INTERVIEW WITH DANI RODRIK

Dani Rodrik (A.B. in Economics from Harvard College, and MPA and PhD in Economics from Princeton University) is Professor of International Political Economy at the John F. Kennedy School of Government, Harvard University. He visited Colombia last March to participate in a Conference on sustainable policies for development. At the Faculty of Economics of the Universidad de los Andes he presented his work on the role of geography, institutions, and economic integration on long term economic development and agreed to give this interview for Webpondo.

WP: Professor Rodrik, we want to begin by saying that we appreciate your generosity in accepting to give this interview. Actually, it has already been a fruitful experience since, as we commented among ourselves yesterday, it's amazing how much we've learned by reading a number of your papers to prepare the interview.

In a recent paper you have argued that what the world needs right now is less consensus and more experimentation. In fact, you have said that the "laissez-faire" outcome fails, since economic development is a process of self-discovery that requires experimentation and policy intervention. Could you tell us about these ideas and how they help us understand the contrasting experience of development in East Asia and Latin America?

DR: Well, I think what happened in the 1990s is that we grew overconfident in terms of having the recipe for economic growth. And I think the main difference between East Asia in the 60s and 70s and Latin America in the 1990s was that the East Asian strategy was the one that was much more pragmatic and much more based on the actual reality of these countries and the strategies were much more home-grown as compared to Latin America. While there are examples of that as well in Latin America (and I think the sole, the only clear superstar in Latin America, Chile, is an example of a country which very much developed its own strategy, it was very much a home-grown strategy), throughout much of the rest of the region, there was an excessive reliance on a somewhat simplistic set of prescriptions on which Northern academics had converged on. The sentences that you quoted from my writing, are written against that background. Right now we need a lot less consensus and a lot more experimentation, in the sense that we need to liberate policy makers from some of the dogmas that we have developed.

That *doesn't* mean that anything goes. I think that economics and economic science still has a lot to say in terms of providing a frame around this experimentation, and providing a sound of the way of doing it. But this middle ground between ideas that we know are clearly wrong and ideas that are clearly right, is much larger than we have admitted to be, and I think that's the area which needs to be explored. I think without question, East Asian countries would have been far worse off if they had encountered something like the "neoliberal consensus" or the Washington Consensus. China would have been far worse off if it had had no choice but to start its growth process through a structural adjustment loan from the World Bank, as opposed to having the relative luxury of being able to develop it on its own. So that's what I had in mind.

With respect to this same paper[1] we are referring to, there was a point that called our attention about it and it's the fact that you say that the very process of economic development has to do a lot with experimentation and, in that sense, there is some active policy intervention that is necessary. And there are two points with respect to this conclusion which calls our attention. The first one is that you say that there was some kind of more optimal mix in that policy intervention in Asia in terms of "carrots and sticks" (in your terms) and we would like you to explain a bit about that combination. And also, if you are calling for active policy intervention as a consequence of the very process of economic development, what about the imperfections or the potential problems that economists so much worry about in the public sector?

When I talk about experimentation, I have two sorts of experimentation that I have in mind, and I think both are equally important. One, and what you are referring to in the first part of your question, is experimentation in the productive sphere. The other is institutional experimentation. The former is the process of figuring out where your comparative advantage lies, figuring out what you can produce profitably, and this is an activity that's *by and large* undertaken by the private sector. This is not an area for the public sector to be doing. But, what I do think is that because this process of experimentation in the productive sphere (what Ricardo Hausmann, my coauthor, and I call "self-discovery") is a process which is rife with externalities and informational shortcomings, it is also one area where the government potentially has a role to play. And we summarize that role by

[1] Hausmann, Ricardo and Dani Rodik (2002). "Economic Development and Self-Discovery" in: http://ksghome.harvard.edu/~.drodrik.academic.ksg/papers.html

way of this combination of “carrot and stick” policies.

You need the carrot policies so that there is a positive incentive for private entrepreneurs, private investors, to start production and investments in non-traditional activities. That requires a positive inducement. It is not generally going to be undertaken in optimal amounts simply through free market forces, because this is a process which provides tremendous social externalities. The first investor in Colombia that discovered that cut flowers could be profitably exported to the United States, created tremendous social value. And, in fact, this innovation very rapidly dissipated itself to Colombia with many, many more entrepreneurs coming and starting to produce. Economic development is fundamentally a process of this kind where at least early on you need sufficient incentives in place for this investment in *new* activities to take place. That’s the carrot part of the policy.

I think the stick part, and I think that’s closely related to the doubts you were raising about the possibilities of useful intervention, the stick part of the policy has to do with ensuring that such incentive policies do not deteriorate into effectively just protecting long term incumbents. Particularly those that end up having done wrong experiments or having ended producing not high-productivity low-cost activities, but activities that you can prop up only through protection or continued subsidies. So it does require certain amount of capacity on the part of the government to weed out the losers; a certain capacity on the part of the government to separate out, ex-post, the winners from the losers.

I think generally economists have said that governments cannot do this. I think when economists say that governments cannot do this, they are really, for the most part, really doing amateur political science. Because there is really very little systematic analysis of when and how, or if at all, governments have the capacity to do interventions of this sort. I think it’s clear that this is not something you can recommend across the board. Typically, you have to look for parts of the government where there is bureaucratic competence, where there is professional expertise with certain amount of autonomy. And I think, where you have those, programs like these can be undertaken. It will never look the same way from one country to another. In some country it might be a public private venture fund; in another country it might be an export processing zone; in a third country it might be tax incentives or investments in new areas. Particularly, this will depend a lot on where the capacity in the system, in the public sector, is really located. But I think it’s just empirically not true that governments cannot do this, or that any attempt to do this is necessarily doomed to failure.

And I should emphasize one more thing: often people react to such ideas by saying, "the government can never pick winners". The argument is not that the government has the capacity to pick winners; it is a much weaker argument that says, "the government does not have the capacity to pick winners, often it will pick losers", but, what we need to do is design institutions that at least give the government the capacity to let go of the losers. That's a much less demanding requirement on the system than simply presuming that the government can pick winners, because it allows that the government will make mistakes. In fact, from this perspective, making zero mistakes is surely suboptimal.

Coming back to the "carrots" and intellectual property rights incentives, what do you think about this WTO agenda of intellectual property rights that developed countries want to impose on developing countries –this "TRIPS" stuff[2]?

I think TRIPS was a terrible idea. I think developing countries knew it was a terrible idea, but two things happened: one is that they did not appreciate how terrible an idea it was; and, second, that they thought by agreeing to TRIPS they would be getting substantial amounts of liberalization and market access in return, which, by and large, I think has not happened. So I think bringing TRIPS into the WTO framework was just a lousy idea, and I think most economists will agree to that. I think this was largely driven by the interest of pharmaceutical companies in the United States, and I wish we had never come down that path.

In that particular, what's your position in terms of the compliance with, for example, intellectual property rights by developing countries? There appears to be an analogy between your notion of "self-discovery" and traditional arguments for intellectual property rights protection...

Well, I think a lot of transfer of new technology to developing countries, historically, has happened through a process of reverse engineering. Taiwan and South Korea benefited tremendous amounts

[2] TRIPS refers to the Agreement on Trade-Related aspects of Intellectual Property Rights signed in Marrakesh, Morocco on 15 April 1994. The text of the agreement is available online at:

from reverse engineering, things that they would not have been able to do if WTO rules applied to them at the time. Or, I should rephrase that, if WTO rules of post 1995 applied at the time. So, it doesn't mean that developing countries ought not have patent rules, or should not have IPR protection. It just says that this is an area where it would have been very valuable for developing countries to maintain autonomy to figure out what would be the best system for that. This is with regard to industrial patents and there is of course a whole question having to do with public health and patents on medicines. That of course is an ongoing issue, and I think that this is again an area where developing countries gave way too much and now they're trying to negotiate a retreat with the United States which has been very adamant.

We would like to move to another area where you have also argued that it is necessary to do experimentation or more precisely to be aware of context specificity and that's the point of institutions. In particular, you have pointed out that the Russian, Latin American, and Asian crisis have taught economists that institutions are important, that incentives are important for markets to work, but you have stated that the Augmented Washington Consensus that tries to take account of this issue, is "infeasible, inappropriate and irrelevant". We would like you to explain why.

I think what has happened with the so-called Second Generation Reforms or what I call the Augmented Washington Consensus or what some people call the Washington Consensus Plus approach, is that it's gone from a good starting point into a rather unhelpful and irrelevant agenda. Let me explain.

When the original Washington Consensus was first enumerated by John Williamson, almost all of the items on the list were relatively simple policy measures; they didn't have a very strong institutional background. Things like price liberalization, opening up to trade, having a realistic real exchange rate, eliminating financial repression, none of these really required institutional investments. They really did not recognize that what was really important was the institutional underpinnings. However, number ten on the agenda, was "Secure Property Rights". And the history of this is interesting because, actually, that was an after-thought for John Williamson. When he had first listed all the things he had then on the agenda, he came up with nine. Of course, that was not a round number, so he said: "I better come up with a tenth item!" and the tenth item, number ten in

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http://www.wto.org/english/tratop_e/trips_e/t_agm0_e.htm.

the list, was Secure Property Rights! Secure Property Rights is a clear institutional recommendation; that's the foundation, many people would say, of long-term prosperity.

And, so the good idea that the second generation reforms or the Augmented Washington Consensus takes is basically to say that the heart of it is really institutions. That long-term development requires good institutions. In a way therefore, the objective was to take that last category, the tenth category, Secure Property Rights, and then turn it into something that was going to be more operational.

The reason that I think this whole discussion on the importance of institutions ended up taking a wrong turn is that the premise of a lot of the institutional recommendations ended up being that we had a fairly good idea of what form and shape good institutions take, and that we could be fairly certain that all countries ought to have these types of institutions. And therefore, we started listing, you know, various reforms such as corporate governance along Anglo-American lines, deep financial liberalization, international financial codes and standards, labor market flexibility, WTO agreements which are now heavily institutional in their demands such as TRIPS, and are getting even more so. So, what we did was essentially take a good idea which is the importance of institutions and we turned it into a bad idea by attaching, listing, a whole series of very specific institutional recommendations that developing countries ought to follow.

And the trouble with this is that we ended up generating institutional blueprints, which are largely untested and which are administratively very costly, without having a very good sense of whether in fact all countries ought to converge necessarily to the same type of institutions. Because if you look at the range of variation in institutional arrangements in today's advanced countries, there is a tremendous amount of variation. Compare, you know, European countries with Japan, with the United States. All these countries have property rights, they're all market-based systems, they all have, you know, some monetary and fiscal systems, but when we get into the details in what the regulatory regimes look like, what social welfare state arrangements are, what their labor markets look like, what the corporate governance regimes look like, these have been very different. So that the message there is that you can actually end up being wealthy, and end up with institution arrangements that could be very, very different. I think that message was forgotten.

The other reason why this whole approach ended up becoming irrelevant, was that it became sort of a whole new checklist of things to do, a rather undifferentiated set of things that clearly didn't provide a very good sense of priorities. I think what we confused was all the good things you need to have in order to become successful in the long run with the important things you need to do in the short run in order to ignite growth. And what countries such as Colombia right now need is thinking in terms of growth strategies that will in the short run be able to ignite private investment and private economic activity. And it's just not very helpful to say that you need this long list of institutional requirements, even if they are what Colombia needs for the very long run. This is such a demanding agenda that is not particularly helpful.

If institutions are context specific a major puzzle that remains is the enthusiasm that so many countries, in particular Latin American countries have had with the Washington Consensus reform agenda. You have advanced an interesting hypothesis with professor Mukand[3] in this direction. Please tell us about it.

I think you're identifying an interesting point, which still leaves me puzzled to some extent. But there are two elements here at least. One is the powerful effect of a herd phenomenon which is that for political leaders it's much safer to be wrong in the conventional way than to end up being wrong in the unconventional way. And here I must also say that to the extent that political leaders have looked to economists, we have not been able to articulate a sufficiently compelling and sufficiently realistic vision of how things could be done differently. And maybe it wasn't the job of economists to do that. But nonetheless, the alternative, the compelling and realistic alternative hasn't been so concrete as to be an offer to substantially alter this cost-benefit calculus as to whether you'd rather go down in history as having failed, but at least having done what looked like the right thing to do at the time, or as having failed for something which surely you will be interpreted as having been misguided from the beginning. So, in a way, part of the explanation is that there is this tremendous pressure to conform, even if you don't believe in the underlying model to begin with. I think this has played a role.

A second phenomenon which I think has played a role somewhat in some countries (although probably not in Colombia, and it might explain some of the Colombian peculiarities) is that many of the countries in Latin America that ledged on the most enthusiastically to this agenda were new

[3]Mukand, Sharun and Dani Rodrik (2002). "In Search of the Holy Grail: Policy Convergence, Experimentation and Economic Performance" in http://ksghome.harvard.edu/~.drodrik.academic.ksg/papers.html.

democracies. I think for new democratically elected leaders, it was particularly important to signal trustworthiness to their electorates. And it's much easier to do that when you are doing what Washington and the World Bank and the IMF seem to be telling you, because this is at least one way you can signal that you're not corrupt. That the policies you followed had the blessing of, you know, technocrats and therefore, at least you can identify yourself as a politician that is not corrupt, whereas any politician in a new democracy without a track record and so forth, who comes and starts to do, let's say, East Asian style industrial policies, would immediately have been branded as being corrupt and doing it not for the right reasons, but for the wrong reasons. So I think the transition to democracy has also increased somehow the premium on convergence on these policies.

Then we have a problem: we need experimentation, but experimentation is politically costly. What can we do?

I think that is correct. I think fundamentally you need two types of things. You need a certain set of programmatic elements, certain new ideas about how you can do things. That's a technocratic job, in the sense of having a better idea of the kinds of policies that work. And, secondly, you need more self-confident political leadership, and a self-confident political leadership requires a political leadership that's going to have a strong political and social base. And I think it's not impossible. I don't think necessarily you need future disaster before this can be accomplished although traditionally of course big breaks in policy paradigms appear only after big, you know, big crises. In fact, of course, Latin America's own movement in this direction was direct consequence of the debt crisis.

So, I don't have a good answer as to what's going to take us out of here. Frankly, I'm still trying to figure it out.

We'll move to some other questions on globalization, which are perhaps some *must* questions, after your book and everything[4]. But we want you to be brief in these questions. What is your view in the costs and benefits of globalization? "No pain no gain"? And, if so, how can we minimize the pain and maximize the gain? Finally, what is your opinion of Professor Stiglitz's diagnostic on globalization in his recent book?[5]

[4] Rodrik, Dani (1997), *Has Globalization Gone Too Far?*, Institute for International Economics, Washington, DC.
[5] Stiglitz, Joseph (2002). Globalization and its Discontents. New York: Norton.

Well, I mean, it's very hard to discuss this at this broad level of "globalization". Anytime somebody says globalization they have something in mind, but it may not be, you know, what the respondents have in mind. So I don't feel very comfortable discussing this issue at the level of "globalization". I sometimes say that the best thing that could happen to the debate on globalization is if we stopped using that term. Because then we could talk and be specific about the things. If you're upset about trade liberalization, let's talk about that; if you're upset about capital flows and short-term volatility of capital flows, let's talk about that; if you're upset about McDonalds and Nike around the world, let's talk about those. So, I find it's very hard to make sense of either the benefits of something like "globalization" or to discuss somebody else's views on it without sort of unpacking this question.

So, this is an evasive answer.

Okay. We'll accept it.

In the seminar you just gave you argued that "institutions rule" in the sense of being the most important or the key determinant of long run economic growth. What about the role of geography in integration in the making of the modern world income distribution? In particular, what would be your answer to Professor Sachs' "institutions don't rule" recent paper[6]?

Well, I think this is an empirical question. I think it's a question that has to be discussed on the basis of the evidence. Jeff Sachs feels very strongly that geography plays a very important role. He doesn't opt deny that institutions are very important as well. What he argues in this new paper that you just mentioned, is that it's not that institutions aren't important, it's just that institutions are important, but that geography is also systematically important. In particular, a new variable that he has, which relates to incidence of malaria, is very important. Now, we haven't had a chance to look at that data, and in our own paper we don't necessarily say that geography doesn't matter, it's just that its systematic effect does not seem to be as strong as the effect that we identified from institutional quality.

[6] Rodrik, Dani, Arvind Subramanian and Francesco Trebbi (2002). "Institutions Rule: The Primacy of Institutions over Geography and Integration in Economic Development." NBER Working Paper 9305.

Now, I don't think that it's going to make a tremendous amount of difference in the sense that there are very few things we can do about geography anyhow. If you were to discover that geography does matter and matters more than in our own paper, I don't think the policy implications would matter a whole lot.

If institutions are the most important determinant for long term-economic well-being, and we said we need experimentation, we need specificity, and there are no blueprints, we just get naturally to, you know, concentrating in particular case studies. And this is the topic of your forthcoming book "Analytical Country Studies in Economic Growth". What are the main lessons one can draw from this volume?

First, before I talk about the volume, I think it's important to be clear that the idea that we need experimentation and that good institutions are going to have a lot of context specificity, *does not* mean that anything goes. I think we still have ways of thinking in disciplined and systematic ways about what type of institutions, and for what purpose, and how to design them, and that's an area where good economic analysis can still make a lot of difference. I want to be very clear that what I'm saying is not that basically anything goes. We need to think about institutions systematically, we need to think about the incentive structure that institutions generate, we need to think about institutions in the various different domains that I list in my work, and we need to do a better job of mapping desirable institutions to initial structural conditions and political economy. And that's really an area that could be of very productive research. We may ultimately be able to generate contingent generalizations about what type of institutions under what circumstances are most likely to perform best. And we have done very little of this work. There are areas, such as regulatory institutions for telecoms, where some of this work has been done, and there sure is a lot to be learned from this. So I just want to be clear that we're not throwing out disciplined systematic thinking on these issues.

What the book that you mention does is basically go through a series of case studies, or country studies, and looking at each one of these countries (India, Botswana, Indonesia, Mauritius, Mexico, Venezuela, China) from the perspective of modern growth theory. It's essentially taking modern growth theory to each one of these countries and then distilling a number of lessons which then we can address back to the theory. I think some of the main findings from these country studies, which were done by a range of specialists in economics, in growth economics, and political economy, were quite interesting.

I think, once again, the importance of institutions was one of the key themes in the papers. One of the things that came out very strongly is how little sometimes it takes for countries to suddenly experience a rapid growth spurt. I think that's probably the case that highlighted this most was the case of India, which experienced a doubling of its growth rate in the early to mid 1980s. And this was done through relatively minor changes in the policy environment. Similarly, the tremendous increase in growth rate in Mauritius or in China can be attributed to relatively small changes in policies or small changes in institutional arrangements. I think that was one hopeful message that comes out of the book, which is that these transitions to higher growth do not require a long checklist of things that you have to do. That you can get these very significant growth boosts if you can identify the binding constraints well, from well designed but relatively minor interventions.

But, the more pessimistic message was that there was very little in common across these policy changes. What was common was the importance of finding ways of increasing private economic activity, or private investments. These were very much productivist strategies, but the key elements, the key policy elements in each one of these, were very, very different. In Mauritius, it took the form of an export processing zone; in China, it was the introduction of household responsibility system and a two track price regime; in India, it was simply a change in the government's attitude from being extremely hostile to private entrepreneurship to being supportive, with very little by way of changing the underlying institutional policy arrangements. This is the more depressing part, which says that basically a lot of the hard work has to be done on the ground, and it's very difficult to design growth strategies in the abstract.

A third set of issues examined in the book had to do with growth collapses. These are countries like Venezuela, or Indonesia, which experienced growth over a period of decades, which were quite significant and where all of a sudden the economy, typically under the influence of some external shock, collapses, and then is unable to reignite growth for a while. I think the key thing here is the importance of using high growth periods for building high quality institutions that will provide these economies with resilience to shocks. What happened certainly to Indonesia was the total absence of the good institutional underpinnings that made the economy very susceptible to an adverse shock, and compared to Korea which did build good institutions in this period, Indonesia was unable to recover very quickly. So, these growth collapses illustrate the importance of using periods of high growth for reinvigorating and strengthening the institutional base of markets.

Sometimes you speak of democracy as a "meta institution". In a couple of papers you've actually shown that democracy is associated with significant lower levels of aggregate economic instabilities. Why do democracies tend to perform better economically than authoritarian regimes?

This is an interesting question and I think there are a number of hypotheses. I have not, actually, empirically tested for any one of these specific channels, but there are a number of different stories one can imagine.

One is that democracy is a system of planned and expected alternation in power. And in a system such as that, politicians have an incentive to cooperate in a long term way and avoid extreme policies. So that if you and I represent different groups, any time I'm in power, I may have the incentive to expropriate you. But I know that I may not be in power forever. In fact, there's a good likelihood in democracy that some time down the line you will be in power, and that you have the incentive maybe to expropriate me. In situations where there is sufficient alternation in power, then you can have an equilibrium where moderation in policies is the equilibrium strategy for all parties, rather than extreme vacillation from one policy to another, aimed at benefiting one's own constituencies and hurting the others. This is the kind of story that Avinash Dixit, Gene Grossman and Faruk Gul have developed in a paper, and I think that would be one story for why you get much greater stability under democracy.

A second story is that democracies tend to be much better at handling adverse shocks, precisely because democracies have institutions where stake holders have a bargaining table around which they can fashion-out compromises. Anytime an economy is hit with an adverse shock, the issue is what will the distribution of this reduced pie look like. If you have an institutional setting (it could be parliament, it could be social partnership agreements) where different parties have a forum where they can discuss this burden sharing, you're much more likely to get a result than when you don't have a forum, where the only way that groups can exert their claims is by rioting and going on the streets, and over-turning, and burning cars, and so forth. This is exactly the contrast between South Korea and Indonesia in 1997-1998, where the response of Korean democracy is basically to get businessmen and workers and unions around the table, and to agree on a framework, whereas the response in Indonesia is for people to go on the streets and riot, because there is no mechanism through which they can make their voices heard and their demands heard other than through violence. Those I think would be two possible explanations for this relationship between democracy and lower instability.

We have a very brief question and we want to know your opinion.

Yes or No question?

No, no, no. We want to know your opinion. We've read most of your papers that have formal models for political economy, usually written with co-authors. Sometimes we have the feeling that the paper, or the hypotheses, or the conclusions, or the empirical testing of the hypotheses, they do not need the model per se. What's your opinion of using the political models in political economy?

Well, you know, I think models, mathematical modeling is primarily a tool to ensure that your conclusions fall off from your premises. Now, it sounds boring, but the process is tremendously helpful because it forces you to a) articulate your premises, and b) make sure that you can go from your premises to your conclusions and see after you've done all of that, to lay there any silly things that are embedded in the argument, even if it is internally coherent. I think that's a very, very useful discipline and when I have done formal models in my papers, it has always been to straighten up my own thinking, and it's rarely the case that a model comes out in exactly the way that I thought it would. A model always teaches me something because it either reveals an incompleteness in my logic before I try to write it down or, as it often happens, it reveals an unexpected result that I had not thought about before. So, I think those are very useful.

Now, I am a fan of simple models. I'm not a fan of an approach that writes papers by saying: "such and such did such a model, I think this particular part of the model is not very good, I'm going to fix it". I'm a fan of modeling that's driven by real world puzzles, and then generates particular explanations, and then tries to articulate them in an internally coherent and systematic way. I don't want to defend all kinds of modeling, but I think modeling is important.

The final thing I want to say about this is that it's often misunderstood why we use mathematics and mathematical modeling: it's not because we're smart, it's because we're not smart enough. Because if we were smart enough, we would figure out whether the argument was complete and coherent and internally consistent, and what else it implied. It's precisely because we cannot deal with that without putting it all down in an equation, that we do it.

That's a really reassuring "how do you work question". We're going to finish now, with very brief questions. Not yes or no questions, but almost. Two words.
The first one is: if you could decide the next Nobel Prize in Economics, who would that be?

That's a hard one.

Yes, but it's two words.

We know it's a tough question…

You probably don't want to hurt any feelings.

It's not… it's really not that. Let's skip that, I really don't have a good sense of that, I don't have any…

What about political economy? If there was a political economy Nobel Prize, wouldn't you think about anyone?

I think the work that Daron Acemoglu and Jim Robinson and Simon Johnson have been doing is extremely important. I think the work that Alberto Alesina has done is very important. I think Tim Besley is doing very, very nice work in political economy. Guido Tabellini and Torsten Persson have done very nice work.

Now, has political economy as a field contributed so much to economics that it deserves its own Nobel Prize winner? I don't know. I wouldn't try to make a case for it. In fact, I think what's interesting is that, as I think of it, I think there is less interest in political economy these days. The most interesting work that I'm reading these days, such as the Acemoglu-Robinson-Johnson series of papers, forthcoming books, and so forth, is not really cast in a political economy framework. It's more cast, I guess some of it is political economy, but it's cast more in the growth tradition, in the macro tradition, sometimes in the labor market tradition. But I think the direction in which political economy literature looked like it was going at some point in terms of unpacking the black box of how public policy decisions are made, in terms of being very explicit about the processes of influence, and lobbying, and so forth, it sort of hit a dead end. I think the interesting work in institutions is not going down that path. It's dealing with bigger issues, it's not explicitly political economy, in the sense of unpacking the nature of political institutions. In that sense, actually political economy is not as strong a field in which people are gravitating into as it was perhaps five or six years ago.

Name your favorite economics book, if you can choose one.

My favorite economics book…You know, the thing is my brain is so fried that I can't think of any book that I've read right now.

(Laughing) These were the really tough questions…

I think it would be…my favorite…I would say probably Tom Schelling's book, *Micro-Motives and Macro-Behavior*. That probably would be.

We will read it, that's basically the aim of this question…

Yes, you should, it's an excellent book.

Who has been particularly influential in your career?

I think some of the big thinkers in development early on have been important in terms of influencing my own thinking. Carlos Díaz-Alejandro was and still remains one of my intellectual gurus. I think he was an extraordinarily insightful economist and just a wonderful writer, which is a very, very rare combination. And of course I was influenced by others who were the big thinkers in development, Albert Hirschman, Sir Arthur Lewis...

A person who played a very important role in the way that I do economics is Avinash Dixit. What he and I do these days is very different, but he was my main dissertation advisor and he was always a model of clarity for me in terms that I've never seen anybody who's a clearer thinker than him, so he's always been a model for me. There's never been a paper that I've written that I haven't thought what will Dixit think about this. So he has been a very strong influence.

At Princeton I benefited a lot also from Gene Grossman and also from Bill Branson. Peter Kenen was actually extremely important because I believe he was single-handedly responsible for my admission to the PhD program at Princeton. I believe my math skills were viewed as inadequate for admission to the PhD program at Princeton and it was Peter Kenen from whom I had taken a course as a Master student before, who prevailed on the Admissions Committee.

Another economist who was very important to my career was Raymond Vernon, who was probably best known as an analyst of multinational enterprises, but he was probably one of the earliest and most impressive analysts of globalization before that term gained any currency. I was his undergraduate research assistant when I was at Harvard and he used to teach at the Harvard Business School, and then for some reason that I never quite fathomed he took some liking to me and thought that I had some promise. He actually helped me a lot and he was quite important in my first job, because by the time I received my PhD, he had joined the faculty at the Kennedy School. He was I think instrumental in my getting hired as an assistant professor at the Kennedy School, and he and I taught a course together for a couple of years. And it was the most terrifying thing I've done, because he still scared me.

So, those were some of the key people who played a very important role in my early career.

Okay, thank you very very much for your time.