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Toward experimental international business : unraveling fundamental causal linkages

Reference:

van Witteloostuijn Arjen.- Toward experimental international business : unraveling fundamental causal linkages
CROSS CULTURAL MANAGEMENT - ISSN 1352-7606 - 22:4(2015), p. 530-544
Full text (Publishers DOI): <http://dx.doi.org/doi:10.1108/CCM-06-2015-0075>

Cross Cultural Management: An International Journal

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Article information:

To cite this document:

Arjen van Witteloostuijn , (2015), "Toward experimental international business: unraveling fundamental causal linkages", Cross Cultural Management: An International Journal, Vol. 22 Iss 4 pp. -

Permanent link to this document:

<http://dx.doi.org/10.1108/CCM-06-2015-0075>

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Toward Experimental International Business

Unraveling fundamental causal linkages

THE EXTERNAL VALIDITY MYTH

In terms of an empirical research design, the randomized experiment is without any doubt the Holy Grail. Only then can causalities be really unraveled; any other design cannot be but imperfect, notwithstanding the impressive econometric toolkit packed with a plethora of correction tricks (see, e.g., Antonakis, Bendahan, Jacquart, & Lalive, 2010). Indeed, in their *Journal of International Business Studies (JIBS)* editorial, Reeb, Sakakibara and Mahmood (2012: 211) argued that “In general, the iconic test procedure involves developing a random experiment, regardless of whether the unit of analysis centers on individuals, firms, industries or countries.” The dominant manifestation of the randomized experimental design is laboratory research. The term “laboratory” (or lab, for short) refers to any artificial experimental setting, from computerized rooms with cubicles and classroom contexts to mobile laptop facilities and Internet platforms. Strictly speaking, of course, experiments are not limited to lab settings, as is clear from the popularity of field experiments in Economics (see, e.g., Harrison & List, 2004; List, 2011).¹ However, in International Business (IB) and Management, lab and experiment tend to come together, because (particularly Western and international) enterprises are rarely willing to engage in randomized field experiments within their organizations (but see Bandiera, Barankay, & Rasul, 2011, for intra-firm field experiments in Economics).² Therefore, this editorial mainly relates to lab work.³

Regrettably, the laboratory research design is still not part of the standard toolkit of Management (and International Business) scholars. In 2008, the then *Academy of Management Journal (AMJ)* editor Colquitt concluded that “A search of the past decade reveals that about 5 percent of the articles published in *AMJ* are lab studies” (p. 617). Five years down the line, he observed that “12 percent of the articles published in the past three years [in *AMJ*] included a laboratory experiment” (Colquitt, 2013: 1514). In International Business (IB, for short),⁴ such double-digit figures are nowhere to be seen. Rather surprisingly, immediately after praising the power of the experimental method, *JIBS* editors Reeb, Sakakibara and Mahmood (2012: 211) continue by concluding that “Unfortunately, in international business research, we are seldom afforded the luxury of a randomized controlled experiment.” Although they do not explain why this is so, my guess is that this has to do with the external validity myth (cf. Falk & Heckman, 2009).

From my own experience, looking at my large pile of lab experiment studies rejected by IB journals (and their Management⁵ counterparts at large, for that matter),⁶ this is how the myth is constructed, and uncritically maintained, by editors and reviewers. The myth’s steppingstone is the widespread belief that IB research has to be relevant for management practice. This is immediately clear from the oftentimes mandatory “Managerial Implications” (sub)section that is omnipresent in IB journals.⁷ The next step in this mythical logic is that the lab is an artificial environment so far removed from the real world of IB that the lab study’s external validity cannot be but very low.⁸ This myth implies two debatable claims – one, that lab experiments are associated with low external validity; and two, that external validity is essential – if not a *conditio sine qua non* – for each and every individual unit of IB research.

For one, as any other piece of research, lab studies may or may not be associated with low external validity. In fact, as pointed out by Colquitt (2008), the correlations between lab and out-of-lab studies are often quite high. For example, the meta-analysis of Cohen-Charash and Spector (2001, 2002) reveals a very high correlation of .97 between effect sizes reported in lab *vis-à-vis* field studies regarding justice. Additionally, claiming external validity is pointless without internal validity. After all, only internally valid findings can be sensibly generalized.⁹ Moreover, requiring each and every piece of research to be externally valid is not only nonsensical and unrealistic, but also frustrates scientific progress. External validity should rather emerge from a lengthy and cumulative series of highly internally valid studies regarding the issue at hand. Here, triangulation is key, using a variety of different and highly complementary research designs, including lab studies. As Colquitt (2008) argues, the external validity critique cannot be substantiated by the method of science, but rather comes from what he refers to as misguided methods of authority, intuition and tenacity – i.e., in my own words, the non-thoughtful and non-reflexive replication of nonscientific beliefs or statements.

THE INTERNAL VALIDITY MIRACLE

The extremely valuable, and difficult to overrate, twofold advantage of randomized experimentation is high internal validity in combination with identification of causality (cf. Spencer, Zanna, & Fong, 2005; Falk & Heckman, 2009). Key is the randomization of a controlled treatment, similar to drug-and-placebo studies in Medicine. A randomized half of the participants receives the ‘drug’ (say, performance-based pay) and the other randomized half the ‘placebo’ (say, a fixed salary) in an experimental setting (say, in which they have to decide to compete or collaborate: cf. Boone, De Brabander, & van Witteloostuijn, 1999; Huck, Müller, &

Normann, 2004). This offers a powerful test of the causal effect of the treatment (performance-based pay) on the outcome variable (to compete or cooperate) in a noise-minimizing environment (a laboratory setting). As Colquitt (2008: 616) rightly points out, “For this reason, random assignment [in combination with a treatment] has been termed ‘the great *ceteris paribus*’ of causal inference (Cook & Campbell, 1979: 5; italics in original).”

Because randomization in combination with a treatment is (argued to be)¹⁰ rarely doable in IB- and Management-related fieldwork, by far the majority of randomized experimental studies are conducted in a lab. In Economics and Psychology, the randomized experimental lab design is well established. Take the not-so-obvious example of Economics. Already in 1982, Charles Plott wrote a review article in the *Journal of Economic Literature* entitled “Industrial Organization Theory and Experimental Economics”. In 2002, Vernon Smith¹¹ was awarded the Nobel Prize for Economics for his impressive work, largely conducted in lab settings (Smith, 2003). In the current year 2015, the highly reputed journal *Experimental Economics* publishes its 18th volume. And quite frequently, such Economics-inspired lab studies are published in *Science* (see, e.g., King-Casas, Tomlin, Anen, Camerer, Quartz, & Montague, 2005).¹²

Lab experiments come in many different forms and shapes. An important observation is that the tradition in Economics in this respect is very different from that in Psychology, with IB and Management being undecided. Specifically, contrary to Psychology, Economics cares about two very strict principles: a ban on deception and a need for incentive compatibility (see, e.g., Schade, 2005).¹³ The ban on deception implies that the experimental instructions should transparently and truthfully explain how participants would earn their money, and incentive compatibility means that the participants’ (monetary and non-monetary) incentives should closely match the experimental stimuli.¹⁴ Only then, according to experimental economists, will

participants really care about the experiment in the way they are supposed to care about the experiment. This is quite different from the Psychology tradition, where “deception” and non-compatible monetary incentives abound.¹⁵

IB being a multidisciplinary domain, I would suggest an agnostic attitude in this respect, in principle: Depending upon the study at hand, either the Economics or the Psychology tradition will be appropriate. However, applying Economics’ strict principles throughout would certainly be counterproductive. For instance, in experimental IB studies of a foreign language effect on competitive versus cooperative behavior (e.g., Akkermans, Harzing, & van Witteloostuijn, 2010) or altruistic versus prosocial investment (e.g., Urbig, Terjesen, Mühlfeld, Procher, & van Witteloostuijn, 2015), **not** revealing this purpose to the participants is essential in order to avoid unwanted priming that would seriously bias the results. After all, if we were to announce upfront that the purpose of the experiment was to explore the potential effect of using English *vis-à-vis* Dutch as the language of instruction on competitive versus collaborative or altruistic *vis-à-vis* prosocial behavior, this would have frustrated the experimental aim by affecting the participants’ decision-making.¹⁶

More generally, I would argue that applying the strict principles of Experimental Economics, particularly incentive compatibility, is often problematic in IB. Incentive compatibility in its most general terms, and from its origin in mechanism design, means that “a process is **incentive-compatible** (IC) if all of the participants fare best when they truthfully reveal any private information asked for by the mechanism” (Wikipedia)¹⁷ – that is, as indicated above, when participants care about the experiment in the way that the experimenters want them to care about the experiment. If participants do not care in the way they are supposed to, given the experiment’s aim, why would we believe that their responses reveal anything interesting?

Basically, this argument implies that Economics rejects the assumption that hypothetical decisions can be as revealing as decisions with real (even if small) consequences: i.e., people behave differently when there is no real (monetary) consequence.

What Economics rejects here is the assumption of what is known as Epsilon Truthfulness: “*If the agent is indifferent between lying and telling the truth, assume he tells the truth* (see Rasmusen 1989, p. 161)” (Cummings, Elliott, Harrison, & Murphy, 1997: 610; italics in original). This tends to be problematic in an IB context for at least two crucial reasons. First, the nature of IB-relevant behavior and decision-making implies that oftentimes we have to revert to hypothetical decision-making. For example, implementing an internationalization decision in a lab setting in a non-hypothetical manner is not an option. Second, the IB community expresses, time and again (but see endnote 14), a strong preference for non-student samples, arguing that we cannot expect undergraduates to have the background and experience to sensibly relate to IB issues. However, providing IB-experienced participants (e.g., MBA students or MNE managers) with meaningful monetary incentives is more often than not far too expensive, if not plainly unaffordable.

Psychology does not adhere to both strict principles – quite the contrary. For instance, classroom experiments with undergraduate students for course credits or limited monetary rewards are very common. Indeed, in the majority of my own experimental work regarding language and personality, only a very few of the participants were rewarded a monetary prize (see, e.g., Boone et al., 1999, vis-à-vis Urbig et al., 2015). Actually, course credit rewards are standard practice in a wide variety of Psychology journals, such as the *Journal of Experimental Social Psychology* (e.g., Bos, Dijksterhuis, & van Baaren, 2008) and *Psychological Science* (e.g., Yu & Smith, 2007), as well as in other non-Economics disciplines (see, e.g., Cheng, Paré,

Collimore, & Joordens, 2011, in *Computers & Education*; Dijksterhuis, Bos, Nordgren, & van Baaren, 2006, in *Science*; and Yeung & Sanfey, 2004, in the *Journal of Neuroscience*).

The above is not to say that the strict Experimental Economics protocol is always useless in the context of IB work – quite the contrary; my only point is that being dogmatic in this respect will be counterproductive, given the nature of much of IB. Particularly with student samples, though, satisfying the incentive compatibility condition is not so much of an issue. Moreover, Experimental Economics has developed a large number of experimental designs, often associated with ready-to-be-used z-Tree software, which can be easily and productively turned into lab settings highly relevant from an IB perspective – e.g., by imposing an international frame, by collecting IB-relevant survey data, and / or by focusing on international participants. For instance, the many market designs developed in Experimental Economics are highly suited to run IB-relevant experiments by adding a cross-cultural twist to the study of competition (or collaboration). Below, I will briefly refer to a few examples in somewhat greater detail.

A DESPERATELY NEEDED TREATMENT

A cursory electronic search in IB's top journal – the *Journal of International Business Studies* – in May 2015 revealed that laboratory work is very scarce indeed. Using search keys such as *experiment* and *laboratory* identified six studies. Of these six studies, only two turned out to really use a randomized laboratory experiment (close to) as defined above. Buckley, Devinney and Louviere (2007) report the findings of 70 managers making investment choices in a controlled setting, and Grinstein and Riefler (2015) include three randomized

experimental framing paradigms. But Bruton, Khaval and Chavez (2011) utilize a series of non-experimental qualitative case studies; Lam, Ahearne and Schillewaert (2012) use a randomized survey method; Williams and Grégoire (2015) develop a verbal protocol design; and Harmeling, Magnusson and Singh (2015) employ a web survey. Of course, this selection of six is unlikely to be representative. In *JIBS*, and in the other IB journals, lab-based work has been published, certainly in the domain of cross-cultural Psychology. But this disclaimer notwithstanding, randomized experimentation is a rarity in IB (cf. Reeb et al., 2012). In this editorial, my key plea is that the IB domain would benefit from a desperately needed treatment, by establishing a powerful (lab) experimental tradition, as I hope to illustrate below by providing a series of examples.

In this context, I would like to argue that, apart from “pure” lab studies, such an experimental tradition in IB could benefit from mixed designs involving (randomized) experiments in combination with (participant) surveys. Indeed, often, the relatively rare IB- and Management-relevant laboratory work involves such quasi-experiments. That is, a randomized treatment design is combined with survey data in order to explore how individual attributes affect attitudes, behaviors, decisions and / or outcomes. For instance, Boone et al. (1999) explore the impact of a series of personality traits (i.e., locus of control, self-monitoring, sensation seeking, and type A/B behavior) on competitive or collaborative behavior in prisoner’s dilemma games; Urbig, Weitzel and van Witteloostuijn (2012) examine the influence of entrepreneurial intent on investment decisions in externality games; and Esteve, Urbig, van Witteloostuijn and Boyne (2015) investigate the effect of public service motivation on common good contributions in public goods games. This line of work shows that the combination of a randomized treatment experiment with survey information offers a powerful design for exploring fundamental

causalities determining individual attitudes, behaviors, decisions and outcomes. And as a by-catch, this may also help to increase the lab experiment's external validity.

This quasi-experimental design is not only powerful at the individual level of analysis, but also at higher levels of analysis. For instance, Mühlfeld, Weitzel and van Witteloostuijn (2013) reveal how the performance of an experimental asset market is affected by the distribution of traders with the very fundamental Behavioral Approach System-Behavioral Inhibition System (BAS-BIS) traits.¹⁸ BAS and BIS are two deeply rooted motivational systems. BAS is the driving force or 'engine' of human behavior, making people respond actively to stimuli; BIS is the opposite force, or the 'break', triggering halting behavior. In their experimental asset market with 50 student traders and exogenous shocks (the treatments), pushing the asset's fundamental value up or down, Mühlfeld et al. (2013: 206) find that "After a positive shock, a higher median BIS level is associated with lower bid-ask spread, greater informational efficiency, and reduced volatility, compared to pre-shock levels." This study is an example of a research design combining a macro-level lab experiment with micro-level survey data.

Actually, my argument is that experimental studies are **particularly** appropriate in the context of cross-cultural and International Business research for at least two reasons. First, in cross-cultural and / or international contexts, extremely subtle but far-reaching **fundamental** processes affect attitudes, behaviors, decisions and outcomes. The prior example of the effect of foreign language is a revealing point in case (see also, e.g., Neeley, 2013). Laboratory experimentation is highly suited to explore such complex, deep and subtle fundamental processes, which are clearly generalizable to real-life contexts. As Bello et al. (2009: 362) argue, "Fundamental processes, structures and outcomes are concerned with the basic characteristics of

human nature that are relatively independent of context and life experiences.” Indeed, Buckley et al. (2007: 1087-1088) come to a similar conclusion, arguing that IB has “to date, borrowed heavily from economics, sociology, social psychology and management but little from cognitive psychology or the rising field of experimental economics. This has limited the field in many ways, most clearly in the study of the role of the individual decision-maker”. In a lab setting, the associated causal linkages driving IB-relevant decision-making can be carefully unraveled, step-by-step, by working on a cumulative chain of randomized treatment studies.

Second, precisely because external validity is a huge challenge in cross-cultural and international fieldwork, particularly if of a comparative nature, the laboratory environment provides a powerful design complementary to fieldwork (see, for a similar argument, Buckley et al., 2007). In cross-cultural and international studies, different cultures and languages introduce much measurement noise, as we know from the literature regarding translation issues (Harzing, 2006) and the etic-emic dilemma (Davidson, Jaccard, Triandis, Morales, & Diaz-Guerrero, 1976). In fieldwork, this noise comes on top of a plethora of other measurement errors, such as common-method bias, omitted variables and endogeneity issues (e.g., Podsakoff, MacKenzie, & Podsakoff, 2012). In a lab context, fieldwork-related measurement error can be avoided, providing the opportunity to study subtle cross-cultural and international issues in a controlled randomized setting. Again, the above-discussed foreign language effect issue offers an illustrative example (Akkermans et al., 2010; Mühlfeld et al., 2015; Urbig et al., 2015). The lab setting is very well-suited to unravel the relative importance and potential interplay among alternative explanations of a foreign language effect, such as cognitive load, lack of understanding and cultural accommodation (Volk, Köhler, & Pudelko, 2014).

A BASKET WITH GOLDEN EGGS

A few examples, out of many, should suffice to illustrate the huge potential of Experimental IB, fitting within either the Economics or Psychology experimental traditions, or both. For one, at the macro-level of the market, Experimental Economics has a long tradition of investigating a wide variety of issues relating to market competition (see, e.g., the early review of Plott, 1982). From an IB perspective, these experimental market designs can be the platform from which to develop quasi-experiments by adding an international dimension in terms of a participants' country-of-origin effect. An interesting take may be to focus on the impact of culture or cultural differences on market outcomes. For readers of this journal, it will not come as a surprise that country-of-origin has an impact on participants' behavior in experimental settings (see, e.g., Boone & van Witteloostuijn, 1999; Oosterbeek, Sloop, & van de Kuilen, 2004). In the context of market experiments, adopting an IB lens immediately triggers two questions that can be examined easily in a lab setting, using well-established experimental market designs:¹⁹ (1) Do markets behave differently in one cultural context than they do in another?; and (2) How do firms from country A behave if they compete against enterprises from country B?

The first question is cross cultural in nature, starting from the baseline case of market-level cultural homogeneity. Do, for example, competition outcomes (like tacit collusion and price volatility) among American participants only evolve differently than would these outcomes among Chinese participants only? Extant culture theory can be applied to develop hypotheses here,²⁰ which can be far more easily and "purely" explored in controlled lab settings than in very noisy fieldwork contexts. The second question is international in nature, involving market-level cultural heterogeneity. For instance, does a market with one-third American and two-thirds Chinese participants generate outcomes (again, e.g., in terms of tacit collusion and price

volatility) that are different from the ones with two-thirds American and one-third Chinese participants?

Additionally, at the micro level of individual decision-making, an example from Psychology involves the impact of personality – for instance, HEXACO and the Dark Triad – on all kinds of IB-related choices.²¹ The relatively recent personality trait battery known as HEXACO is widely regarded as a valid measure of an individual's core personality traits (Ashton & Lee, 2001; Lee & Ashton, 2004). HEXACO is an improvement over the well-known Big Five personality trait model, adding a sixth trait (i.e., Honesty-Humility (H)) and modifying the earlier Big Five dimensions: Emotionality (E), eXtraversion (X), Agreeableness (A), Conscientiousness (C), and Openness to Experience (O). In their 2004 paper, Ashton and Lee introduce their 192-item HEXACO Personality Inventory (HEXACO-PI),²² and Ashton and Lee (2009) present the shorter HEXACO-60 scale as used by Esteve, van Witteloostuijn and Boyne (2015). The Dark Triad personality traits are Psychopathy, Machiavellianism, and Narcissism (Paulhus & Williams, 2002) – all three are associated with “dark” attitudes and behaviors such as callousness, exploitation, insincerity, manipulation, and superiority. Lee and Ashton (2005) offer evidence of the association between HEXACO and the Dark Triad.

Kets de Vries (see, e.g., his 2004 *Harvard Business Review* interview) has convincingly argued that those who make it to the top in large enterprises tend to possess Dark Triad-like attributes and reveal *ditto* behaviors, which have upsides and downsides in leadership roles. Lab studies can be designed to examine how such personality traits – HEXACO and the Dark Triad – are related to IB-relevant attitudes, behaviors, decisions and outcomes. For instance, do such personality traits impact collaborative behavior (e.g., in a prisoner's dilemma game) or common good contributions (e.g., in a public goods game), depending upon the nationalities of the

participants concerned? Does, say, a highly extravert and narcissistic participant from Brazil make different choices in a public goods game than a comparable personality from Russia? And if so, what is the outcome if both are knowingly (not) from the same country-of-origin group? Lab studies are not only appropriate because this issue involves basic and fundamental human processes, but also because measuring such personality traits of MNEs' upper echelons is notoriously difficult, due to no or limited access to the top management teams' ivory towers. More generally, this implies that (quasi-)experimental lab studies offer a highly appropriate design for the examination of the impact of many – if not all – deep-level characteristics of participants in roles of MNE employees and managers on IB-relevant attitudes, behaviors, decisions and outcomes.

Finally, combining the macro-market and micro-individual perspectives, another promising example is to focus on explicit and implicit motives (particularly the need for achievement, affiliation, and power), which – in isolation and in combination – are known to have far-reaching but divergent consequences for business-related behavior (see Slabbinck, van Witteloostuijn, Hermans, Brassey, Ramdani, Vanderstraeten, & Dejardin, 2015). The general claim that motives and rewards²³ subtly interact in driving behavior is well established (see, e.g., Deci, Koestner, & Ryan, 1999). Specifically, a chain of psychological work has evolved into a fine-grained theory of human motives (see, e.g., McClelland, Koestner, & Weinberger, 1989; Schultheiss & Brunstein, 2010), explaining why and how explicit and implicit motives, in conjunction with their (non)congruence, affect attitudes, behaviors, decisions and outcomes differently. What is missing, though, are insights into how such motives affect market-level outcomes in both cross-cultural and international settings, as introduced previously.²⁴

THE WAY FORWARD

Already in the classic book *The Behavioral Theory of the Firm* (1963) of Richard Cyert and James March, a chapter was included that reported the findings from an experimental lab study, based on Cyert, March and Starbuck (1961); and in 1965, Karl Weick had a chapter titled “Laboratory Experimentation with Organizations” in the *Handbook of Organizations* edited by James March. But for whatever reason, probably the external validity myth, an Experimental Management tradition never really gained momentum. To me, however, the lab experimental design has a very bright future in IB (and Management at large). The experimental design is so powerful and the to-be-studied IB issues are so rich that bringing both together carries a great promise. To facilitate the development of an experimental IB tradition, initiating Web-based tools would be highly instrumental. This will not only boost further progress in IB research, but will also increase the effectiveness and playfulness of IB teaching. The well-known experimental economist Charles Holt’s classroom games (<http://people.virginia.edu/~cah2k/teaching.html>) are a good example, offering about 60 Web-based experimental programs in the Experimental Economics tradition to the worldwide Economics’ community.²⁵ In an IB context, an additional Web-based service could be to pool participant samples – student and non-student – across the globe, providing access to a very international subject pool.

In this editorial, I would like to argue that the time is ripe for establishing an Experimental (International) Business, similar to the well-established traditions in such fields as Economics, Finance, Marketing, and Psychology.²⁶ Given my arguments in this editorial, it will hardly come as a surprise that I wholeheartedly agree with Falk and Heckman (2009: 535), who argue that “With the exception of psychology, the adoption of laboratory experiments has been much slower in the social sciences [compared to life and natural sciences] ... there remains

considerable resistance among social scientists who argue that lab experiments lack ‘realism’ and ‘generalizability’ ... We argue that many recent objections against lab experiments are misguided and that even **more** lab experiments should be conducted” (emphasis added).²⁷ Hence, given the high potential of an Experimental IB, the *Cross-Cultural and Strategic Management* journal will offer a platform for such exciting and intriguing work, cumulatively contributing to the establishment of an Experimental IB tradition.²⁸

ACKNOWLEDGEMENTS

I gratefully acknowledge the very constructive comments provided by Bas Bosma, Dessi Dikova, Anne-Wil Harzing, Shaista Khilji, Katrin Mühlfeld, David Ralston, Vasyl Taras, Rosalie Tung, Diemo Urbig and Utz Weitzel. Of course, the usual disclaimer applies.

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NOTES

¹ The lab versus field experiment distinction is not absolute (cf. Harrison & List, 2004). For instance, one may argue that randomized vignettes studies with non-students bring the lab into the field (see, e.g., Aguinis & Bradley, 2014; for a recent IB example, see Baack, Dow, Parente, & Bacon, 2015).

² An inspiring and typical example is Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013). However, the typicality of this example makes clear why the observation that organizational field experiments in IB are so scarce hits the nail on its head. Bloom et al. (2013) is a very expensive study in Indian textile enterprises, backed by the World Bank. Without the support of a rich authority like the World Bank, organizational field experiments are notoriously difficult to get off the ground in the Western world, let alone in collaboration with MNEs.

³ This is not to say that field experiments should **not** be part of an Experimental International Business tradition – quite the contrary: they should, absolutely so. Actually, the classic Hawthorne studies were precisely that: an intra-firm field experiment (cf. Franke & Kaul, 1978). But being associated with specific bottlenecks (e.g., of access, costs and ethics), the issue of field experiments in IB requires an editorial of its own. Note that many of the issues discussed in this editorial apply to field experiments, too. However, in this editorial, we first set the ground for lab work in IB, as this type of work is far more controversial in the IB community (and that of Management at large, for that matter) than are field experiments.

⁴ In the literature, subtle debates can be found regarding differences and similarities between International Business *vis-à-vis* International Management. In this editorial, for the sake of parsimony, I use International Business as an inclusive label, also covering what others might consider to be International Management.

⁵ As a mirror image of what is explained in endnote 4, this is a catch-all label, also covering what some might consider to be Business. To restore symmetry, in this editorial, I refer to Management rather than Business.

⁶ Such negative biases are not restricted to lab experiments. Another prominent example is mathematical modeling. Rarely is a paper that only presents a mathematical model accepted for publication in an IB journal. Of course, not only IB (or Management at large, for that matter; see, e.g., Adner, Pólos, Ryall, & Sorenson, 2009) suffers from such biases; all (sub)disciplines do. For instance, Economics is associated with a few opposite prejudices. Having degrees in multiple disciplines myself, including Economics and Psychology, I always struggle to adapt to disciplinary mores, depending upon the audience, implying that I regularly punch my nose.

⁷ I will not engage here with the endless rigor versus relevance debate in the Management scholarship community (see, e.g., Gulati, 2007), nor will I discuss the naïve assumption that managers really read all these “Managerial Implications” (sub)sections – an issue that deserves a separate editorial.

⁸ What makes matters worse is that by far the majority of lab studies use student samples. This issue, too, deserves a separate editorial (but see, e.g., Harrison & List, 2004; Bello, Leung, Radebaugh, Tung, & van Witteloostuijn, 2009).

⁹ Not all agree on this. For instance, in Economics, Friedman (1953) (in)famously argued that the validity of a model’s assumptions is irrelevant if the model predicts well. Similarly, in Psychology, Bartram (2005) emphasizes the dominant importance of predictor validity.

¹⁰ But see endnote 3.

¹¹ In a personal footnote, Smith (2003: 465) points to another advantage of lab work: “Doing experimental economics has changed the way I think about economics. There are many reasons

for this, but one of the most prominent is that designing and conducting experiments forces you to think through the process rules and procedures of an institution. Few, like Einstein, can perform detailed and imaginative mental experiments. Most of us need the challenge of real experiments to discipline our thinking.”

¹² Much, but not all, of this “*Science-fit*” lab work is in the domain of Neuro-Economics (see, e.g., Hare, Camerer, & Rangel, 2009). As yet, probably to a large extent for similar reasons as discussed in this editorial in relation to lab research in general, Neuro-Management or Neuro-IB is close to non-existent.

¹³ A third, but less fundamental one is that Economics has a clear preference for computerized experiments, often using z-Tree software (Fischbacher, 2007), whilst paper-and-pencil classroom methods are much more common in Psychology.

¹⁴ And indeed, without any doubt, incentives do matter, even among monkeys (Brosnan & de Waal, 2003; note that, in this literature, they refer to “nonhuman primates”).

¹⁵ Actually, many Psychology experiments involve (undergraduate) students only receiving course credits in return for their participation. Interestingly, precisely because of the strict requirement of incentive compatibility, Economics tends to be associated with a **preference** for student samples (e.g., Schade, 2005) – strikingly different from IB and Management indeed (Bello et al., 2009).

¹⁶ One could argue that this is not really an example of deception as long as the instructions regarding the incentive mechanism are crystal-clear (for a critical discussion of the deception issue, see Bonetti, 1998).

¹⁷ Formally, following Hurwicz (1972), the (weaker) definition of incentive compatibility (sometimes referred to as “Bayesian”) is that “If a choice mechanism is Bayesian incentive-

compatible, then no player would expect any positive gains from being the only player to lie ... when all others are planning to tell the truth“ (Myerson, 1979: 63).

¹⁸ Somewhat further removed from this quasi-experimental design are business simulation studies (e.g., Boone, van Olffen, and van Witteloostuijn, 1998, 2005; Mühlfeld, van Doorn, & van Witteloostuijn, 2011). But if a business simulation context can be combined with a randomized treatment set-up, this offers rich opportunities for the study of fundamental team processes in semi-controlled settings closer to real-world contexts than “pure” lab studies.

¹⁹ Market designs, or forms, come in many different shapes (see, e.g., van Witteloostuijn & Boone, 2006), as do competitive games (e.g., van Witteloostuijn, 1993). Hence, in later work, the moderating impact of market design can be examined. Initially, however, to really focus on the impact of culture and cultural differences, taking a basic design (e.g., Bertrand or Cournot oligopoly with product homogeneity and nonbinding capacity constraints) can be used in order to first accumulate baseline results.

²⁰ Additionally, this offers ample opportunities, in light of heated debates in the literature, to compare the explanatory power of rival culture theories (for instance, Hofstede versus GLOBE or beyond: see, e.g., Verbeke & Tung, 2010).

²¹ Interestingly, Experimental (or, more precisely, Behavioral) Economics is primarily based on merging in insights from Cognitive Psychology (as is Experimental – or Behavioral – Finance) (see, e.g., Camerer et al., 2011). In IB and Management, cross-fertilization with Personality and Social Psychology is far more influential (cf. Boone et al., 1999), upper echelon theory offering a prime example (Boone, van Olffen, van Witteloostuijn, & De Brabander, 2004; Finkelstein, Hambrick & Canella, 2009). A nice example of a team-level experiment is Staats, Milkman and Fox (2012).

²² Relating to the student samples and incentive compatibility debates, note that all HEXACO's initial validation psychometrics were conducted with 409 undergraduate students from two Canadian universities, many of whom only received course credits in return.

²³ The motives-rewards interaction offers interesting experimental research opportunities as well, by bringing strategic delegation theory into the lab (cf. Huck et al., 2004; Jansen, van Lier, & van Witteloostuijn, 2007; Jansen, van Lier, van Witteloostuijn, & von Ochssee, 2012). How do specific CEO bonuses interact with these CEOs' explicit and implicit motives in driving their competitive behavior in the marketplace, depending upon the cross-cultural and international context (as defined in the main text)?

²⁴ Moreover, as far as I know, cross-cultural work regarding explicit and implicit motives is rather limited (but see Hofer, Chasiotis, Friedlmeier, Busch, & Campos, 2005; Hofer, Chasiotis, & Campos, 2006), which offers another opportunity for future work: Are particular explicit and / or implicit motives more or less prominent in one culture or country *vis-à-vis* the other?

²⁵ Holt's work is a good example of how research and teaching can meet synergetically, with teaching feeding into research and the other way around. Holt has published a series of papers in the Economics' top journal *Journal of Economic Perspectives*, arguing how classroom games can be used as effective teaching tools (e.g., 1996, 1997 and 1999). By far the majority of my own lab experimental publications were born in the classroom, too.

²⁶ Note that Experimental Economics and Experimental Finance are closely related to the "behavioral revolution" in both domains (see, e.g., Barberis & Thaler, 2003; Camerer, Loewenstein, & Rabin, 2011).

²⁷ Falk and Heckman (2009) refer to a few other misguided critiques of lab research, such as those related to small sample sizes and subject self-selection.

²⁸ Of course, the proof of the pudding is in the eating. As this editorial is not the place to provide detail as to how an IB experimental design could look like, I can only advise those interested to consult the many examples referred to in the main text. Perhaps, the pair of lab experimental language effect studies of Akkermans et al. (2010) and Urbig et al. (2015) offer an accessible introduction into the world of IB-relevant quasi-experiments.