

THE BROOKINGS INSTITUTION

FALK AUDITORIUM

WHAT WORKS IN DEVELOPMENT?

THINKING BIG AND THINKING SMALL

Washington, DC

Thursday, January 21, 2010

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P R O C E E D I N G S

MR. KUMAR: Welcome, everybody. I'm happy to see such a great turn out for a great book. I think you will be pleasantly surprised by the conversation we're going to have today and how interesting it's going to be.

This is an interesting topic. I just came from giving a talk to a bunch of students who are interested in considering a career in development. And at the end of a long session one of them said, well, how do we actually know this is going to have an impact in developing countries? I said, well, that is kind of the fundamental question, isn't it? And that's what this book really gets at.

And it's a book which focuses on two approaches to understanding whether or not aid has an impact. On the one hand, you've got sort of the big picture which we're going to be hearing a bit about from Bill Easterly. And the big picture includes the traditional macroeconomic analyses cross-country studies which try to take a whole range of factors and figure out among them, well, if we tweak this one or tweak that one, what drives growth? And that area of study is a bit in crisis. This is what the book talks about, that it's hard to really know what works.

So, there's another school of thought which has gained prominence in recent years and Jessica Cohen will be talking about that school of thinking predominantly, which is the micro approach. Essentially it takes a lot from the medical field, from randomized clinical trials -- RCTs, you'll be hearing a lot about that today -- starting at the small scale instead of the big cross-country study, looking at an individual village and saying, you know, if we insert bed nets, what does it do? And if we can find out what actually works, if we can get real evidence of works there, maybe we can extrapolate that and look at it at a larger scale and find interventions that we're sure will actually work.

So, this really gets to the heart of what all of us who are in the aid industry

are doing. Now the question is, what kind of impact are we having? And if we can't measure it, if we don't know what our interventions are doing, then what's the point? It gets back to the student's question that I just got earlier.

So, I'm just going to give you a brief introduction to these two panelists because I think you're probably very aware of their work, but Dr. Cohen is assistant professor of global health at Harvard School of Public Health. She's also a nonresident fellow here at Brookings and malaria research manager with the Clinton Foundation. She co-founded a nonprofit organization in East Africa that is working in the malaria field. It's called TAMTAM, Together Against Malaria. And interestingly, she's actually working on these field trials, these RCTs that I mentioned recently, in Africa, so we'll be hearing more from her on that.

And Bill Easterly, who is well known to all of you as a professor of economics at NYU, he's also the editor of the Aid Watch blog, which I can recommend to all of you. If you haven't read it or are not a regular visitor, check it out. I think the subtitle is "Just Asking That Aid Benefit the Poor." Is that right?

MR. EASTERLY: Yes.

MR. KUMAR: Which is a lot of what we're going to be talking about today. He's also the author of, obviously, the well-known book, "The White Man's Burden: How the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good," 2006.

So, let me just get started. I'll just remind you before I do to turn off or silence whatever electronic devices you may have brought with you unless you're blogging for Bill, and we'll get started.

I'm just going to ask Bill at the beginning here to tell us a little bit more about this crisis in the big picture. You know, is it really the case that we don't know what works because these macro studies can't show us with any conclusive degree of evidence

that certain interventions do work? And if that's the case, should they be dispensed with entirely?

In the book there's an interesting phrase given to people who support the random clinical trial agenda called "randomistas," which, I guess, I take it to be people who are ideologically in favor of these randomized clinical trials and against the traditional big picture approach. Are they right? Are they right to sort of dispense with the macro approach to understanding what aid does?

MR. EASTERLY: Okay. Thanks. Thanks, Raj. Good afternoon to all of you. It's really great to see a lot of old friends in the crowd as usual.

I feel a bit handicapped because I almost always give talks with PowerPoint and I'm kind of out of my comfort zone just sitting up here naked without PowerPoint. On the other hand, I did do one experiment recently on the Aid Watch blog where to celebrate Martin Luther King Day we did a little experiment of how you would translate the "I have a dream" speech into PowerPoint. You might want to check it out. The title of it was "Mainstreaming Inter-Ethnic Inclusiveness," by Martin Luther King, Jr. So if you don't have any other good reasons to go to Aid Watch blog, then check that one out.

So, yeah, let's get serious. There is a big crisis in thinking big about development. And actually we have a quote that David Brooks picked up on in the New York Times that is right in this book, which, as it happens, was by one of the micro economists contributing to the book, Abhijit Banerjee. He says -- let me read you his exact words. "It is not clear that the best way to get growth is to do growth policy of any form. Perhaps making growth happen is ultimately beyond our control. Maybe all that happens is something goes right for once, like, say, privatized agriculture raises incomes in rural China and then that sparks somewhere else in the economy and so on, and perhaps we will never learn where it will start or what will make it continue."

Now, I'll be honest with you, when I first read this I was actually a discussant at the conference of Abhijit's paper. And when I first read that statement I thought he was just expressing the usual racist attitudes that micro economists have towards this other racial group that's known as macro economists. There's a lot of interracial hostility, talking about Martin Luther King, between these two, between macro economists and micro economists. But actually macro economists -- a lot of macro economists would pretty much say the same thing about what we know about how to achieve growth in the short to medium run, that we really don't know. I think the only thing Abhijit went wrong on is he really didn't give us enough credit for really just how hard we've had to work to get to the point where we know absolutely nothing. We've really had to do a lot of work to get to this point and, you know, I wish he'd given us more credit for how hard it is to be ignorant.

So, you know, the main thing we tried was we exhaustively did these cross-country econometric regressions of growth rates on every possible factor under the sun from, you know, coups and assassinations per capita, to ethnic diversity, to the interaction between ethnic diversity and assassinations per capita -- I was never quite clear what the rationale for that one was, but it was in one of the -- it was in an American Economics Review paper on aid that was very famous. And what we can say now is that this attempt to find the -- you know, the determinants of growth has failed so decisively, so comprehensively, and I can -- you know, I'm happy to supply more details later or check out the blog -- it's failed so comprehensively that anyone today who makes any policy recommendation that is based on the growth regression has zero credibility. And I hope that is one takeaway that you all get.

By the way, I should alert you a little bit. The first part of this brief chat that I'm giving you now, the first part is going to be all doom and gloom and pessimism and hopelessness and despair, and then I'm going to shift gears and give you more happy,

positive messages in the second five minutes.

So, why did growth regressions fail? Well, there are lots of reasons. One of them is just sheer data mining which is, you could just keep running regressions until you find some net results that look nice, and that's not really respectable, and that's one problem that randomized -- RCT-based methods really solve. They really don't suffer -- they suffer -- they could suffer a little bit, but, by and large, they are free from data mining. And that was one big problem.

But I think there's more fundamental kind of conceptual problem with growth regressions that Danny Roderick points out very forcefully in his volume "What Works in Development." He says, you know, why should we expect a policy to have the same effect in every country? I mean, this is -- why should we expect -- let's take the policy of promoting education in some way. Some way you promote education so that people are better educated and you're graduating more, you're producing more skilled workers every year. That sounds like it's good for development. But, of course, the effect of that policy is very different in an economy that has a very hostile business environment which is not creating jobs for skilled workers. Then that policy is going to be pretty useless, and the policy will work very well in an economy that has a very friendly business environment that is creating a lot of jobs for skilled workers.

So, this is so obvious and yet somehow we sort of missed this, that, you know, this is a big reason why growth regressions didn't work. We were expecting to uncover sort of stable relationships between something like education policy and growth, and there is no such stable relationship because the effect is really different in every country depending on the context.

Now, by way of macro economists commenting on RCTs, what Jessica's going to talk about, the randomized trials, unfortunately, that's also the main criticism of the

randomized trials, that, you know, why should we expect the effect of bed nets on malaria to be the same when you give out the bed nets in a community that knows a lot about the health science of malaria and another community that has not really absorbed the belief that mosquitoes cause malaria. So, in those two different communities you obviously expect radically different effects, and there are lots of other things that would cause the effect of bed nets to be different in every community. So, this is really the main criticism that macro economists have and that we had in this book, of the randomized trials. They suffer from the same problem that we had as macro economists when we were trying to think big.

And I'll leave it to Jessica to have a really compelling rejoinder to that criticism. So, I'm not going to try to be any more balanced than that at the moment.

So, you know, second, macro economists, we reached our current level of sublime ignorance by just observing the failure of various attempts that we could call sort of social engineering, large-scale engineering attempts and development, you know, the ambition to transform a whole society through some outside intervention. I think the first one was sort of the aid financed big push to try to increase investment in everything at once and promote growth and that we really, you know, just at least in a sense of sort of studying a lot of case studies and what are plausible counterfactuals, it's pretty clear that the aid big push failed to deliver the high growth it had promised to deliver.

And then second, you know, another similar type attempt was sort of the imposing the Washington consensus through structural adjustment on Latin America and Africa and the Middle East in the 1980s and '90s. That is also widely agreed among macro economists to be a failure.

Another one that is well know is sort of the failure of the ambitious hopes of shock therapy in the former Soviet Union, that it would -- we would immediately be able to jump from communism to capitalism if you just followed all the advice of the brilliant

economists that were pouring into the former Soviet Union in 1991, '92, which I was one of them, if you just followed our advice, you know, you would immediately have a very happy outcome. Instead you had a 50 percent, 70 percent decline in output before we have now started to recover.

So, I think we have learned as macro economists that we have to be extremely cautious before we recommend anymore kind of large-scale engineering schemes on hapless developing countries that may have to do these things as a condition of getting aid or World Bank loans or whatever. It's just not a good idea I think we've learned at this point.

Now, unfortunately, there is some possibility that randomized trials will themselves become another large-scale experiment that we try on developing countries. If you stick to randomized trials as something where you're thinking small, just whether one little thing works, that's fine, but a lot of the randomistas, as Raj has called them, you know, have said some pretty ambitious things about how we should, you know, guide all of social -- we should revolutionize all of social policy using randomized trials. That then becomes, you know, something that started off as a "thinking small" idea has become a "thinking big" idea. It's become the big idea that we should do a lot of randomized trials to revolutionize social policy.

To pick on poor Abhijit again he said, "Social policy may be the best thing we can do for growth to happen and micro evidence, randomized trials, on how to do it well, may turn out to be the key to growth success." So, this was just after he made that devastating statement about macro economists knowing nothing. So, you know, I have to hold him to the same standards of skepticism that he was applying to big picture growth economics. You know, if you're so skeptical of the first, why are you not skeptical of the second?

We really -- and other randomistas have made even more ambitious statements than the one I just read. He at least said "may." Other people who I really respect and admire a lot like Esther Duflo and Michael Kramer, they've made even more, stronger statements without the "may." And you know, I admire what they've done in this methodology, but I'm a little nervous that it's becoming, itself, a big idea. That, you know, we're going to completely redo all of development policy using randomized trials. And this is really an untried experiment. It's a large-scale, untried, social engineering experiment and I've told you about all the failures of other social engineering schemes, and so, you know, I really think that's a big concern about how sort of enthusiastically are we going to embrace randomized trials? How big of a panacea do you really think they are? Are you really going to try to do everything?

And we certainly -- and this is pretty much an unprecedented experiment, also. It's not that we really have any record of any rich country that has done its social policy or development policy based on randomized trials. Randomized trials have been around for 30 years in the U.S., but have had very little influence on any U.S. social or economic policies. There are a few examples here and there, but overall policy is not being made on the basis of randomized trial. So we really have no precedents for such an experiment.

So, this is one big take away that I want to leave with you, that you can do - - you know, with randomized trials, they're so great that you can test almost anything. Randomized trials can test almost anything except whether you should use randomized trials. That sounds kind of paradoxical so I better repeat it again. Randomized trials can test -- you know, almost anything that you want to test can be tested with a randomized trial except whether you should do randomized trials. So, we cannot test with a randomized trial whether we should have a large-scale program of doing everything by randomized trials. And I'm exaggerating when I say "everything." I don't think Jessica or anyone else would

say everything, but they are saying, you know, let's do a lot, let's have a lot of stuff guided by randomized trials, and that worries me as yet another social engineering experiment.

Now I'm ready to switch to something positive, but I'm already running out of time, unfortunately, just when I'm trying to be positive. Let me give you some positive messages to counteract this doom and gloom that neither macro nor micro economists know very much and we shouldn't try anything and we should just go home and hide in a closet and not recommend anything ever again. Well, now, okay, first let's have a reality check. Just because growth regressions failed, we did not suddenly unlearn all of Economics 101. We did not unlearn the whole field of economics just because growth regressions failed.

Now, we have a good reason why they failed. We still have, you know, the whole field of economics, which is, you know, an incredible accumulation of theory, common sense, and empirical evidence that is extremely useful in making all kinds of policy decisions. And there's lots of specialized expertise within economics in particular fields that is incredibly useful, including some of which comes from randomized trials by micro economists. So, you know, I'm not trying to throw out all of economics. It's obvious that economics is a powerful set of tools and a powerful set of principles that's still available to us. It didn't disappear just because growth regressions failed.

Second, there's just plain old common sense. With the current Haiti earthquake crisis, on the Aid Watch blog one of our -- what turned out to be the second most popular post that we ever had of all time -- and the first most popular post was not really serious, it was kind of a joke on -- about Bono. The second most popular post we ever had was just a couple of days ago written by someone probably most of you have not heard of, but just a veteran field worker in humanitarian emergencies, and the title of it was, "Nobody Wants Your Old Shoes." Her point was, you know, the best way to help Haiti is not to send

kind of big packed boxes full of old shoes. You know, they're very expensive to transport. They clog up Haiti's ports and airport, which will then block a lot of the really necessary vital stuff that you need to be coming into Haiti. Now, this is just common sense, and there's a lot of this common sense that's out there among development workers who work in the field that we can draw on to make sensible recommendations.

And in case you think that's obvious, this has been a chronic problem of all disasters, that all disasters are flooded with huge piles of used clothes and old shoes and out-of-date expired medicines with the labels in foreign languages that nobody on the site can understand. There's a lot of useless goods. And so the general principle that you should send cash rather than goods is something that is obvious from the experience of field workers, but has never been absorbed yet. I mean, as we speak, there are old shoes piling up at the ports in Haiti. So, you know, this kind of common sense is out there to guide us in works in development, also.

And then finally, there's this little word -- there's this little concept that we have available to us when we don't know what to do at the -- if we don't know sort of how to have a centralized comprehensive policy package to achieve development. Well, then let's not try to hire experts who claim to know comprehensive development strategies to raise growth because I've just told you there's no empirical evidence they can draw on to support their claims. Let's try something else besides, you know, one great centralized comprehensive plan to raise growth. There is an alternative that has been successfully used by a lot of countries, it's this little thing called democracy. Democracy is a decentralized system where there's -- I mean, we can talk about sort of political democracy, economic democracy, technological democracy, scientific democracy, where there is a decentralized process of using a lot of localized knowledge and positive and negative feedback so that you keep doing what works and you stop doing what doesn't work, and governments -- most rich

country governments use democracy rather than RCTs to decide what social services to supply, that they get feedback from their constituents what they want.

So, remember, this is a very -- this is a system that does have a track record and it does seem to work and this is an alternative to the idea that you need either to do everything with randomized trials or to do everything based on some kind of, you know, super empowered development expert running the whole economy to achieve growth. Neither of those things is going to work as a big picture idea to make development work. We need the decentralized process that comes with, you know, democracy and markets and all those things.

So, to conclude I just want to offer one conciliatory thought before I've -- I've said some kind of mean things about some of the randomistas, even though I really do respect and admire them a lot, and I want to close with a very conciliatory thought, which is, you know, there is a lot of heated debate between the believers and the nonbelievers in randomized trials. But I think there is a much bigger issue that unites us rather than divides us, and that is that, you know, all of us that were writing in this book and that were at this conference, you know, we really do believe in using at least some kind of evidence check to see what works. And if we see evidence that something doesn't work, then we want to stop it. And if we see evidence that something is working, we want to continue it. And it may be localized micro evidence, it may be through democracy, it may be through common sense, it may be through Econ 101, however we get it, we do believe in these principles of, you know, testing, verification, empirical evidence. We're all united on that.

Now, you might think that's something that is so obvious everybody would agree on, but, in fact, you know, a lot of aid, actually, is operating in an evidence-free environment or continuing to do the same old thing even though there's a lot of evidence that it's already been failing. It's like, you know, I already gave you the example of the old

shoes that keep being shipped even though we know that that's a bad idea.

And to now give Abhijit a lot more credit, Abhijit actually gave the example of a -- and this is my closing thing -- Abhijit gave the example of a computer kiosk program for the poor in India that failed to work because there was completely unreliable electricity and Internet connectivity in the places where they were trying to set up computer kiosks. So, again, a context-specific reason why something didn't work, and it was obvious that it didn't work. You didn't even really need to do any more research than just show up and see that the, you know, that the computers were not working. And he pointed this out and he criticized the World Bank's empowerment sourcebook who had noted the success of the initiative. Somehow even though all the computers were turned off, the World Bank had declared this a successful initiative. And even after Abhijit's criticism of the empowerment sourcebook somebody wrote into Abhijit, whom he quoted, defending the program even after Abhijit pointed out that, you know, none of the computers were working. Some World Bank official wrote back saying, well, this program facilitated greater empowerment.

You know, "empowerment" is one of these words you use when you have no other excuses. And Abhijit's rejoinder was that helped to achieve greater empowerment? You achieve greater empowerment through nonworking computers? Having silent computers gives you greater empowerment? Huh.

So, in the end, what unites us in this book is bigger than what divides us, that we all believe do more of what's working, stop what's not working, and get the best kind of evidence you can to tell the difference. Thank you very much.

MR. KUMAR: So, Jessica, obviously we want to hear from you on the random clinical trials agenda and why that makes sense, but just to follow up on Bill's last point that we all believe in evidence -- so, let's sort of take a look at the flipside of his argument. If we all do believe in evidence, the importance of it, why not used randomized

clinical trials across the aid industry? In other words, why not make it a standard part of the way that we do development work? Surely it's expensive, and that's brought up in the book, but the alternative, what Bill is pointing to, is large amounts of aid money that potentially have been wasted over the years. So, is it better to just make randomized clinical trials a key part of the way we do development?

MS. COHEN: Is that a question to me?

MR. KUMAR: That's a question to you. And in addition to -- obviously we all want to hear -- I don't know whether you are a randomista --

MS. COHEN: I would first like to say that I do not consider myself a randomista or a fundamentalist in any way.

MR. KUMAR: I should point out that this book is an edited volume which includes about a dozen economists who have a range of views on the micro/macro spectrum.

MS. COHEN: Yeah. Yeah.

MR. KUMAR: So, you're hearing from two who are not necessarily on the fringe ends, but can --

MR. EASTERLY: Half enemies doesn't have macro economists.

MR. KUMAR: Go ahead, Jessica.

MS. COHEN: Yeah, so, I think I'll answer your question a little bit in my comments.

So, I would like to say that I don't consider myself a randomista. I, like many others that do randomized trials, draw on several different types of methods, though the randomistas do exist. And I will not take you on the emotional rollercoaster that Bill took you on. I'm going to sort of stay even-keeled and not sort of talk to you about why I think RCTs are the solution to the world's problems or a panacea, but rather sort of why they just

make a lot of sense.

And so I want to start by clarifying what I and what many others who work in this area consider thinking small. And the first thing is that, you know, we often think about tweaks to NGO programs or policies or, you know, small improvements in poverty programs or education programs, or small amounts of foreign aid in the thinking small world. But actually much of the thinking small literature really is right at the heart of countrywide programming and planning and also, you know, thinks very carefully about how many, many millions of dollars in foreign aid should be spent.

And so rather, I think that what sort of unites the thinking small world is that it tries to break the world up into manageable and actionable pieces. And so, for example, I think, you know, those of us who think small focus less on the causes of poverty, for example, and think more about how we could make recommendations to improve it, and those are often interlinked, but the former sort of plays second fiddle.

And I think, you know, the micro research recognizes that there are many, many factors that contribute to poverty and growth. There are historical circumstances, political factors, but he chooses to focus on that set of policies or programs that are or could be in our control and evaluate their effectiveness.

So, you know, offering recommendations to aid organizations or to governments often requires us to make if-then statements. If you do X poverty program, you will reduce poverty by Y percent. And in order to be actionable research we have to try to identify the causal impact of certain levers on outcomes. And so generally, I think, those of us who think small, focus on things that we can control or manipulate and observe outcomes in order to sort of make broader claims about the relationship between those policy levers or programs and outcomes.

And so, you know, I think a very natural method for asking if-then questions

is experimentation. Right? We use experiments all the time to manipulate cause and observe effect in almost every avenue of research and of course we do it in our daily lives. We diet to see if we can lose weight; we -- I give my dog a treat to see if she will listen to me. We experiment with routes to work. And, you know, it's just sort of in my mind a really natural way of trying to understand the relationship between things that are in our control and outcomes. And so experimentation in the form of randomized trials for economic and social policy seems to me kind of like an uncontroversially good idea in that sense.

They're also considered the gold standard in a lot of natural sciences and much other research and they're becoming increasingly common in development economics. You probably all know this already, but there are randomized trials in just about everything, part of Bill's problem with it, but, you know, in everything from reducing malaria to reducing corruption to even, you know, how to design nationwide health insurance programs.

And so a common criticism of this approach is that it's sort of too narrow and it limits the number and types of questions that we can ask. And I think this actually gets to Raj's point, which is that there are certain things you cannot ask with randomized trials. There are sort of administrative reasons why, but there's also just generally lots of things out there in the world that are not manipulatable, right? If we want to know the impact of race, for example, on income, we can't put someone in a lab and change their race. If we want to know the impact of, you know, cultural factors that favor boys' education over girls, we, you know, arguably can't change that, but what we can do is focus on scholarships for girls and see whether, you know, that improves girls' education.

So, it's true that this sort of narrower and more pragmatic approach to development grew in large part as a reaction to failures of thinking big. I think, you know, that Bill was definitely right about that, but I still think that it represents progress rather than

looking backward. And, of course, you know, just point out, I don't think this is sort of an academic debate. I think we're asking, how can we make recommendations, but randomized trials can be used for very basic science academic questions. I mean, they can be used to test very deep and subtle behavioral theories and, in fact, I would argue that they're the best way to test theories and in that sense, actually, link very closely to more sort of academic, economic research and this is also a reason why their external validity -- and I guess we'll come back to that -- can be improved.

So beyond sort of the contribution of individual experiments to malaria policy, education policy, et cetera, I think generally it's a step in the right direction for several reasons and the first is just that we're having this conversation. I think Bill was, you know, getting to this at the end of his talk which is that, you know, so much money and planning is based on whims and political expediency and hunches. Just that we're having the discussion about, you know, sort of what is the standard of evidence that we can use to go forward with a policy or program is progress and so I think that, you know, the fact that, you know, more governments and donors are asking about, you know, evidence for the programs they're about to go forward with is progress and I think that randomized trials have really put the issue of evidence quality high on the agenda.

The other is that, I think randomized trials have the potential to improve aid effectiveness by creating a sort of feedback mechanism, and again, this is reasons why beyond sort of individual trials, just this sort of movement toward them represents progress to me. And, you know, as Bill was saying, a major sort of hurdle in aid effectiveness is just the lack of mechanisms for efficiency and accountability. You know, the for-profit world has markets and they have competition. They have a mechanism for weeding out, you know, what's not working and for channeling resources toward, you know, the highest value -- in the highest value directions. And the inability to hold governments accountable for spending

aid dollars wisely or even for NGOs to know whether their program recipients are benefitting is a huge, it introduces a huge hurdle for ensuring that aid actually translates into improved outcomes.

And so absent markets, I think of RCTs as a sort of second best feedback mechanism. I think there are other feedback mechanisms. Our friends at Center for Global Development across the street have proposed one, for example, but experiments sort of offer a second best feedback mechanism and they allow for experimenting with different approaches and a way in which one approach can justifiably be taken over the other. And what they do, I think, is offer us -- you know, they don't offer a panacea, but they offer a way of sort of incrementally doing better. You can tell if something works or it doesn't, and, you know, I think in a world where the number of policy levers that we could pull are basically infinite, this is an invaluable mechanism.

And so, you know, in conclusion, our book, I think, offers a lot of suggestions for how randomized trials could do better and be made more useful and relevant and, you know, I'd be happy to talk about that.

MR. KUMAR: So, let me just follow up right away, Jessica, on a couple of things that you said. You talked about the feedback mechanism aspect of randomized clinical trials which makes a lot of sense. So, in other words, you know, in the private sector if somebody goes out and buys a can of Coke with their own money, that's a direct mechanism which signals something to the market. In this case it sounds like you would need the aid agencies, the donor agencies, to buy into the idea of these trials.

So, I guess I've got two intertwined questions. One is, what's your experience so far? Are the aid agencies positive about this? To what extent are they using randomized trials? To what extent are they integrating to their programming? And second, there was a point about conflict of interest raised in the book saying that in the

pharmaceutical industry, which we're sort of modeling this after, there's a big question mark around drug companies that sponsor these trials and whether or not the results are objective or not. So, how do we address those two issues, that the aid agency is actually buying into this? And then if the aid agencies are actually funding it, how can we know that the evidence is real?

MS. COHEN: So, I've observed a lot more enthusiasm for doing randomized trials than for incorporating their results and I think that's too bad. It makes a lot of sense, though, because in a lot of cases organizations have sunk a lot of money into a program and to find out that it's not working is troubling.

So, you know, I think there are several sort of solutions to that and when randomized trials work best is when you're testing a prospective program. And I would recommend that -- I mean, in addition to the fact that it probably makes sense to pilot something before you spend a ton of money on it, I think -- I would recommend that organizations that want to take this method going forward test -- either test only prospective programs that they -- you know, that are less, you know, susceptible to potential data mining or manipulation of the experiment to get the results that they want, or to test both their existing programs and several alternatives simultaneously, so not just our program, and nothing, but, you know, several different approaches to the objective they're trying to accomplish so that there's something positive.

You know -- sorry, what was the --

MR. KUMAR: The funding for these trials. Who's funding them and is there a conflict of interest?

MS. COHEN: There is. There is a conflict of interest, I think, when organizations fund their own trials unless you find, you know, that rare gem objective, you know, program manager. I happen to work with someone who I think fits that bill, but I know

that they don't exist everywhere. You know, I think there needs to probably be some kind of institution that operates independently to -- not just because, you know, of this conflict of interest issue, but, you know, something we'll get into later, I assume, is the need for replications of randomized trials and this sort of lack of incentive for academics to kind of do the same thing over and over again, but sort of the need for that. And so I think we do need to think about, you know, institutions that support or host those types of trials that have different incentives than the programs that are doing themselves and the academics.

MR. KUMAR: I'm going to ask Bill a question, but I just want to remind you, after this we're going to take questions from the audience, so start to prepare your questions.

Bill, I'll just give you kind of a general question, which is, getting back to the title of the book, what works in development? So, I understand the down sides of both of these approaches, but after all these years and all the study, do we know anything about what works in development? I mean, do we really have no knowledge?

MS. COHEN: Quick, think of something.

MR. EASTERLY: Yeah, I remember a while ago after I had left the World Bank, someone asked me that and I said, well, you know, there's this Michael Kramer paper on how effective de-worming drugs are. De-worming drugs. And they said that's it for development? The secret of development is de-worming?

You know, I think what works is having a system that is sort of like democracy in the aid system, that you have some kind of independent feedback to hold the aid agencies accountable and that's -- you know, Jessica was very right in stressing that point. And I think there is huge concern, as Jessica has just said, that, you know, the randomized trial agenda is really going to fail. Let me just say this bluntly, not only the other concerns I have about it, but just -- I do believe in it as a way to test small things, but it's

really going to fail if it gets captured by the aid agencies. And there's already very worrisome signs that it is being captured by the aid agencies and it's -- in fact, it's very hard to do a randomized trial without the cooperation of the implementing agency, and there is plenty of scope for them to manipulate results. So that's an area that really needs a lot more sunlight to be thrown on it by both sides, by both, you know, the randomistas -- sorry about the label. I'm sorry. You can call me anything you want.

MS. COHEN: Okay.

MR. EASTERLY: And the anti-randomistas. Both sides are very concerned about this issue and the bottom line is we really want to find something that holds donors accountable for results. And that's the reason foreign aid has been failing and wasting so much of its resources is because it's not accountable, that the existing evaluations are almost entirely self-evaluations. And now there's really a gigantic risk that randomized trials will just become another way to do self evaluation, but less -- lend more scientific credibility to the whole exercise by claiming they're using this sophisticated methodology.

MR. KUMAR: Well, we're going to go to the audience now for questions. There are microphones, I'm told. Are the people with microphones here? Can I see where you are? Okay.

I will call on you. Once I call on you, somebody with a mic will come to you. Stand, give us your name, your organizational affiliation, if any, and try to keep it relatively brief so we can get to a lot of people.

I see somebody right here in the third row. Start with you. And there are a lot of hands. I will try to get to all of you.

MR. WESCOTT: Thank you very much for your comments. Alec Wescott, no affiliation. Secretary Clinton delivered a speech on the future of U.S. development policy

a few weeks ago in which she posed -- she sort of posed it as a defense of U.S. aid and development efforts at a time when many argue we should be looking inward to address our own domestic issues. Would you please comment on her speech and this particular argument she made?

MR. EASTERLY: Well, she covered virtually every development topic in the book, so it's kind of hard to give a succinct commentary. I mean, her big points -- her big thing -- I'll talk about her thinking big point, which I profoundly disagree with, which is her big think point was that we can merge defense, diplomacy, and development. And that's probably one of the worst ideas I've heard in my career as a development economist. I mean, come on. Give us a break, all right? I can understand why she would be forced to say that as a politician, but anyone who's not, you know, forced at gunpoint for political reasons to say something so ridiculous would admit that defense is often a contradiction of development.

There's a tradeoff between, you know, catching the terrorists in Afghanistan and promoting the development of Afghanistan. Let's be realistic about that. You know, there's tradeoffs. It's not that somehow the world has changed in a way that now makes all current policies of the U.S. government magically complimentary to each other to achieve the greater good of all mankind, which is what she was essentially saying in her speech, that defense is, you know -- good development is good for defense; what's good for defense is good for development. This is just nonsense. And so, you know, I hope that we can -- as big thinkers, that we can at least shoot down the big-think ideas that are just on their face just sort of laugh out loud silly for anyone who's not under very strong political compulsion to try to, you know, merge the defense lobby and the development lobby.

So, that's my gut reaction. I don't know if you have a reaction, Jessica?

MR. KUMAR: Okay, we're going to go to the next question. I see someone

about the middle of the way back there.

MR. WEINTRAUB: Thank you. I'm Leon Weintraub, University of Wisconsin, in Washington for a semester on international affairs. I'd like to see if I can expand this a little bit beyond strictly economics to get at the idea of values. If we're looking at experiments or random trials, whatever it is, and you see something that brings about greater income -- it might be a change in lending, it might be a change in micro lending, it might be a change in less corruption, so, therefore, people have greater income. But if the recipient of that greater income, for example, decides to spend it on more elaborate weddings for his children rather than on education for his children, or chooses to extend his influence through his extended family by being a patron and helping out more of his cousins rather than using it for personal investment, I mean, do we just have -- yes, we have maybe a wealthier society, but it doesn't seem it's a changed society that's going to move on some type of a structured and integrally kind of a development type of a path. So, perhaps values is much too ephemeral to talk about, but I think we're missing something if we don't address it.

MR. KUMAR: Jessica, is that something that randomized clinical trials can get to, the issue of how the greater growth, potentially at the small scale, is used for development?

MS. COHEN: It's interesting. I mean, one of, I think, the big shortcomings of randomized trials is that they don't, at least currently, have good methods for measuring kind of welfare writ large. You know, how much happier does a program make a person? And that's -- you know, because we focus on specific outcomes and so I think that we miss a lot.

I think most economists would say, you know, whatever they want to spend the money on is fine, although, you know -- and I think it's actually very important for us to

capture that kind of thing because if we -- you know, if we hope to improve children's education with micro lending and the money is going to something else, you know, we want to learn that. But right now we don't have very good ways of capturing it.

I mean, I think that, you know, one way forward for randomized trials is to integrate better with qualitative methods and some people are doing this, both to sort of motivate the study and to interpret the results. So, you know, I would say, I think you point -- I think you make a good point about one of the limits of randomized trials as they stand. I don't think it's -- they're inherently unable to measure things related to, you know, to values, as you say.

MR. KUMAR: Generally no worse than macro studies would be, right?

MR. EASTERLY: Thanks a lot.

MS. COHEN: In general we know we have a much better sense of where the data comes from with randomized trials than we do with macro studies. And so that is, you know, a huge, huge benefit.

MR. KUMAR: I'm trying to create a debate here.

Okay, we're going to go to the front row.

MS. LUBIN: Hi, I'm Nancy Lubin with JNA Associates, and I have a question about this whole notion of accountability, whether it's micro or macro or whatever, of the donor organization. And I'll use our own experience as part of this question.

Back in -- we focus exclusively on the former Soviet Union. Back in the mid 1990s, we were funded by five private foundations to write a big assessment of what works, what doesn't, in aid to the former USSR. As part of that, we came up with two things that seemed so obvious, but weren't. One is, one size doesn't fit all, of course. What works in one country might not in another. And second, the need for accountability and evaluations.

The Hill, the Appropriations Committees, really picked up on this. And in

1996, an amendment was added to the Appropriations Act that required every donor agency to, as part of the process for every contract it was letting, do sort of a societal impact assessment and show that they really understood the political and economic workings of whatever country this aid was going to be applied in and how they expected these interventions to affect, you know, the larger society and not just the households in the countryside. And then second, we called for any evaluations that were done in the donor community. If they were negative they couldn't just be filed away, but they still also had to be made public.

My understanding is the donor community didn't like either recommendation at the time. The law was never, ever enforced. Within a year it had sort of died and wasn't renewed, and evaluations rather than being made public, were stopped, certainly in the former USSR, and pretty much worldwide for a time.

So, my question is, if those two recommendations were part of an appropriations bill today, do you expect that the reaction on the part of the donor community would be the same or has something changed in the intervening 13 years?

MR. EASTERLY: I'll say one thing quick and then you.

MS. COHEN: Sure.

MR. EASTERLY: You know, the accountability is a really hard cause to promote and donors are never going to accept accountability voluntarily. You know, accountability is really forced on you by, you know, the people who demand their rights to hold you accountable and that's how democracy happened. It's not that public officials suddenly decided they wanted to hear from the voters. No, there was a campaign to expand the vote and have more people vote and that's what started holding politicians accountable.

And so we need something analogous to that in foreign aid. We need someone to demand accountability from the donor agencies. They're never going to

surrender it voluntarily, surrender voluntarily to outside accountability.

So, our problem -- of course, the big, general, gigantic problem in foreign aid is that, you know, foreign aid recipients do not vote in Massachusetts Senate elections. The people who are getting our foreign aid programs, you know, they don't vote. So that's what makes the political economy of aid so difficult is, you know, the people that we're trying to reach, that we're trying to benefit, don't have any voting power or any other kind of power to hold anyone accountable.

So, we could either try to sort of mobilize a rich country lobby on their behalf to find a way to give them more voice and really make that a cause, that their voices be heard and that they have allies in the rich countries. That will force that on the official aid agencies funded by the rich countries and will also make that a visible enough cause for private philanthropy as well, that private foundations and NGOs will also feel the heat that they have to, you know, respond to the voice -- a more powerful voice given to the intended beneficiaries with the allies of the beneficiaries in the rich countries forcing it on them.

It has to be by force. I'm sorry. I mean, I don't mean force like the defense and diplomacy idea that Hillary had, but, I mean, you know, political force. We need stronger political force. And I'm not -- frankly, still not that sure how that's going to happen, but I hope it happens.

MR. KUMAR: Okay, we'll go here to the front row.

MR. SEWELL: I'm John Sewell from the Wilson Center. Bill, that was a very interesting presentation. I missed two things. There was a missing participant here, it's what people in the countries want. The aid community is very good at telling people what they should have --

MR. EASTERLY: Right. Right.

MR. SEWELL: -- and what we think they should have, but it seems to me

that most programs don't work unless it's something people want.

MR. EASTERLY: Right. Right.

MR. SEWELL: Microcredit worked because (inaudible) hit upon the formula that responded to people's implicit demand, I suppose, at the original point. And it seems to me that a demand-driven aid program such as the cash payments -- I realize it's not an aid program for sending kids to school in Brazil and Mexico -- works very effectively because most mothers want their kids to go to school and even some fathers do, by the way. And that seems to be missing from the book. It may be in the book, which I haven't read.

The second one is that there's a pretty general agreement, which I share, that competent government, not necessarily good in the overall sense of that word, but governments that are competent to do what government should do, is a very important component of any country's success. But it's the country's choice that they want to take certain policies in place and implement them that moves them to improve customs services, tax collection, you know, whatever you want.

And I didn't know where that fit into the book because all of the literature on growth says, well, yeah, it's worked. The countries have grown. The poverty has been diminished and so on and so forth, but there's no formula and there's nothing that can be imported from the outside. They all looked at their own circumstance.

And that's my -- the last part of my question is, tell me what you think of Dani Rodrik's growth diagnostic approach to look for the easiest soluble barrier to Brazilian growth as the first step in all of this.

MR. EASTERLY: Do you want to say something and I'll answer the Dani Rodrik question?

MS. COHEN: Sure. So, the first question -- remind me of the first question

again.

MR. EASTERLY: What people want. What poor people want.

MR. SEWELL: (inaudible) assistance is not very much --

MR. KUMAR: How to make it demand-driven.

MS. COHEN: Right, right. It's a very good point. I think that, you know, to some extent when I was speaking about combining randomized trials with qualitative work, I was getting at this. And I think, you know, there's almost no way around figuring out what works with a randomized trial without having a lot of contextual knowledge.

I sort of started working in East Africa and have never managed to go anywhere else because I can't really tell -- I can't really tell -- I don't have my bearings on any other circumstances. I can't tell what makes people happy and I can't tell if someone's lying to me.

So, I think improving the relevance and usefulness of randomized trials is about asking the right questions and I think, you know, there are sort of two ways that we could do better. I think one is asking questions about, you know -- in exactly this way, limiting randomized trials to thinking about things that will, you know, not be forced on people, but will be, you know, there's a demand for them, and also the same with, you know, at the government level. So, thinking about potential policies or programs that they would want to adopt or that are in the realm of possibility.

You know, one of the things we discussed in the conference was -- you know, and I don't have an answer to this -- but why are we in the business of sort of doing these trials at all? I mean, is this -- do we know something that, you know, people in these countries don't know or, you know, do we have more -- do we place more weight on their welfare than they do? I mean, it's a very sort of strange business we're in and I don't have an answer.

MR. EASTERLY: Well, let me comment on the Dani Rodrik point, but I'll also point that you were saying, Jessica. I think the randomized trial should be much more oriented towards finding out what people want. You know, it should not be, you know, does the randomized trial give good results on what I the donor want. It should be, does it get good results on what the beneficiaries want? And then we start getting back to the idea of, you know, more democratic rights, more voice of the poor, et cetera.

And on the Dani Rodrik point, I think, you know, Dani Rodrik's growth strategies, you know, I think, like I said before, I think he has a valuable point in pointing out that the effect of policy in every country, in every period, is different. And, you know, that's sort of the general idea of his growth strategies. Of course, you know, if that's your whole -- if that's all that you know about growth, that the effect of policy in every period in every country is different, that doesn't really get you very far. Because it will fit the data perfectly, I can tell you, because the policy can -- if you want to defend any one policy, you can just say, you know, where any country following that policy had good growth, well, it worked there. And any country where it didn't work, you just say, wow, there was some other necessary condition that was missing. And then it just becomes -- everything becomes, you know, non-falsifiable and tautological and we'd get nowhere. So that's the risk of the Dani Rodrik extreme eclecticism approach.

And I think, you know, the only hope that we have is that we have some source of knowledge of knowing what the effect of a policy on growth is going to be and then who is most likely to have that knowledge. And here is where I really disagree with Dani Rodrik.

I think Dani Rodrik thinks that the sort of outside experts, the Kennedy School and NYU faculty flying in, will be best placed to figure out those time-specific, country-specific coefficients of policy, and I don't believe that. I think it's much more likely

that it's going to be local people, local politicians, political entrepreneurs who sort of know what's possible and sense what's likely to happen in the local environment. It's going to be local business, it's going to be local consumers. And when you're talking about policy at a very decentralized local level, the local people are going to know much better than anyone else what's working, what's not working.

So, again, I think we should move away from this expert -- top-down, expert-driven model where you have all the outside experts deciding what works towards a model where you are incorporating tons more feedback from the local people.

MR. KUMAR: In the blue suit here.

MR. CHANG: Yeah, Chow Chang, (inaudible) correspondent. I just still want to know that why we know the problem, but never try to solve, so I would like suggestion. We have a recipient, right, that means we have to bring the recipient to the table in the origin state and the duration state.

MR. KUMAR: I think that gets to some of what you were talking about Jessica, qualitative surveys, right, going in and talking to them about what their interests are.

MS. COHEN: Right. I would qualify Bill's last statement and say that I think -- you know, I think it's perfectly fine for academics and researchers to be in the business of, you know, really nitty-gritty pragmatic policy, but they need to, you know, just plain and simple, they need to be in the field. They need to talk to people. They need to talk to the beneficiaries. They need to talk to all of the stakeholders. They need to talk to the government. And that is how you -- you know, it's sort of a very vague --

MR. EASTERLY: I was talking about macro experts. Even if they talked to everyone, the macro experts are not likely to get it right compared to a local politician or businessman who actually has his livelihood at stake, right?

MR. KUMAR: All right, let's move on. We're running out of time here. Go

ahead.

MR. IENSU: My name is Franklin Iensu. I'm from Ghana. I'm not affiliated with an agency. I'm based here in D.C. I'm an editor.

My question piggybacks on the first two and it generally concerns whether there's a way of adopting the RCT approach in a broader, looser sense, not doing quantitative, maybe statistically based studies, but taking the idea that the only thing that really works is structuring in a hard way, careful, step-by-step interventions that actually change people's behavior. You want them to take malarial tablets correctly. You're not going to solve it sitting, you know, on the 21st floor of some high-rise building in New York. You'll have to actually look at how people respond to particular incentives.

So, is there a way of taking that general idea and applying it to the big picture issues as an African and an African belonging to a number of online discussion forum that are Africa-centric as well as actually attending a big African mega church here in D.C., which is sort of a microcosm of all the, dot, dot, dot. You don't need to -- you see a lot of -- it seems as if RCTs would create islands of success in a sea of dysfunction. And everything I hear about in Africa, I mean, Africa is currently engaged in a soccer tournament. I was talking with a Guinean yesterday and she said, listen, under Sékou Touré, if the Guinean soccer team loses, straight to prison.

I'll give another example. You lend money just person-to-person, and why arm's length transaction business doesn't work in Africa: you lend money, people don't pay back. It's just a cultural thing. We come from an anthropologically gift exchange society and it's very difficult for us to pay -- so, you have corruption, nepotism, all these big picture things that, you know, Transparency International tried to put on the agenda, but they're really not taking it for -- you're not really taking the bull by the horns yet.

So, the question is, how do you -- not actual RTCs, but how do you

structure step-by-step, micro interventions, whether it's trying to get the recipients to express demand or accountability at the donor level or whatever, taking that general idea to address the big picture issues? Because the big picture issues are sinking -- whether it's Haiti or wherever -- they're sinking the boat while we sort of encapsulate ourselves in small islands of success that are working very well.

Sorry for the long question.

MS. COHEN: Sure. I think, you know, one of the -- the reason that we see these islands of success is because RCTs are often done with NGO programs and NGO programs often develop as a sort of parallel system to the largely dysfunctional health systems, education systems, that are public.

You know, I guess "accountability" has been sort of the watch word of the day, but, you know, I think the creation of knowledge, at least about what works, if there's some sort of mechanism that feeds that back to the government, you know, has the potential to at least nudge policy in the right direction. You know, we talk a lot in the book about whether we're just wasting time doing randomized trials because the governments don't care -- you know, governments don't care about it anyway. I mean, they're going to do what they're going to do. And the reason they adopt a policy is not because, you know, someone told them it works best, but, you know, there's this mysterious policy process.

But I think what we all sort of agreed on was that at least it moves you kind of in the right direction. And I think, you know, if there are these little pockets of experimenting with education, you know, with infectious disease, and if donors and NGOs and, you know, constituents, sort of at least nudge their governments in the direction of, you know, something that appears to work better, then that could be progress.

And the other thing is that, there have been some randomized trials on empowerment issues and I think there should be more because it's clearly a focus -- there's

clearly an area that we need to focus more on. But the upshot was that they were quite successful at empowering people to demand safer driving, for example.

MR. KUMAR: I see one in the middle there, and please try to keep the questions brief.

MR. WILSON: Thanks. (inaudible) Wilson with Johns Hopkins (inaudible). I was particularly influenced by a recent book, Dambisa Moyo's "Dead Aid," which argued that foreign aid is intrinsically -- you know, it's harmful, and even something as innocent as say provision of mosquito nets will destroy the local industry and so forth. And I just wanted to get your thoughts on her book, which was particularly influential for me. Thanks.

MR. EASTERLY: Well, you know, I think Dambisa Moyo is part of a sort of new generation of African intellectuals who are much more independent and critical of aid, and I think that's an extremely healthy trend. You know, I think it's -- you know, that's -- you know, the accountability system definitely has to include, you know -- if we're talking about Africa, for example, you know, African intellectuals, locally based people who really have a lot at stake, you know, either politically or economically or whatever. And it's amazing the degree to which we do -- those people are not included in the development conversations right now because there's this mindset that you can only talk to the government. And people don't want to hear from Dambisa Moyo or from other African intellectuals, like Andrew Mwenda, a Ugandan journalist who's been a crusading voice against corruption in Uganda and in favor of democracy, and it goes on and on the list of new African intellectuals. And this is such a great and healthy trend, and I think the aid agencies really need to start taking them more seriously.

And also, if you want to think about, you know, what can outside agents do that would facilitate this kind of homegrown, local leadership that would maybe do a lot better than foreign aid, because we know actually most development successes are really

homegrown and not the result of grandiose outside developed aid programs, then, you know, why not think of programs that promise to create even more African intellectuals, programs like much more generation immigration policy towards Africans that allows them to circulate through the U.S. or the European economy and gain other access to new skills, new contacts, new technologies? And a lot of them will go back to Africa temporarily or permanently with that knowledge.

And, you know, Michael Clemmons is sitting here in the audience and has been a great proponent of increased immigration from poor countries, which is one of the best things you can do for not only the poor individuals themselves that immigrate, but I think the old idea of the brain drain is wrong. That actually there's a brain gain when you allow Africans and other poor country immigrants to enter rich countries and gain knowledge.

And also, you know, one aid initiative -- one type of development initiative that I think is one of the -- it has the highest ratio of being really good to having really next to no funding is just scholarships. Scholarships are sort of the ideal kind of aid because they help people help themselves. They help them get an education that they will use to help themselves and then they will use to help their home country to be in the next generation of business and political leaders and aid critics like Dambisa. More Dambisa Moyos, you know, that's, I think, what you're getting with scholarships. And, in fact, that process worked in Latin America and in Asia. But then by the time Africa's turn came, the aid community had turned against scholarships and was terrified of the brain drain and no longer funds scholarships, and I think that's a shame. I think both the restrictions on immigration and the lack of funding for scholarships is really one of the most embarrassing features of all aid in general today.

MS. COHEN: I'll just say quickly on the issue of whether aid destroys industry, I mean, I've heard this. I haven't seen any sort of -- I hear this said a lot and I

haven't seen any good evidence that that's the case. But, you know, my uninformed sense is that it's not a good reason to not do an aid program that is effective because most people in poor countries are very entrepreneurial and if the aid goes away, then a market -- most likely a market will develop for that product. And perhaps there's sort of theory to be worked out about why there's these sort of longer term problems with, you know, squashing markets for things, but I think it's context dependent.

MR. KUMAR: We've got time for just two more. We're going to take one in the front and one all the way in the back. Keep them, please, brief.

MR. COONROD: John Coonrod with the Hunger Project. I want to ask about a more democratic way of measurement and that, to me, was characterized by the Narayan book last March, "Moving Out of Poverty," that the World Bank published where they've actually done a very good job, I think, of surveying lots and lots of people who had moved in and out of poverty, and what factors they assessed had been critical in that.

It seems more consistent with what might actually be a democratic forum of putting spotlights on where one ought to invest at a policy level than random -- we're involved in some randomized trials. I'm still not convinced that they're going to work because it seems to me that the real issue is more systemic in the interplay of things rather than this linear interventionist approach, where as a more populist approach like Narayan's might be a better way for us to go.

MS. COHEN: I couldn't -- I don't think that one should be a substitute for the other, but that type of, you know, qualitative research leading to things that can then be - - that should then be tested more rigorously, I think, makes a lot of sense and I do it myself. I think, you know, with many randomized trials, usually I've talked to so many -- you know, people that I know who do randomized trials, usually have talked to so many people that, exactly, you know if something's not going to work, actually, because you sort of have a

sense for the environment. So, I couldn't agree with you more, but I don't think that one is sort of a substitute for another.

MR. COONROD: Well, except -- just that you -- I think it might be.

MS. COHEN: Okay.

MR. COONROD: And the reason is that there's an implied assumption in a randomized trial that intervention A on its own, put it in or remove it, makes a difference whereas the intuition of a lot of people on the ground is it's the interplay and the synergy among 9 or 10 development factors in the village level that are going to make the difference.

MS. COHEN: Yup.

MR. COONROD: So, that synergy way outweighs the individual intervention.

MS. COHEN: Right. So, that is a limitation when you're getting up to 9 or 10 factors. But when there are several, I think it's, you know, routinely you don't need to vary one thing in a randomized trial, so if we think that there's some combination of factors that matter, then I think, you know, that just focuses you on what you should test.

So, there isn't technically an upper limit to how many things you can test. You just need a gigantic sample size. But I think it's becoming more routine to look at interactions between.

MR. EASTERLY: Can I just make a couple of comments, John? One is, I think, you're right that the kind of things the Hunger Project does actually do not lend themselves that well to randomized trials because they're -- you know, one of your good ideas, I think, is letting the local community lead when they build community centers and so on. And that itself becomes -- it's sort of like the Heisenberg Certainty Principle, that when you try to evaluate it, you change the nature of the intervention because the randomized trial cannot let the local people lead and move at their own pace on how fast they build the

community center, which combines a microcredit and a school and grain storage and so on.

You have to start ordering them around, you know, you have to move faster, or we're not going to have the results in time for our randomized trial, and the randomized trial scientist becomes the dictator of the village, and, you know, suddenly you've gone from community leadership to a dictatorial community. That's not good.

And by the way, I have now given you a great out in case the results are very negative on the randomized trial.

The second thing, I love your reference to the Narayan -- which is actually also co-authored by the great development economist Lant Pritchett, you know -- of just asking people, you know, how did you get out of poverty. They went around asking people how did you get out of poverty? So, you know what the percentage was that said that they got out of poverty because of charity? 0.3 percent. 0.3 percent. Things like hard work, thrift, my own efforts, help of relatives, that was accounting for the other 99.7. So, you know, you do learn a lot by -- just ask people. There's a lot to be said for that.

MS. COHEN: I just want to follow up on something quickly. I would never propose doing a randomized trial to tell you that something that is clearly working, is working or something that is clearly not working, is not working. I think the point is in case other places want to adopt that strategy and, in that case, they should be the ones doing the trial, right, not you.

So, I think, you know, we can see with our eyes whether something's working. We don't need to evaluate it. I think they come into play more when you're thinking about replication and when there's something that's sort of more ambiguous about one strategy versus another.

MR. KUMAR: We're going to take one last question all the way in the back.

MR. ALTMAN: Hi, I'm Fred Altman. I'm retired from NIH. And I'd like to know if any of the randomized clinical trials are really done in a way that in the medical profession works, that is, you identify some program or something that works, then you go back and do more studies to see what the active ingredient is that's actually making it work. And then after that you have to do a translational study to see how this works in the real world because in the real world that one isolated thing you've identified can well be swamped by everything else.

MS. COHEN: Do you want to talk about the successes -- the African successes?

MR. EASTERLY: Why don't you answer the narrow question first and then I'll give a more general comment.

MS. COHEN: Okay. Not much, actually, and even -- you know, that whole approach is not taken much by randomized trial. Typically they look for what works rather than finding something that works and figuring out why. But it's even more basic than that. I think people who do randomized trials often fail to specify the, you know, what sort of analytics they're going to do and that's sort of standard in medical trials and is not yet standard in randomized trials and social and economic policy.

MR. EASTERLY: Yeah, I think Jessica said something very important that kind of partly answers your question earlier in her presentation. She said that one of the most valuable uses of randomized trials is testing behavioral theories of how people behave, and then that gives us a huge sort of multiplier effect that it has a big payoff way beyond the impact of just that one intervention, that we're starting to understand how people respond and behave in a way that aid agents can try to adjust to. And again, that's understanding how local people behave. That's kind of, again, getting back to the essence of one of the central ideas I wanted to push, that, at the end of the day, what really matters is that the

local people are in charge; that the test of what works is not whether the donors go away feeling happy, you know, that's -- I know I myself have a dramatic inability to make anyone who funds me happy, but -- or anyone who employs me happy unless they're a university.

Unfortunately, right now we're in a situation where we have to keep the funders happy. And we need to switch to a model in which we understand what makes poor people happier about what they're getting through development programs or less happy. And that's what understanding the behavior and the culture, which randomized trials can partly help do -- obviously a lot of other methods will help do also -- will get us to the goal when we really do get to what something that I hope does in the end eventually look much less like an authoritarian aid system where the donors dictate what poor people need and we come to a new world of aid where poor people say what they need and they're in charge. That's the world we want to see.

MR. KUMAR: I guess that's the key difference between the medical field and what we're talking about. The medical field you've got a private sector for pharmaceuticals and then you've got the FDA, which is a government entity that requires these clinical trials and they have to meet a certain standard, whereas we're talking about a field that doesn't have that outside actor to impose these trials. It's really coming from within, so that's a fundamental difference, I think, between the two as well.

So, we've run out of time for questions. I want to give both of our panelists just a minute to recap or to say anything you haven't had a chance to say.

MR. EASTERLY: Do you want to go first?

MS. COHEN: No, you can go first.

MR. EASTERLY: Just thinking what she's going to say while I babble for a few minutes. I'll try to give you some space here.

So, you know, I think the bottom line is randomized trials have -- they've

been useful, really, in two ways. First of all, they do represent a welcome recognition that we have to lower our ambitions from the old days, the kind of ambitions that the aid system had in the old days of transforming whole societies from poverty into prosperity. That's really just not on the table anymore. That's not something that foreign aid donors can realistically do, transform everything: you know, the economy, the political system, the institutions, everything. That's just not going to happen. A lot of historical evidence supports that, like, you know, that's the kind of stuff that Dambisa Moyo was talking about and lots of other books and articles talk about.

So, the randomistas did introduce -- I mean, I remember Esther saying at another conference very memorably, you know, development economists just need to lower their ambitions. You know, that's like a saying that I once heard in some kind of personal advice column, the secret to happiness is to lower your ambitions. Development economists and the aid system will be a lot happier if we realize -- if we're realistic and humble about what we as foreign, outside actors can do for other people's problems if we're more realistic. And then, of course, that does leave you unsatisfied.

What is going to lead to development and prosperity in the long run? And there I think the macro economists have basically gotten it right through some combination of empirical evidence and common sense, that basically it's -- you know, the values of a democratic market-based society and the institutions of a democratic market-based society that get you to prosperity. And that kind of redounds with a theme that we're talking about today, that what we need is sort of more democratic accountability of aid donors to the beneficiaries. We need that to make the aid system to work better. But that's also what we need to make development happen, and that's going to happen mainly by homegrown efforts in the poor countries, to demand, as poor people -- as outsiders we've been very lukewarm and lame in our support of democratic rights for poor people. And I think that's

where the future development is coming from, from greater democracy, greater democratic accountability by which poor people can hold their rulers accountable for good public services, for good policies, and for market economies that give consumers what they want.

MS. COHEN: So, I'll just, you know, be brief. I think -- I mean, I hope that what you take away from the book and today is that while there may be a few people saying that randomized trials are the solution to everything, I doubt that sort of any instrumentalist or planning approach to development is going to be the solution to everything, but all they are is a methodology. But they're an extremely powerful and useful methodology to not sort of fly blind with, you know, how aid money is spent. And while we're all concerned about accountability and hearing, you know, sort of poor people's voices, I think that one of the goals of randomized trials is try to -- you know, to try to get some kind of feedback going.

I would never be -- I don't think any of us would be in favor of imposing the results of randomized trials on people, but, you know, they do, when conducted properly. And I think, you know, we've brought up a lot of things today about how, you know, how they could be made, how they could be improved. But when done properly, they do provide, you know, a sort of feedback mechanism, an accountability mechanism.

MR. KUMAR: So, let me just close with a quote from the book which Bill alluded to in the beginning, and in a way it's sort of a call to action to those of us who are in the aid community. It says, "The most relevant divide in development may not be between micro and macro, but rather between those who value objective evidence and those who do not."

So, to the extent that -- since we do not have an FDA in this field, to the extent that those of us who are a part of aid industry value objective evidence, it seems to me that that is the call to action from the book, that we've got to change the culture and put more emphasis on that which we know.

With that, let me thank our two panelists. Let me also recommend a fantastic book to those of you who have not read it. It's a little bit heavy for economics. For those of you, like me, who haven't had an economics class in 10 years, it's a little heavy, but it's worth getting through.

MR. EASTERLY: You're exaggerating. Your mother will understand it.

MR. KUMAR: It's great. It's great.

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Expires: November 30, 2012

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