

## **QUESTÕES DE PESQUISA EM NEGÓCIOS INTERNACIONAIS**

### **Palestra de BRIAN SILVERMAN**

Professor de Estratégia Internacional na Universidade de Toronto

Editor do Journal of International Business (JIBS)

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#### **Brian Silverman**

I 'm very pleased to be here. In a minute, I 'm going to take this microphone and start walking around because I'm much more comfortable being able to get close to you than just standing here. In fact, I'm going to start that now.

You can still hear me?! Ok, I will try not to break out in song while walking around with the microphone. It really is a tremendous pleasure to be here. Thank you for inviting me to be part of this, and thank you for allowing me to take time here to speak on two different issues. Today about current issues in international business research, and on Friday on more practical or policy-oriented issues about what we can learn from academic research that is of use to managers or of use to policy makers. So again, Thank you Thank you.

One thing before I start. I have a colleague at The Rothman School at the University of Toronto who is from São Paulo originally. His name is Bernardo Blum. He is a very very strong international trade scholar. And every December, January, February, he complains bitterly, very bitterly about the winter in Canada and I never really understood why until I came down here during your winter. Now I realize that your winter is as lovely as our summer and so now I really understand where Bernardo is coming from when he says the nasty things that he says about Canada in our winter time

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<sup>1</sup> Os textos disponibilizados nestes Anais não são transcrições *ipsis verbis*, mas registros elaborados a partir das transcrições do áudio captado durante o evento. Buscou-se, contudo, manter a maior fidelidade possível às falas, assim como preservar suas características de linguagem oral. Além disso, todos os textos foram revisados pelos respectivos palestrantes.



I would love to talk to you today about current issues in international business research. I'm going to talk for some period of time and try to get done quickly within about 35 or 40 minutes in order to leave time for questions, but I'm also used to getting questions as I go – people never let me talk for too long before jumping in with questions. So if you have some along the way of clarification or whatever that you would like to ask, please do and I will put on the headphone and hear what the question is.

So, just to give you a sense as to where we are going to end up today, the basic idea... I'm going to talk in a minute about what I believe are the cutting edge issues in international business research today around theory, around methods and around the phenomena, the issues or research questions that we really care about. I'm going to spend time on each of those three things because, to my mind, when you think about making a real contribution to any kind of research – but in this case to international business research – it involves three or four things. First, it starts with an interesting research question about some phenomenon, be it foreign direct investment or alliances or something hopefully very interesting to you as the author, the scholar, and to readers. Second, when trying to answer this question the paper develops logical hypotheses that come from theory so that's why I'd like to talk a little about theory so that when we actually try to come up with answers associated with our question, we don't just get to say: "Oh, we can answer the question about this alliance at this point in time, but we come up with something that is more general and more enduring. Not only can we come up with an answer about this alliance at this point in time, but we can say something about how the theory of alliances applies or doesn't apply here and how we might extend it.

Ideally, after developing those hypotheses we can assemble good data and run reasonable empirical tests, so that we can convince people, the readers and the referees, the editors – those nasty nasty editors -- so we can convince them that what we've done makes sense and that what we've come to as our conclusion is believable, and that the way we've interpreted the data makes sense. And if we do all that, then, this great study finds results that leave the reader with new insights not just about the particular thing we've looked at, but with insights about the broader theory so that other people can now follow and say: "Hey, well, Silverman found this; now we can push a little further in that direction because of that". So that's why I am going to talk about theory and methods and phenomena and I am going to talk about what are really kind of the top standards in the international journals these days. And I don't want it to sound too



daunting, so my bottom line really is: there are many interesting research questions in international business these days, a huge number of them deal with transitional economies and I know that's a subject of interest to many people in this room. And I want to leave you with the idea that you have a competitive advantage in studying research questions about transitional economies. If you choose to do so -- I don't mean that you should only study transitional economies -- but if you choose to do so, I'm going to suggest later why I think you have an advantage in trying to get at those and so, all you have to do -- if you can marry that advantage with the general approaches, the common approaches, for developing theory today and the common approaches for using methods today, then you ought to be able to make great contributions to the literature. I'd love to see them in JIBS [Journal of International Business Studies] and in other journals that are out there.

So, that's where I'm going but now let me talk about the theory, the methods and the phenomena, one question before I go on: "Am I speaking too quickly? No I'm doing ok. Should I speak faster? No, No".

Ok, so again I'll be talking about theory, methods, and phenomena. I started thinking when I was asked to come and talk about this, "Before we think about where we are going or what the current issues are, maybe it would be useful for me to go back and think about where we've been and where the field has been moving over the last 15 or 20 years." So I went back and looked through JIBS simply because that is the journal that happens to be on my shelf and went through one year of JIBS from about 15 years ago and looked at what kind of theories were big, back in the articles published 15 years ago. Well, in about half the articles there was no theory, these were articles that basically said I am going to look at something. I've looked at it. How do you like it? To the extent that there was theory being applied in these articles there was a great deal on cultural differences -- things are different in different countries; let's try to understand the degree to which culture plays a role and there are some theories about how culture might matter. And for those of us on the more strategy side of the world -- and that's where I am, in fact let me make one quick aside: I've used this somewhat imperialistic term of International business research today, but please remember I come from the strategy part of the pond so I am really talking mostly about International business strategy research, and there are lots of things that are out there in International business research that I won't really touch on because they are outside of my area and so I apologize for that, but given that -- in strategy, the eclectic paradigm



of Dunning was just beginning to permeate the literature this time and that, of course, is a way of thinking about multinationals and what countries they choose to enter and what activities they choose to do where and why. In terms of methods the methods were relatively simple for those that were not case studies. They were statistical, you typically saw differences-of-means tests and there were just a few cross-section estimations, a lot of surveys, with not much use of archival data. As for the phenomena, here is what really interested people: (1) foreign direct investment. What attracts it? Where do firms go FDI? When do they make these investments? Do they make them by acquiring or by greenfield or joint venture? (2) Cross cultural effects? (3) The organization of a multinational. What do we do in the subsidiaries? What do we do at the headquarters? (4) And some work on national policies that might improve international competitiveness. I'm guessing this may sound generally familiar to many of you.

What happens if we come up to last year? If you look through JIBS from last year, well, on the theory side we still have the eclectic framework, it's fifteen years later and what's interesting to me about the eclectic framework is that it's been augmented by transaction cost economics which is very close to my heart and by the resource-based view and the way I think about it, for those of you who've dealt with the eclectic framework, is one way...The way I think about it is, transaction cost economics is very good at helping to formalize or think more rigorously about the internalization part of OLI. The resource-based view is very good at helping to think a little more systematically about the "O," the ownership part of OLI. And so in some sense the theoretical advances, to my mind, that have come out of strategy and entered IB have been to make slightly more rigorous the OLI framework, but that's really pretty much all that's happened. Well, that's a little stark but that covers a lot of what's happened over the last 15 years. [Added by Brian Silverman two weeks after the talk: In retrospect, I think that I should have also briefly discussed the Uppsala model of internationalization. With more time I would have done so. Please don't take my neglect of this model as evidence that I think it is unimportant.]

In terms of methods, they have become far more advanced. I'll be talking a bit about the cutting edge methods, and the great thing about these methods is that while they are advanced, much more sophisticated, much better at getting accurate results, they really aren't that hard to do. I didn't know a lot of these methods a few years ago. I've worked to learn them and they are not that hard, and if you are not familiar with them they really are relatively easy to pick up, which is great news. So typically we've seen a move towards longitudinal estimation, looking



over time at the evolution of countries or the evolution of firms. We see estimation that better understands what I'm going to call: endogeneity issues, a little while later, and thus is better at getting true effects rather than getting spurious effects, which is crucial for making good policy prescriptions to managers and policy makers. And the survey techniques have gotten better and the... Well, I thought I had something around here about case studies. I'll be talking about case studies later as well, the ways that case studies are done have also been formalized in certain ways that provide the reader with a better sense of... comfort that the case study has been done in a persuasive way.

What is fascinating to me is that the phenomena are very much the same. We are still taking about FDI, we are still talking about cross cultural effects and the organization of the multinational, still talking about national policy, because these are crucial critical issues, they are also enduring issues. What we've seen added recently are alliances, which have become very popular in the real world and so eventually academics have been drawn kicking and screaming to study alliances because they clearly matter to practitioners. And clusters, the idea of geographic concentrations of firms of a particular type, have become very very important as well.

So the bottom line is: it is pretty clear that there is a enduring set of phenomena, research questions that were interesting to people 15 years ago and are interesting to people today and I hope they are of interest to you because I think they are going to be around for a while longer. The theories and the methods that are being used to look at these are getting more sophisticated, but they are still relatively straightforward to pick up and apply. Are you comfortable so far? OK.

What I'm going to talk about for the bulk of the time is where we are going in each of these fields, so I'd like to talk about my best sense of where theory is going, where the exciting interesting issues are going to be, where any of us might think about making a contribution and then do the same for methods and for phenomena and for the theory I'll be talking about extensions of the existing theories of international business and then also talking briefly about taking advantage of advances that are being made in related disciplines and their theories and how we might take advantage of those so: Where are we going in theory?



I am going to assert, I hope nobody throws a rock at this point, I am going to assert that the OLI framework, the eclectic paradigm is by now so widely accepted that it is not interesting anymore. Ok? Maybe you already believe that, but my sense is, the theory in some sense is so eclectic that it is hard to think of anything it can't explain, because, well, that is just the way it is but unfortunately a theory that explains everything ultimately is not very satisfying. So the OLI, it is fabulous, it has gotten us a long way, to my mind the exciting part dealing with OLI in the future, may be the debate over the theories that have formalized elements of OLI, to try to understand which of these theories carries the most weight, does the most work in terms of helping us to understand multinational behavior and location choices and so on. I don't know if you agree with that, but that's my sense and so right now there is this huge debate going on about precisely that. Transaction cost economics has become in my mind now -- remember, I'm an adherent of transaction cost economics, I'm a believer, I'm an ideologue of transaction costs -- but in my view, in my objective view, transaction cost economics has become perhaps the dominant theoretical way to understand why firms choose to internalize certain transactions, why they choose to do certain things as multinationals and so, what we are seeing now is a reaction to that, there's a lot of energy being spent trying to the topple transaction cost economics as perhaps the fundamental underpinning of the eclectic framework. And if you read the journals that I've been reading recently, you see a lot of articles with particular focus from the resource-based view or the knowledge-based view that have been working very hard to say: "Look we don't need transaction cost economics, a knowledge-based view of the world actually explains everything that transaction costs does and more". The big challenge is that, so far, this debate has generated a lot of heat but not much light, a lot of emotion but not much progress. One challenge is that, for those of you who are familiar with this you will recognize, the knowledge-based view of the firm or the resource-based view tends to generate predictions that are very similar to those of the transaction cost view and so one problem is you can argue about which theory is doing a better job but if they both generate the same predictions then it is really impossible to test and see which one is doing better. There is been just a little work recently that has succeeded at teasing apart some of the resource-based versus transaction cost prescriptions and has started to say: "Ok, it looks as though transaction cost economics does a little better job explaining this, resource based view seems to do a little better explaining that, when they generate conflicting hypothesis one seems to do better than the other. To my reading of those works, transaction cost economics is winning, but again, you may want to take my words with a



grain of salt. But what strikes me is that there is a huge potential to join this debate and think about how could we come up with conflicting hypotheses from one theory and another theory – in this case, transaction costs and the resource-based view, but it could be others. How can we come up with conflicting hypotheses and then come up with an empirical test that will, basically run a horse race between these and support one and refute the other? And that ‘s a real advance as we go forward and try to figure out which subset of theories that might fall under the eclectic paradigm are really carrying the most weight, so there you go on theory in the existing IB literature.

How about theory and strategy on managing more generally? So this is the first of the related disciplines I’ll look at and I’ll look at them pretty quickly. There is some research now looking at the application of real options theory and of social networks to understanding international business issues. These are obviously areas that were developed in other parts of the field in mainstream strategy or sociology, but they have been applied recently to IB and here I think that there are some fabulous opportunities to study existing questions better with new theory. And, to actually look at some new questions that we didn’t think about before we had these theories to work with. The examples that I’d like to suggest are: there have been several studies recently that looked at international alliances as real options. What’s particularly fascinating about this area, when we talk about International Business, is that it turns out, I won’t talk about it now but if you request we can talk about it later, there are cases where the way a particular country allows foreign firms to form joint ventures, actually, structures these alliances very much the way real options should be structured in theory. For those of you who are familiar with real options theory, you know that when you look to the real world.... Step back for a second. Real options came from the financial options literature where there are some very fixed rules about what an option should look like. When can you sell? You can only sell you can’t buy, or you can only buy you can’t sell. There are certain timing issues. In the real world it is very hard to find cases that are just like that, but it turns out, for example: in this paper [a working paper by Xavier Martin and Ilya Cuyppers], the way China allows foreign firms to come in and form alliances restricts them in certain ways so that they really do look like options the way theory says they should. So we can apply real options theory to some of these alliances questions and not only understand whether alliances are being used by firms in a option sense, but also get a



better understanding of real options because this provides a particularly nice real world example and wish to study them. Makes sense?

Social networks: there is some fabulous work when we think about clusters. Why is it that firms locate near each other? Why is it that we have a Silicon Valley? Why is it that you have a huge concentration of auto firms near Detroit and so on and so forth. Social networks help us understand some reasons why you may see a bunch of firms that do similar work locating in similar places and that's true internationally as well. Social networks may help explain the division of labor across different types of industries internationally. And there is some fabulous work that is just beginning to take place that looks at emigrant groups and sometimes their relation to the home country, again drawing on social networks. So Ajay Agrawal has a paper looking at the diasporas, the spread of people from India that immigrated to the U.S. He looks at those who work as scientists or engineers in companies and demonstrates that whereas normally geography really matters for the spillovers of knowledge -- normally if I'm sitting in New York and you're sitting in San Francisco the odds of you learning about my work are pretty small -- if you and I are both from India and in particular both from the same part of India then you learn about what I'm doing. Our kinship, somehow, our common background somehow transcends geography and that's a huge, huge fascinating issue for understanding knowledge transfer, for understanding when clusters are not necessary and so on. And related to that, there is some wonderful work that 's looking at how these folks emigrate to, they go the US or whatever, some of them even go to Canada and are successful and start sending money back to India or eventually move back to India to set up companies to work with the American and Canadian firms that they now know. What does that mean to the Indian economy? I don't know, but there's research that is just now being done that is beginning to answer that and this is a fascinating issue. I think we've got a couple of decades of work that can be done making huge contributions trying to understand how these flows of people out and then sometimes back in will affect transitional economies, so there you go. And again, the best of these studies extend the theory as much as they tell us about the phenomenon. [Timekeeper holds up "15-minute warning" sign.] Oh my goodness, I am going to go very fast. I have gone very slowly I am going to speed up now. You've been very kind. Ok, I'll finish briefly here because I want to spend some time on the methods and the other stuff.



In related disciplines, if you look at New Institutional Economics and Positive Political Theory, these are areas of economics and of political science where, thanks to the models and theories they have come up with, we now know a lot more about how and why institutions that are good get established or don't, and how and why bad institutions persist. So if you believe that one of the key issues in international business is to think about how and why countries can adopt better institutions to support growth of business and a better economy, these are crucial ways to try to understand, and these are great theories to bring in to try to understand that particular issue.

On to the methods There are... I 'm going to talk about statistics and very quickly surveys and cases and then we will be off to the races. The big lesson – the one big lesson from this area and in all of methods – is that we have a much better appreciation these days for “endogeneity.” Basically, in the old days there was a lot of work that said for example: “Companies that do this perform better”. The problem with a lot of those studies – and I have one myself like that from a few years ago, before I saw the light – the problem with those studies is: “doing this” is a choice of the company. For example, companies that enter by greenfield rather than acquisition do better, the greenfields do better than acquisitions. Well, the managers got to choose whether they were going to enter by greenfield or by acquisition, so, you have to assume – unless you assume that they were idiots – then you have to assume that they thought about whether greenfield or acquisition would be better and they made the choice that was better for their firm. And so, going out and saying “you should enter greenfield and if you enter by acquisition then you are foolish” is problematic. It is better to think about what led these guys to choose acquisition in the first place. That is the idea of endogeneity: you make a choice based on what's optimal for your firm. Your choice is endogenous to your situation and presumably you are doing what's best.

So what that means is that we need to use methods that help us deal with that. Here is the example: greenfield entries perform better than entries by acquisition, that is a huge literature. At the same time, there is a separate literature that says: “You know what, stronger firms, firms with more internal capabilities, tend to enter by greenfield.” Well, that suggests we better revisit this first issue. Is it that the greenfield entry leads to better performance or is it that the stronger firms enter by greenfield and then happen to also perform better? It is really important to know which of these is what is going on. Well, there are ways to deal with that. There is an



empirical solution. Myles Shaver has been the prime mover in the international business literature and I won't go into the details because you can read the papers and I'm happy to post later the actual cites [J. Myles Shaver, *Management Science*, 1998 and Shaver & Frederick Flyer, *SMJ*, 2000] but the bottom line is: there is a method, it is called two stage least squares. It sounds scary, it is not, most statistical software packages do it for you. That is not an excuse to be ignorant of how it works, you still have to understand what is going on because there are still some important decisions associated with doing it right. But the point is: it is more and more common to see people doing this in their studies and that's great because it means that as an editor, as a reviewer, or as a reader, I have much more confidence in what the results say. What Myles finds is that when you correct for the fact that managers are making choices in the full knowledge of whether they are strong firms or weak firms, the choice of mode doesn't matter. The firms that choose greenfield do better than they would have, if they had chosen acquisition. The firms that choose acquisition do better than if they would have if they had chosen greenfield. And It all comes down to whether the firm was strong or weak to begin with, not the mode issue, so this is a crucial and fabulous advance.

As far as surveys, a very similar thing, you often see folks on a survey wanting to study something like "does 'X' affect performance"? Such as "does trust affect the performance of an international joint venture?" The classic way has been done for decades: you do a survey, and you ask someone "On a scale of one to seven, how much do you trust your partner?" and then you ask the same person "On a scale of 1 to 7, how satisfied are you with the alliance?" The problem is, this is ... I'm sorry, and then you regress satisfaction with the alliance on trust and you know what? Trust always ends up with a positive coefficient that is significant. And then the scholar writes: "Therefore trusting your partner is crucial to having a successful alliance". The problem is: if the alliance has gone well, this respondent is likely to both be happy about the alliance and the partner and so you can't really tell from these results – which are at one point in time from one guy – whether it is the trust that led to the alliance being successful or whether it is the successful alliance that led to me feeling that I trust the other guy. So again there are straightforward ways of dealing with this, and again I can point you to exact studies but: number 1, if you have already done a survey and you have data like this, don't worry, there are diagnostic tests you can do to convince the referees that this isn't a problem for you. Even better, if you are just starting to write a survey you can think about ways to deal with this,



beforehand, like having multiple respondents from different parts from the same company or from both sides of the alliance, or talking to the same person at different times. And this is an example of a paper that has done that very well [the example is Yadong Luo and Seung Ho Park, *Journal of International Business Studies*, 2004] and the more confident we can be that what you are writing about really is accurate. And, once you start doing things like talking to multiple respondents, these guys talk to someone at both sides of the partnership, you can suddenly start asking new research questions that we couldn't think of or couldn't ask before like: "What happens when partners to an alliance have different perceptions of what is going on"? Well, until you interview both sides or survey both sides of the partnership you can't do that, once you do that there is a whole new range of issues where you can make a huge contribution.

On the case studies, all I will mention is that for a while it was very challenging to publish them. There is now a bit more recognition that case studies offer something valuable that complements statistical work. That said, there are a couple of authors I would point you to if you are trying to do case research, who have seem to have come up with a formula for doing it in such a way that they really persuade the reader that what they are finding is true [Nicholas Argyres, for example *Strategic Management Journal* 1996, pp.. 129-150 and *Organization Science* 1999 and 2004; and Kathy Eisenhardt's frequent use of multiple-case methods]. And if there are questions later I can talk more about that, but in the last few minutes let me skip the measures. There's been a lot of work on coming up with improved measures, which is great. The one point I really wanted to make is that it is rarely a contribution just to come up with a new measure that is not related to theory. As long as you've measured something that is closer to the true underlying theoretical construct you're trying to get at, that's great. But just to go out and say well I have a different survey with a different kind of measure is often not perceived as a contribution by the field.

And finally, on the phenomena, where are we going? Again this list, it is growing but it doesn't look that much different from 15 years ago in terms of what the phenomena are. What I think the hot burning questions are right now may be changing a little. There is a bit more emphasis on performance consequences of FDI and alliances and knowledge transfer, particularly with these new methods to try to make sure we get the true answer there. There's always been work on how multinationals are organized, but there is a bit more work these days,



I believe, on how the multinationals interact with local firms and how the multinational might use its subsidiary to try to understand local firm innovation rather than just: “Our subsidiary is there to push out what we do and feed it to the local folks”. There is a lot more understanding that the local folks have a lot that they can tell us: “How can use our subsidiary to try to draw that back and diffuse it through our international firm?” So, that seems to be an area that is coming up.

Now that we know a lot about the importance of clusters and the importance of good institutions, now that there is an agreement on that, the argument is about: “Great, how do you get them? How do you build a cluster where none exists? How do you get good institutions in place where none exists?” And there is more and more interest, I believe, in transitional economies which gets me to the last couple of slides I have which are: “What are the implications of international business research for transitional economies?” and my next slide is going to be: “What are the implications from the transitional economies for research?” But, let me start here: “What can IB research tell us about transitional economies?” Well, a lot of it seems to be about: “How can multinationals from developed economies best be harnessed to benefit the host country?” The IB research is doing a reasonably good job and there is still room for a lot more work here, especially to help us understand the effect of multinationals on local workers, on local competitors: “Is it good or bad to have these multinationals come in?” On local suppliers, on the natural environment, on social issues. How does the multinational affect institutions and of course macro-economy? Another area where I think IB has something to say for transitional economies – and I’ll talk more about this, by the way, these are sort of policy issues that will come up again on Friday – there is some fascinating work about how firms can compensate for the lack of strong institutions, whereby having a large business group for example provides an internal labor market or an internal capital market, or the ability to make credible commitments, when the laws don’t really allow for this or when the financial, the external markets don’t really allow for this [example: Pamkaj Ghemawat and Tarun Khanna, *Journal of Industrial Economics*, 1998]. and finally, there is the classic question: “How might a transitional economy become a fully developed economy?” And the general understanding at this point from the research seems to be: “We can use the research to understand how strong institutions matter and understand the limitations of national policy.” That is, it is awfully hard



to go in and pick winners in this and that industry, but getting the background institutions right seems to be a way to do it, again more on that Friday.

What about the implications of the transitional economies for the IB research? I think that transitional economies can tell IB scholars throughout the world a huge amount. One thing is that the transitional economy experiences help us understand the boundaries of our theories. Transaction cost economics was developed in the U.S., it is based largely on the U.S. background. And so there are some real questions about: “Does this theory really make sense in Brazil, in Japan, in China, in any other country?” And by looking at transitional economies we are able to get some sense as to the degree to which that theory, or any other theory, really applies in this setting as well, and that is crucial because part of the fun of our work is trying to figure out where the theories stop making sense. Because that’s where there is room to start building new theories.

I think that the transitional economy experiences are telling us an awful lot about how important path-dependence is to our theories. And let me stand up here and say that -- I am living in Canada now but I’m from the States originally, so I have a lot of the typical American biases – and so in Eastern Europe after 1990 there was a lot of talk about how do you develop the right institutions for these countries to become developed right away? And the answer was clear to me as it was unfortunately to several folks who went over to try to make it work: the U.S. has the template. Just take the U.S. constitution and the U.S. laws about competition, and contracts, and put them right on top of Russia or Czechoslovakia and everything will be great. I am willing to admit I was wrong on that one. And while that is bad news for the countries that went through that experience, it is good news for scholars in the sense that it really points out how important it is to understand the unique historical experiences of each country and think about how do we find the right path to good institutions given each country’s unique background and context? This is a crucial problem and one that we are grappling with now, and the transitional economies, research in the transition economies is crucial to understanding that. And finally, which variation on what things on what characteristics, which variation matters? When you are a scholar all you really care about is variation – at least, when you are an empirical scholar. I can’t test anything unless there is variation in the data and a wonderful place to get variation is in the transitional economies and so, being selfish as a scholar, thinking about how I can do better research, the transition economies provide a wealth of variation for testing



and refining and building theory. So in sum, just to come back to that first slide I put up, I believe that you have a competitive advantage in performing research on transitional economies. That doesn't mean the only thing you should study is transition economies. No, not at all. But if you choose to do so, then because you have a better understanding of the challenges than I do, because you have a better understanding of the context and the language than I do, because you have better connections to policy makers and managers than I do, you ought to be able to do much better research on transitional economies than I can. You ought to be able to make contributions that I can't. The big challenge is to combine your understanding of these things with the current state of the art in theory and empirical methods and to position the paper as a contribution that is general rather than just "country X is different." At JIBS I get a lot of papers that kind of say: "I'm going to study something in country 'X'. Country 'X' is different from the usual country we look at it and now I'm just going to look at country X" and that's basically it, without really linking it to: "Here is some theory, here is why these aspects of the theory might not apply in this context, and now let me go out and test that rigorously and then at the end come back and talk about what we now know about theory." If you can do that if you can take your advantages and combine them with the theory and the methods, you can make huge contributions to the field and I look forward to seeing lots of them in various journals. This is just a reiteration of the slide I had at the beginning about the best studies, the only difference is I've added this: best of luck choosing your questions and working on the other three important things!