excessive predetermined reliance on a given theory and therefore the better the odds of genuine theory building.

As part of the research design, qualitative methods rigorously contribute to theory building in many ways. First, by providing rich, thick descriptions of real phenomena and action instances (or streams) they stimulate deeper thought (Weick, 2007). They provide a safeguard against the “seeing what you are already believing” risk of semi-structured empirical research and allow richer and stronger conceptualization. New theory is shaped progressively, in the mind of the researcher, over time, in an iterative “constant comparison” and recursive interplay between rich data and emerging conceptual insights that can be related to existing theories and also allow one to create new theoretical insights. They provide the substance of the disciplined imagination process so central to theory building (Weick, 1989).

IB is in need of theory development. Many scholars have noted that for IB to be more than a collage of theories it needs to build its own original theories and not rely exclusively on borrowings and juxtapositions (for example Buckley, 2002; Wilkins, 1997: 41). Theory development hence should be part of the scholarly agenda of the field.

In a slightly less ambitious way qualitative research can also be used for theory testing. Qualitative research allows one to bring a variety of theoretical lenses to bear on the phenomenon being investigated and to compare systematically the nature and extent of the insights provided by these various theories (Van de Ven, 2007; see also Allison, 1971, for an early example). So, theory testing is another potential contribution of qualitative methods. It is possible to compare the prediction a theory would make about a phenomenon to the observed instance and to extend or challenge the validity of the theory, or to establish its applicability boundaries.

Related to theory testing, another use of qualitative research may be to not just help build or test a theory, but also to help communicate it by showing

its applicability, to *illustrate* and emphasize the key elements and relationships in the theory. Although, in principle, a theory should stand on its conceptual coherence and hold in its logical structure, the use of qualitative illustrations (such as “vignette” cases) may make its communication easier, in particular to managerial audiences, and for more applied theories (Siggelkow, 2007). Many of the most telling published contributions of qualitative research (for instance Bartlett & Ghoshal, 1989; Brown & Eisenhardt, 1997; Burgelman, 1994; Weick, 1993) combine a richly textured description of a specific situation, or of several, and a robust and insightful theoretical contribution that springs from a conceptual interpretation of the qualitative data.

As pointed out by Cheng (2007) qualitative research may also be essential for surfacing contextual dimensions in IB, such as differences between countries. Features of context are hard to specify from the outside without having been experienced. Short of deep pre-existing contextual familiarity, qualitative research in a new context is a way to learn about that context up close, rather than risk assuming away contextual differences. Creating theories that recognize context, rather than abstract general theories that ignore its significance is important to IB (Buckley & Lessard, 2005), and qualitative case-based research can contribute to the contextualization of general theories.

In a more fortuitous way, qualitative research may also enable one to discover the importance of a hitherto neglected phenomenon or the relevance of a particular theoretical perspective to that phenomenon. It provides a strong inspiration for new ideas and research agendas. Of course other research methods can yield similar benefits, but the usually more theoretically open nature of qualitative research provides a more likely opportunity to discover new phenomena worthy of investigation. In a domain-defined field like IB where new phenomena of interest arise rapidly, qualitative exploratory research may help identify and understand new phenomena as they arise and assess the extent to which they are worthy of academic research. Emerging market multinationals provide a current example: they are certainly empirically important, but do we need new theoretical developments to understand them, or is what we know about multinationals sufficient to shed light on this phenomenon?

In sum, although there is no clear cut answer to the question of whether qualitative research is



worth the risk and the effort, it clearly contributes to the development of a field of management research in various ways and certainly could contribute more to IB than it has done recently.

### QUALITATIVE RESEARCH AND IB: A MISSED OPPORTUNITY?

Following its early reliance on corporate history monographs and business history, IB quickly moved away from its qualitative origins. As compared to the rich qualitative tradition in sociology or organization theory (e.g., Pettigrew, 1973), or even to an extent strategy (Bower 1970; Chandler, 1962; Christensen, 1997; Quinn, 1980), IB researchers put relatively little emphasis on qualitative research. Of course, the Harvard Multinational Enterprise Project that Raymond Vernon led was informed by the rich case-writing activities then customary at Harvard. But, with few exceptions (e.g., Aguilar, 1973), the wave of international case studies came later, largely at the initiative of Christopher Bartlett and Sumantra Ghoshal.<sup>3</sup> Both of them, however, saw themselves as strategy scholars sharing an interest in the multinational enterprise, and some of their most fascinating case-based research pays only scant attention to the multinational character of their research sites (e.g., Ghoshal & Bartlett, 1994). Their work also occasionally suffers from the dual use (research and teaching) purpose of how they report on their research.<sup>4</sup>

Nordic Europe provided another pocket of qualitative research in IB, in particular the detailed studies of the internationalization of Swedish companies (e.g., Johanson & Vahlne, 1977) and the work of the Institute of International Business at the Stockholm School of Economics (e.g., Kogut & Zander, 1993). So did the early doctoral work of others at Harvard (such as Prahalad, 1975; Doz, 1976; Bartlett, 1979).

But, by and large, IB developed without much benefit from qualitative research. Indeed, the two main paradigms it came to draw from – transaction cost economics and comparative culture studies – did not call for qualitative research in their models and assumptions. Comparative culture research could of course be enriched with qualitative ethnographic research (e.g., Dore, 1973), but such work did not become central to the IB field (perhaps because it was so demanding). Paradigms, assumptions and rules of analysis were all well-defined, and the scope of the research thus

clearly established, qualitative research was not felt as needed.

### QUALITY IN QUALITATIVE RESEARCH

Not only was qualitative research not central to IB, what little was done was of uneven quality. There are several reasons to this. First, successful publishable qualitative research in management requires three quite different sets of skills on the part of the researcher, the combination of which was probably not widespread among IB scholars:

- A multidisciplinary, eclectic, but deep and insightful understanding of collective action in context is needed. This often requires a combination of first-hand experience (Prahalad, Ghoshal, Bartlett, and several of the few other widely acknowledged qualitative researchers in IB had significant management experience before joining academia) and a deep multi-disciplinary training in organization theory, organizational behaviour, sociology, strategic management, etc. Only very few leading business schools in the US and a few universities in Europe have provided such opportunities, as criteria for admission in PhD programmes have increasingly favoured quantitative skills. As a result, PhD studies today are probably more fragmented and encourage their students to overspecialize narrowly in a premature fashion, too early in the programme.
- An in-depth training and some experience in high quality field research practices is necessary. Students in ethnography and anthropology are routinely trained and follow an apprenticeship in the discipline of good rigorous fieldwork. Few students in PhD programmes in business schools get such training and apprenticeship. In some schools (Harvard, Ivey) the tradition of high-quality case writing – where cases are used to present students with genuine management conundra to be addressed in context, not simply as sketchy potted vignettes illustrating the use of this or that theory – provides a partial substitute. PhD students write one or several high-quality cases as part of their apprenticeship. However many of them who have no ambition to perform qualitative research find little value in this effort. They do not take it as an opportunity to learn to do high quality qualitative research. Informally, many universities in Europe, and some in Asia, achieve some of the same benefits by having senior faculty associate students to their work – both research and consulting – in companies.

**Table 1** What is good qualitative research?

## What to research?

- Exercise tireless intellectual curiosity in reading (not just in the management field), in conversation and in discussion and seek “insights from the field” in exchanges with managers.
- Thrive on questions raised by executives, abstract their problems and issues till one can be intellectually playful with them. Research managers’ conundra.
- Fight empirical limitations; push the envelope of known (conventional wisdom) answers.
- Uncover paradoxes and contradictions, both practical and theoretical. Focus on theoretical contradictions and under-theorized areas.

## In which relation to “the field”?

- With full support, but also autonomy from the leadership of the research sites. Gain support by framing the research in terms relevant to the problems faced by executives.
- With patience and respect for executives, and no academic arrogance.
- With a healthy dose of scepticism, both “truth seeking” and theoretically agnostic.
- With explicit assumptions about human character (e.g., believing that given the chance most human beings will do good).

## Doing what research?

- Using “thick” descriptive case studies, based on interviews, ethnographic research (including historical ethnography) and history research (with archival evidence).
- Applying “constant comparison” methods for within case analysis and using cross-case analysis as appropriate.
- Engaging in a multi-theory dialogue with yourself and between theories (in an abductive fashion).
- Operating at the most revealing level of analysis, with units of analysis and abducted theories at the same level of analysis.
- Exercising “disciplined imagination” (Weick, 1989) in developing theory.
- Being careful to be agnostic and sensitive to the risk of theories holding us rather than us holding theories.

## Presenting what findings?

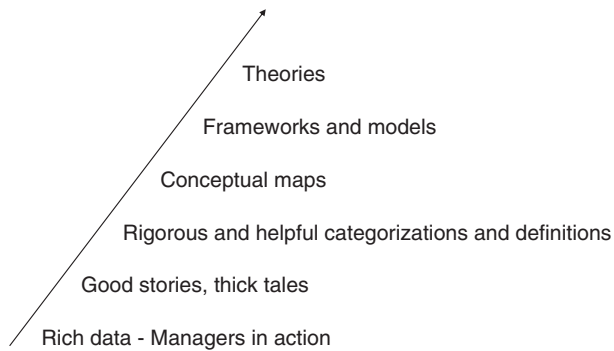
- Showing rather than telling findings, that is, presenting the evidence and allowing the reader to take (vicariously) the same learning journey.
- Developing theoretical findings that can stand on their own, free of the evidence that led to them, that is, using the data as a scaffolding to develop theory, not as a “load bearing” structure.
- Being sensitive to the reality that a “good” theory is what convinces the experts and thus that it needs to be aesthetically attractive and intuitively pleasing.

● Perhaps most important, and often most neglected: a serious, rigorous, deep methodological grounding is required. In the same way as it would not occur to a quantitative researcher to submit an academic paper without an extensive discussion of data and methods, it is incumbent on us, qualitative researchers, to apply hard criteria of research rigour in our work and to report in our publications how we strived for the highest level of rigour (see Dubois & Gibbert, 2010; Gibbert & Ruigrok, 2010; Gibbert, Ruigrok, & Wicki, 2008; and Pratt, 2009, for interesting discussions of these issues). This discipline and rigour is what differentiates academic qualitative research from journalism or merely chronicling interesting business situations.

In addition, qualitative research and the theories it contributes to building have to be compelling, attractive and persuasive (Siggelkow, 2007; Weick, 1989). And to a large extent this depends on the

relevance of the issue being studied at a particular point in time (Buckley, 2002; DiMaggio, 1995; Doz, 2004) and the writing skills of the researchers. Table 1 summarizes in outline form what I see as major requirements for good quality qualitative research. Although at the time we did not formalize the nature of the research work to such an extent, the work on multinational companies with CK Prahalad and the subsequent work on strategic alliances applied many of the methodological propositions articulated in Table 1.

Following the prescription outlined in Table 1, building an inductive theory of a phenomenon follows a roughly similar sequence of steps as sketched on Figure 1. Rich data are required on managers in action in context. The data need to cover activities and interactions, actual processes, antecedents and consequences of activities by participants in a relational system. Such rich data yield a good story, or as Van Maanen (1988) put it, a



**Figure 1** The theory building ladder.

“thick tale”, a multi-actor narrative of the unfolding of events in a company – or a subunit – trying to address a key management challenge. From the story and the sequence of events and actions it aggregates, categorizations and definitions result, for instance of types of action. Categories and definitions have to be rigorous and helpful in analysing the process observed. This is perhaps where qualitative research work can contribute most: it allows a conceptualization from the standpoints of the actors at work, not an abstract objective but distant researcher’s categorization. Conceptual maps and frameworks structuring and explicating the process observed are the next steps in theory development. Conceptual maps provide the structural short-hand from which to build theory. From the particular, concrete, experiential and personal they constitute the main step toward developing general, abstract, non-experiential and formal theories.

In addition to the requirements outlined, which are a common core to good qualitative case-based research, IB adds some specific challenges of its own (Piekkari & Welch, 2010; Piekkari, Welch, & Paavilainen, 2009). At the most basic level, language and cultural norms may require specific contextual sensitivity and perceptual acuity on the part of the researcher. Espoused theory vs theory in use, for instance in how managers relate to their subordinates, may differ considerably among subsidiaries of a multinational company. A researcher insensitive to these differences and hostage to his/her own context and culture of origin might miss the differences, as well as the potential equifinality between vastly different behaviours across cultural contexts. Beyond linguistic limitation, interviewing skills and even circumstances might differ significantly across cultures, national and otherwise.<sup>5</sup>

## LOOKING FORWARD

The evolution of the IB field has largely run its course, and the existing dominant theoretical paradigms – transaction cost economics and cultural studies – are unlikely to take us much further, as Buckley already hinted, at least for the economics paradigm, back in 2002 (Buckley, 2002). Recent research moves beyond these theories. Some issues, perhaps such as the theory of the MNC (not of its management though), have to a large extent been “solved” (Buckley & Casson, 2009). It is not clear that in a domain-defined field suggesting a focus on a single core research question (Peng, 2004), important as it may be, would not restrict the field, so to venture into suggestions for areas where qualitative research would be particularly useful is difficult.

Yet, a number of promising research areas and practical needs are confronting IB today. And overall, the IB field is growing. To mention just a few:

- At the most basic level, the pipeline of qualitative research on the management of leading international companies, and more generally the empirical research stream on the functioning of the multinational companies, have largely dried up. We know less about how multinationals actually operate today, or about their current “globalization”, than we did about multinationals at the time of the Harvard Multinational Enterprise project in the 1970s, or up to a decade ago when Bartlett, his co-author Ghoshal and others were churning out remarkably insightful case studies. IB researchers need to rekindle this tradition, or they risk becoming radically irrelevant, caught between an inability to develop original theory and the difficulty of analysing empirical phenomena.
- The future course of IB today is more than ever conditioned by our collective ability to handle complex worldwide externalities, climate change and energy, water availability and food supply, growing migratory pressures, the “peopleization” of democracies (with populist leaders operating vacuously in the face of constant media attention), security and terrorism, to mention only the most obvious. This calls for research on complex multi-party negotiation and collaboration. International relations and diplomacy schools have delved into these areas, so have negotiation theorists (e.g., Axelrod, 1984, 1997; Gray, 1989). Increasingly, firms’ strategies are going to require the understanding and negotiation of complex collaborative situations (think of the Kazakhstan oil and gas

industry, and in particular of the pipeline involving the collaboration of nine oil companies and of about the same number of nation states for an example). Issues of energy, water, and more generally, sustainability, are not confined to extractive industries; consider the cluster of issues of larger IT server farms, cloud computing, data security and power consumption!

- Globalization has moved from goods and capital to people. Migration is of course not new – the largest waves took place across the Atlantic in the 19th century – but what is new is the growing population of bi- or multi-cultural people, not even so much among executives as among workers. People with mixed cultural identities and backgrounds are becoming a significant factor, one hardly recognized by traditional cross cultural studies. Beyond quantitative methods – standardized surveys and structured interviews – qualitative ethnographic work is needed to understand and appreciate the potential contribution of bi- and multi-culturals as actors in context and what they can bring to multinational companies and the complex global collaboration needs outlined above (Thomas, Brannen, & Garcia, 2010).

The organization and management of multinational companies, complex multiparty collaborative and competitive efforts on critical issues, and changing demographics and migratory pressures are but three issues for IB where qualitative research might help further our understanding. The field is likely to remain phenomenon-driven as new issues and phenomena emerge.

In conclusion, because IB is such an open field – the research agenda of which is more often driven by emerging real life problems and opportunities that need theoretical attention and then testing, building on application of theories – qualitative research methods offer the opportunity to help move the field forward and assist in providing its own theoretical grounding.

### NOTES

<sup>1</sup>A parallel may be drawn here with the transitory role of the “International Division” in US multinational

companies: when the attention of the business divisions was primarily home-centric (US-centric) international divisions provided a countervailing power, bringing management attention to international markets. Once a company internationalized, international divisions were disbanded, business divisions became “global”, but attention often remained or reverted to being home-centric.

<sup>2</sup>Of course, the boundaries between qualitative and quantitative research can at times be fuzzy, for instance around text analysis or in quantitative analysis of case data, such as in the chronology of events.

<sup>3</sup>Christopher Bartlett is one of the very few researchers in IB to have explicitly combined the development of research cases and teaching cases in a joint effort. For instance, cumulatively, the whole stream of cases on Procter and Gamble he wrote over a number of years constitutes a set of very insightful snapshots of the internationalization process of a major multinational (P&G, Vizir, Ariel Ultra, SKII). Together with Sumantra Ghoshal, who was the theory builder where Bartlett was the empiricist, they contributed significantly to our understanding of multinational companies.

<sup>4</sup>The risk of communicating evidence via teaching cases is obvious: under the appearance of a story a teaching case usually contains a strong theoretical or conceptual hidden structure it is designed to surface, illustrate and communicate in a pedagogical process.

<sup>5</sup>One cannot help but recall a revealing anecdote. American researchers studying the aircraft industry had a long day of hard research interviews in a windowless meeting room at Boeing, surviving on coffee and snacks. The same research team, upon flying to Toulouse, France, to meet Airbus management, was taken directly to a three star restaurant around noon and treated to a lavish lunch. Finally, around four in the afternoon, one of the researchers asked the seniormost Airbus executive about going to his office and having the interview, to which the befuddled executives retorted “Oh, we had quite a long interview over lunch, didn’t we? Let me take you back to the airport”. At that point the researchers deeply regretted having exchanged opening their note pads for several glasses of wine! Their memory was blurry.

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