

SPEAKER 1: On November 5th 2019, the Center for International Development at Harvard University. Presented Nobel Laureate, Michael Kremer in discussion with Harvard students. Michael speaks about his journey as an economist. And the experimental approach to alleviating global poverty. For which he was awarded the Nobel Prize with his colleagues Abhijit Banerjee and Esther Duflo of MIT. Followed by a Q&A discussion with Harvard students in the audience.

SPEAKER 2: Without further ado, I'd like to invite onto the stage the director of CID, Asim Khwaja.

ASIM KHWAJA: Thank you everyone. Thank you for coming. I always think in life the most precious thing we have is time. And the fact that you guys are here, you're sharing your gift of time with us. We hope this will be worth your while. I am definitely fascinated to listen to Michael. I've listened to him for years, but I still can't get enough of him.

And so I hope this will be at least some sense of that. I have two very simple roles today. I'll be very quick because I don't want to stand in front of the main attraction. The first is just introduce the Center for International Development. We are based at the Kennedy School, but we very much feel like our mission is to service the entire community.

That ranges everywhere from undergraduate students, all the way up to senior faculty, across all the schools. And so I'd encourage you, if you haven't been involved in our events. We'll be doing a lot more of these events. This is just a fabulous space. So thank you for the space organizers, for this space. But you'll be seeing a lot of events from us.

There are several series of events. Hopefully, you've all signed up on our mailing list. please follow that, we'll send you events there. We have a Friday series which is awesome. Where we have leading practitioners every week. It's a great chance for you to meet some of the leading academics and practitioners in the field. You can also follow us on social media, and the like.

The second role which is really close to my heart is to introduce someone I deeply respect and care about, Michael Kremer. So I'm going to have Michael come up to the stage. So, before Michael begins I'm going to embarrass him, because it's fun. You know about Michael when you read his work. we often as academics tend to see each other as researchers, as walking resumes or walking CVs.

And when you take that approach to Michael, what you see is spectacular. But that's just the tip of the iceberg. The Michael I know and the Michael I've had the chance to learn from. Michael was my advisor by the way in my PhD. I did my here, and he was one of my advisors.

And I wanted to share a few things about Michael from that experience. Michael doesn't know what I'm going to say. So Michael's going to be like oh no. But I think it's important to realize that. Because it's important to realize as much as we're driven by the passions that we have. Intellectual passions, or scholarship passions.

We very much are more driven, and deeply driven by the emotional objectives that we have. We all came into this field, into development, because we truly, deeply care. Often as academics what happens is that discourse gets hidden. When we present our papers, you end up seeing data, numbers, and statistics.

But you don't realize that what gets us up every morning is not that. Is the chance that we have the opportunity that we have to make a difference in the world. And that is something we all share. Whether you're a freshman, whether you're a professor, whether you're a Nobel Laureate. Or Larry is here, whether you're the president of the University.

Any one of us has that passion we share. And that's, I think, the essence of who we are. Michael, when you take that view of Michael, literally typifies that. And that's the part of Michael which-- when I was a PhD student, I remember-- I feel like the biggest gift you can give someone in time, is intellectual guidance.

When you're trying to figure out what you want to do in life, whatever it is. That's some of the most profound moments in your life. And those individuals who make a difference. And you all know those individuals, you've all experienced those individuals. Are individuals you will never forget in your life. Wherever you are how successful you are.

Michael is that for me, and is that for many people. They're many in the room who would say that. There are many others who are not in the room today, who would say that. And that's just spectacular. So I just wanted us to acknowledge someone like Michael. Forget the Nobel for a second. Just on that, I think is just worthwhile recognizing Michael for who he is.

So Michael I wanted to personally thank you for that. The second thing about Michael is-- Michael will never admit this. But Michael is out there to save the world, he literally is. Michael is one of the few academics I know who creates global public goods. Global public goods, there are many examples, you can look at his CV. I won't embarrass him further by saying that.

That's unusual. That's unusual for an academic to be at the top of their career. But still take time out, very precious time. And do things which are chlorinating the world, vaccinating the world. Just deeply, deeply concerned about making an impact off his work. And that's another reason I want to recognize Michael.

At a very personal level, from guiding intellectual journeys. To a really global level of addressing-- and by the way these aren't necessarily things that he was recognized for his Nobel for. I just want to be clear, there are many facets to Michael which we should celebrate. In addition to what we're celebrating today. So with that Michael, thank you for sharing time with us. Over to you.

MICHAEL KREMER: Thank you very much, Asim, I really appreciate that. And let me just say I've learned a tremendous amount from you. And maybe you deliberately didn't mention this. But we first met at MIT, am I allowed to say that? So when Asim was an undergraduate and it's been a pleasure working together.

When you were an undergrad, when you're a grad student, and now when you're a colleague. And I would say that one of the-- going beyond awesome. Great thing about Harvard is you learn every day. I learn from my colleagues, I learn from graduate students. Just sitting in the development lunch, for those who are grad students who know what this means, development tea.

I teach a freshman seminar and I learn from the students in the freshman seminar. Who bring a very fresh perspective to things. And often just working on a paper with a student in a freshman seminar. Who's now just graduated but we work together, well he's an undergrad. So this is a wonderful place and I'm very grateful to all of my colleagues and students. In the economics department and at the Center for International Development.

Asim asked me to say a little bit about how I got involved in International Development. And I could agree with him that many of us in the field. And this was very fortunate to have been recognized for the work. Along with Esther and Abhijit we all feel this way. But really we all know that this is the work of an entire field.

And that includes many, many people. Researchers in this area, but also this type of work requires-- and I think it's part of what makes it exciting, and makes it intellectually interesting. It requires and naturally is suited to involvement of teams of people. Teams of people with very different backgrounds.

So the cross subject matter area. So economists, but also public health specialists, or education specialists, or psychologists, depending on the project. But also it involves very deep interaction with the government workers. Who have problems that they are motivated to try to solve. Or NGO workers or people into the business.

And then whoever the ultimate clients of this are. Whether that's students in school or farmers. And you get insights in working in this field from all of those people. And the numerators, because a lot of-- Asim was referring to it as global public goods. But it requires infrastructure organizations like Jay Powell, or IPA, or CID.

I think what's perhaps unusual about this field is, I think we all feel that we're working towards a common goal. And I don't want to claim there's never aren't unmotivated by other things. But we do, I think at the end of the day, in this field recognize that we've got common objectives. And that's wonderful.

So Asim had asked me to say a little bit about how I got involved in the field. So let me say a bit about that. Just wanted to respond to the wonderful, wonderful introduction. I'd been interested in international development and cared about it. And it's not just interest as Asim said, it's embarrassing to say in some ways. I don't know why we feel embarrassed.

But I cared about this issue and I know Esther and Abhijit the co-laureates feel the same way. I think that's in part because of how I was raised as a child by my parents. That we have certain obligations when there are injustices in the world. To try to help address them.

AS I said later, I spent time in Kenya. But it wasn't, on I spent time in Kenya and then they got interested in this. It was because this was an issue that I cared about, that I spent time in Kenya. I was a social studies concentrator as an undergraduate. Benefited immensely from that program.

I think one of the things that people who work in this field very quickly realize. Is the interdisciplinary nature of the problems. And social studies for me was very helpful because exposure to a lot of different fields. And different ways of thinking about issues as well, so the great thinkers who one reads in social studies.

But I took more economics classes over time. And I took a class by some of the precursor to CID at Harvard Institute for International Development, who worked in Kenya. And through him, I maybe should say that I applied for a senior thesis research on some programs in India and Sri Lanka. And I applied for funds to do research for a senior thesis over the summers.

I assume many of you will, and I got the funds and I very diligently went to Sri Lanka. And I went to the library and the government offices. To look up the records on this program, and diligently wrote them all down. And then at the end of that summer, I got a chance to go visit a village.

And realized I'd been wasting my time the rest of the-- day and waste that maybe too strong. But I'd been missing out the rest of the summer. And so having done that, I realized it was important for me to spend more time. If I wanted to work in this field, to spend more time in developing countries.

So in this class I met John Cohen who unfortunately has died between then and now. I also met my fellow students in the class. And actually as much through my classmates as John Cohen. Was able to arrange to spend some time in Kenya.

And I was originally only planning to spend a few weeks there, and then come back and get a job. But I was called to the-- Kenya was not a democracy at that point. And I was called to the office of the head of the local government. And I thought I might be in trouble, but he told me he was starting three schools. And asked if I would teach at one of them based on zero qualifications, I should say.

But I did wind up teaching there, and wound up spending a year. And then at that time and it's changed radically since. But there were a lot of Kenyan schools that were interested in bringing in teachers from overseas. And looked for somebody to replace me. This was a brand new school not didn't have much money.

And so they were looking for volunteers where they could find them. And it turned out there was a lot of interest among Harvard undergraduates. I originally wrote to the social studies program and to my house. And realizing there was so much interest in each side, set up an organization called World Teach. To help match Harvard, quickly grew beyond Harvard students with schools in Kenya.

Kenya then actually rapidly transitioned to a point where they were producing plenty of teachers. And we started working in other areas.

But the time that I spent in Kenya really deepened my interest in development. And made me appreciate the importance of being involved on the ground. Some people have asked, how did I get involved in RCTs. And so in graduate school I studied development. But I studied it from the point of view of economic growth.

So macroeconomics and Robert Barro, some of you may know was my advisor. So he's contributor to many fields of macroeconomics, but including the study of economic growth. And so this was almost the opposite approach. This was very much driven by theory. Thinking about very big picture questions. And I feel like that's a very important part of training.

Sometimes people create a false opposition between microeconomic work and macro. And I see them both as very much part of the same picture. So after graduating I got a job at MIT, started earning a real salary. Went back to Kenya to visit some friends of mine. And I was just on vacation with my wife Rachel Glennester. Who's very active in this field and was head of the Poverty Action Lab at MIT. So played a huge role in this field as well.

So we were visiting a Kenyan friend of mine who'd been the headmaster of a school when I was teaching. But had gone on to work for a very small NGO. And they were starting work in a new area of Kenya, where they hadn't worked before. And they wanted to work in schools but they didn't know-- they were considering a bunch of different ways of working.

And as we talked about it, they agreed to that they wanted to explore on how to operate most effectively. And what they decided to do was to try different programs in different areas. And because they were just starting, and they needed to phase in gradually. To time the phase in a way and structure the phase in. So they had comparable groups of schools. That were placed in early and phased in later.

So that meant that partway through you could compare the schools that had already got a particular program, to other others that hadn't. And of course this it might seem like basic logic to do something like this. But this was not common at the time. It was a very simple approach and obviously used in medicine, in many other fields. I think it's proven to have a huge impact in the field, this approach.

When I got involved, I was interested in it primarily from the standpoint of getting stronger evidence, and more reliable evidence. And when you try to statistically control for various factors. To isolate the impact of a program, that's very hard. And a lot of work that was going on in the field. Suggested that we weren't as successful at that as we were trying.

And this seems like a way to get at that. But I think what's turned out to be the case over time. Is that in addition to any benefits from the better causal inference. There are other very important aspects of this. Because it naturally involves something that hadn't been standard for economists up till then. There are certainly some economists who were doing it.

But economists tended to either do models with paper and pencil, or to analyze data sets. And I those are both very valuable approaches. But there's also something to be gained by the richer interactions that you

get, it was describing earlier. When you're talking to farmers, to teachers, to students. When you're talking to government officials and people in NGOs.

And that brings in all sorts of new insights and I'm happy to discuss some examples of that later on, if people want. So the other aspect of this I think is relevant. And in some ways different than what many economists thought they were doing, or claimed to be doing. We like to pretend we're scientists and so we emphasize trying to understand things.

But the truth is, not just in development but in many fields of economics. People are motivated because they care about social issues. And partly what we're doing, is we are trying to understand the world. But we're also trying, often, trying to innovate. And trying to come up with approaches to solve problems, that human beings are facing. And that can make the world a better place.

And the recent Nobel Prize to our Roth, who is a theorist. He developed algorithms for matching they are used for medical residency. Matches for those of you who are from the Med school. But are also used for, his network is now developed techniques to use this for matching kidneys. For people who need kidneys and people who are willing to donate them.

And that's an example from a very different field, of actually approaching practical problems and trying to address them. And that involves-- I think one of the exciting things about this whole movement of randomized trials in economics. Is people are and they're open about it now. They're trying to innovate, come up with new approaches, test them.

Go through this process of-- this used to be called, including by me, randomized evaluations. But they're not just evaluations that you do at the end of the project cycle. Think of these like, often they're beta tested. They're trying new ideas, you don't get it right at first. You tweak it, you change it, you realize you're going the entirely wrong direction. You try a new approach.

And over time I think we've made a lot of progress as a field. And again, I'll stay abstract now, but very happy to give examples. We've understood things about the world that allowed policies to be improved. But we've also come up with, for particular problems, innovative ways of doing things.

That are now starting to make a the difference to people's lives. So maybe why don't and I pause now. Happy to answer them or others, if you have questions. But I'd love to open it up to the group and also happy to talk about particular examples.

AUDIENCE: Academically RCTs in developing countries have been a striking success. As evidenced by your Nobel in economics this year. Practically, what would you say is the most striking success story of RCTs in terms of economic development? Did the poster child of development, China, use our cities to become rich?

MICHAEL KREMER: Great, OK, thanks. Let me take the first part of that question. of what's come out RCTs. And let me start with an example and

question, or make a come out here. This too me seems when an example and then I will come to the China question. Because I think it's extremely interesting. As I was saying earlier, I think two things can come out of any economic research but including RCTs.

Which one is understanding of the world, which can maybe shape policies more broadly. And the other is specific innovations, so let me talk about understanding. In the 90s when I started doing work in this field. There was a widespread view among many policymakers. This range from NGOs, like the NGO with which I was working. All the way up to the World Bank.

That it was very important-- It wasn't a good idea to give away things for free. And by the way this view persists. And because if you gave things away for free, people would value them. They wouldn't use them properly. And also that it was important for sustainability. Financial sustainability, to raise money. So people applied this to a wide range of areas.

One area that I researched fairly early on was treatment for intestinal worms. So may not be-- if you have pets you may be aware of them, or if you're a farmer. Here in the contemporary US we think of these worms as problems for pets. And if you're a farmer, you probably feed large quantities of medicine to your livestock, to address them. So these are worms intestinal worms.

But 1 billion people, 1.5 billion people in the world have these. And they used to be a problem actually in the US South as well. So together with Ted Miguel we evaluated the NGOs efforts to treat the worms. And we found huge impacts on education. So we initially looked at a whole range of things in education. Some of them didn't work, some of them worked, but in unexpected ways.

Deworming had a phenomenal impact. It's extremely cheap, it's cost pennies per dose, it's safe. So the World Health Organization recommends that in areas where there's high prevalence, go to schools. Because this is highly prevalent among school kids and treat all the kids. So we looked at this, and we found huge increases. School absence went down by one quarter.

And later by the way, we followed this. and we found that girls were more likely to pass the primary school leaving exam, go on to secondary school. Boys were working more hours as young adults, incomes went up. So the program paid for itself a hundred fold. So we were excited to find this, but the NGO had an approach of saying, we don't like giving away stuff for free.

And so we're going to charge for it. And so we talked to them, we developed a relationship with the NGO. And they thought, OK, look, we'll try it. We'll try in some schools continuing to give them away for free. And what they found was I think it went from 75% take up. When it was just the parents had to sign up, to I think it was 17%, with a very small fee.

So that was one result in one context. But then there was additional work in many other contexts. In looking at mosquito nets, Pascal and Dupar at Stanford. And Jessica Cohen who's your at school of Public Health in Harvard. Found similar results, it showed up case after case.

And Meanwhile, there was exciting work going on in behavioral economics, by David Laibson and Matt Rayburn. And many others who are here at Harvard, as well as some people outside of Harvard. And one of the things that they were finding, is that there's what's called present bias. People focus on the present a lot relative to even just a short period later.

And putting these two-- we were able to try to understand more. That first this is just a systematic phenomenon of under investment in preventive health. And second, we were able to and I think research continues to go on in this area. I don't have all the answers. But there's reason to think that behavioral factors play a role in this. And we're trying to tease that out.

So since that period there's been a huge change in policy in the world. On public health issues, across a variety of sectors. That there's much less attempts to charge for say, mosquito nets or other preventive health. It did work on water and water treatment, same thing. So that's an example of not-- what we learned about one particular thing.

And obviously you can't generalize a result like that, by saying let's subsidize deworming medicine. You don't want to do that in Canada, where there aren't a lot of worms. But probably in Canada there are-- I don't know what the public health issues are in Canada. But I'd be willing to bet that on preventive health people in Canada are not-- I certainly know for myself. I'm not doing all the preventive health things that I should do.

So that's one type of lesson. I've been talking a while, let me say something about China, and then happy to take more questions. I've been learning a bit about China, recently, I know very little. So take this with a grain of salt. But China doesn't do many RCTs, but China does a ton of experimentation. Compared to really any other country I know.

When they are thinking about a new policy they'll typically try it in a particular region of China first. And then depending on the results, they'll either roll it out to the next level. And then do a second wave of testing, or they roll it out further. And often there are many waves of this, before they roll it out nationally. And I don't want to claim that that's the main reason why China has grown.

There are many, many reasons. International openness, a system of decentralization combined with incentives for local party leaders. To achieve essentially set objectives, which were economic growth over this period. And more recently, an emphasis on trying to fight poverty. And obviously all sorts of political things.

But I do think that experimentation has been part of what's helped China grow. It's not RCTs and I think we'll have time for a separate discussion. About in what cases is it OK not to have a very carefully defined control group? And in what cases is it important to have a carefully thought through control group. But they certainly do a lot of experimentation in China.

AUDIENCE: Awesome. Thank you so much for your time, and for being here, Michael. I had another question on behalf of the audience that we submitted audio. The audience was wondering if you could share some

advice on choosing topics and coming up with groundbreaking ideas for research topics.

MICHAEL KREMER: So when I'm talking to people who are interested in senior thesis. Or to graduate students looking for research topic. I would say choose something that you care about, and that's important. Care about could be this puzzle that you just can't get out of your head. And you want to try to solve. It doesn't have to be care about the mission or something like that.

Whatever motivates you, because it's a lot of work to do research. And you'll probably do a better job if you're doing something you care about. And the other thing which is I guess I'm feeling one data point. But people are often worried that I have to do what the market's going to want. And I have to do something that's going to show off technical skills, if it's a PhD student.

I think if you do what you're interested in, it pays off in the end. And I guess I'm feeling like I'm super fortunate in that, because it's doing that now. So I would say work on what you think is important, work on what motivates you. And that's just to be clear that doesn't mean don't give advice from people. Definitely get advice from people, particularly in a place like here. You can get a ton of advice.

The other thing I would say is you have to be open to redefining that over time. I often feel that for me, the most exciting experiences in research. Are when I've done a paper thinking it's about one thing. And then realizing, no, this is about something totally different. And that happens time after time. So try to be open to what-- it's an empirical project. Try to be open to what the data is saying. And it's a theory project, it's the same thing.

AUDIENCE: Thank you. My question is, how can we apply the intellectual rigor of RCTs. To address some of the big policy questions like does free trade help or hinder development in poor countries. Or just free capital mobility help or hinder poverty alleviation.

MICHAEL KREMER: I'm sorry I didn't catch this could you repeat the question?

AUDIENCE: Right, so my question is because you have managed to use RCTs to scientifically measure the effectiveness of specific policy intervention, like deworming. My question is, can we apply the same scientific rigor of our RCTs. To answer to some of the big policy questions. Like whether free trade helps or hurt poor countries? Or whether free capital mobility is good or bad for development.

MICHAEL KREMER: Right, great thank you. So first let me be completely clear about this. I don't think that RCTs can be used to answer every question. And economics or social science more generally requires a whole set of tools. And I think those tools are constantly evolving and improving, and we're adding new things to it.

And whether that's adding game theory to economics, which hadn't been part of economics. Or developments in international trade. All of those things, I think we're improving along a host of dimensions. But one other thing that's turned out to be the case. Is that we are finding that RCTs are applicable to shed light on questions. That we might have

thought we couldn't and shed light on.

So let me take an example from international trade. There's been a view-- so again, this is going to be the intellectual things that you can get out of RCTs. As opposed to we necessarily have a practical policy. So one view in trade has been that it's important to export. Because in the process of exporting you learn things. You learn things about what the customers want, about how to produce better.

And that's an argument, there's a big debate in economics in development economics, but more broadly. Should government stay out of the way in trade? Or should they actively intervene? And people who want governments to actively intervene often argue that there's a learning effect of trade. And that, that learning isn't entirely captured by the firm that's exporting.

And that therefore, it's worth subsidizing this, because other firms will benefit. And ultimately the population will benefit. And I may not be completely up to date on the latest literature. But economists have spent a lot of time trying to look for these effects. And had remarkable difficulty finding them or proving this.

Well there was a recent paper in Egypt where they worked with firms that were producing carpets and exported them. Sorry, mostly producing them not for export actually. But then they worked with exporters, and they got the exporter to choose suppliers. And to introduce some element of randomization into that. So they could then isolate the impact of this.

And indeed, they found evidence that aiming for a foreign market. Allowed the carpet manufacturers to upgrade, and led to production improvements. So that's an example where it's not with the conclusion let's subsidize exports of carpets. They were learning something more fundamental about-- and this is one study. I think there's more work to be done. But it is shedding light on the debate in much broader policy debate. In this case one involving international trade and perhaps on macroeconomic policy.

AUDIENCE: My name is Aban Panjwayi. I'm a senior in the college studying economics. Thank you so much, Professor Kremer for your time. Your work is truly an inspiration to millions across the world. My question is, what are some open questions in development economics. That you believe the next generation of both academics and practitioners, will work on and solve.

MICHAEL KREMER: One thing that economists have been pretty shy about working on is issues of culture. And I think our tendency is to assume people are people everywhere. And there aren't that-- we can just look at the fundamental economic forces of supply and demand. Or things from game theory, they're going to work the same everywhere.

There's exciting work going on in economic history. And more broadly to try to understand the role of culture. So my colleague Nathan Nunn does a lot of that work, Alberta Cena. And that often obviously involves bringing in insights from other disciplines. And I think that's an exciting area and one of many exciting areas.

I think it's been fantastic that-- I'm obviously very excited about the

movement towards randomized trials. But economic theory is very important. And work on economic theory or the macroeconomics of development are also very important areas. And I hope there'll be more work in those areas as well.

AUDIENCE: Hi I'm Amy Yee I'm the journalist who wrote about your work in the New York Times. I wrote about the deworming campaign in Delhi, and I got to see this happening on the ground. It's the most awesome thing to see children lined up to get the deworming pills, so it was awesome. So I'm at the Kennedy School this year, which is great.

So a couple things economics, RCTs, now it's very glamorous, especially with the Nobel Prize. What's not so glamorous are the people on the ground, the NGO workers. Who work really hard to implement these programs, really difficult to do this work. So one question is what do you think could make their work easier on the ground?

The other question is that sometimes these programs don't persist. So I don't know the status of that program in Delhi. And a lot of that depends on the political institutions, and who's and who's in politics at the time. So I don't know if the program in Kenya is still going on, if it is that's wonderful. But can you say something about how to keep these programs going? And to get politicians and policymakers to institutionalize these programs.

MICHAEL KREMER: Let me take the second part of that first. So well give a little bit background, good to connect again, by the way. As I mentioned I was involved in the study of the effects of deworming. We have bigger impacts, we found huge price sensitivity. Took those results went to the permanent secretary.

That's at the top of civil servant the Kenyan system the government. Or any system of government, in most countries there's a top civil servant who's there permanently. Even though the ministers would change, so there's many more. It's not like the US government where there's a ton of political appointments at the top.

So met with the permanent secretary explained some of the results. He was very excited about it. He really wanted to move forward with this. But they weren't able to move forward with it. Between that year and the next time I was in Kenya, and that went on for a while. Why was that? Well that was because if your permanent Secretary of the Ministry of Education, you have a lot of issues to deal with.

And there were teacher strikes, there were other terrible crises in Kenya in education. And Ministry of Education had a thin staff. At the top of it was excellent, but that wasn't necessarily the case all the way through. And so was able just to give a sense of the process of how-- Realized that there was a process of trying to work with the government.

And to facilitate progress on this. And that was partly working with civil servants, and technical assists. So some of the NGO workers who had successfully done this at the NGO. Were seconded to go work with the Ministry. And they talked about how the local program was done. And worked out many, many details of non climbers work, exactly as you said. Of what's necessary to scale this up.

And that included everything, maybe some bits were glamorous. There were non-glamorous bits like figuring out which parts of Kenya had worms, and which didn't. By going through a bunch of studies and then matching those up to the changed district boundaries. So you could figure out how many pills you're going to need. What the logistics of the training was going to be.

Prepared a document for the finance section of the Ministry of Education, that they can consider. and if they like it then they'll pass it up to the finance ministry. And then they'll be considered through the Kenyan government process. I see Asim nodding his head, because I've done work with the US government as well. It's the same thing there's a bunch of steps that have to be undertaken, before something can become a reality.

So there's also getting political support for those. And so I was able to meet with some senior Kenyan politicians. Including the prime minister at the time, Raila Odinga. But also with ministers who came from different political parties, but were willing to come together. Kenya's got amount of coalition politics, so these were rival parties. Even though they were both part of the government.

But they came together and they made a joint announcement about this. So the combination of the civil servants and the ministers. That led to change and Kenya implemented a national program. And I should say they implemented that program, and the donors supported it by the way. But then it was part of a larger program, there was corruption scandal in parts of the larger program. Everything shut down again.

Again I'm seeing Asim smiling, that's part of policy. But then it took a lot more work and then they were able to launch that program. Launch the deworming component of the program again. In India we also did a lot of work to try to get the results out there. Not just in journals, but in the World Bank World Development Report, for example. In the US economic report of the president.

So part of this is was a result of that. Indian state government started exploring this as well. And Kenyans who had worked in the Kenyan program. Traveled to India to explain how they set it up and how it could be done. And the Indian state governments worked out implementation approaches. And then one state launched in India, then other states followed. And then the national government in India followed.

And let me emphasize neither I don't want to claim that either the current Kenyan government or the current Indian government. That they're immune to political considerations, maybe that's the way to put it. But that doesn't mean that they're not going-- if there's something that is very inexpensive. That they can do, that's going to be-- maybe it's going to actually get them some votes. That they want to go ahead and do that.

And in each case, I'm very happy to say, both of these programs are continuing. And I think more than 150 million kids are getting treatment for worms. Every year as a result of just those two programs. So that's maybe an example of you can learn some broad scientific lessons, and that's really important. And that can shape our general

understanding of policy.

But there are also particular programs and they prove effective. Even if you don't have complete wholesale political nirvana. Often politicians and civil servants can make this happen and endure. Oh yes as I mentioned earlier, this is not the type of research that can be done by an individual, or a few individuals. It requires huge teams of people working together, and that very much.

And it also requires-- you mentioned NGO workers. I'll talk both about NGOs workers and about survey enumerators. NGO workers are often doing very difficult jobs. We're starting to actually there's now some RCTs on what motivates them. And how to get better performance and maybe others can talk about that. But the work that they do it's very important. And I particularly, would like to recognize some of the NGO that I worked with, ICS.

There was a brave decision in some ways, by the leader of that NGO at the time in Schepp Barry. To say, hey, I don't actually know all the answers. So I'm not going to assume that I've got the right answer. I'm actually going to try different things and try to learn about what works. And that decision has had big consequences.

The other group is the survey of enumerators. And anybody who's worked in this area know you don't write a survey and then just implement it. You'll be asking all the wrong questions, you'd learn a lot from your survey enumerators, as a researcher. And that goes into producing much better research. And one side comment, but I guess I get to make these comments now.

In medicine papers have 27 authors on them, in economics we're used to three authors. Maybe we should be following medicine and more broadly recognize the many people who go into any paper.

AUDIENCE: Thank you, Michael, and this is following up. Just now you describe the collaboration and communication between researchers, and policymakers, and local governments. So I'm a doctoral student from Graduate School of Education.

As of social science researcher sometimes we bump into a situation where researchers are very enthusiastic. Whereas the policymakers and the real world are not that enthusiastic. However, we do believe the importance of our topic. So would you mind sharing some experiences? Not only as a established a researcher.

But also your experience developing from a junior researcher to an established scholar. What the challenges and experiences, if you could share with us. How to convey the messages from the research side. And how to build the collaboration like you described. With policymakers especially under current political climate.

MICHAEL KREMER: There's I think a process of dialogue between the researchers and policy makers is very important. Often researchers junior or senior they get very enthusiastic about things. And policymakers probably, correctly, often want to be cautious. I understand that LLC they're now teaching a course for graduate students. On how to improve their behavior when interacting with policymakers.

And I think that's very valuable. You can't go in like you know everything, for two reasons. One, you're going to offend people if you do that, and two you don't know everything. And the policy makers probably do know something. And so you'll get good ideas from that. If I think about my own experience, I'll just say a more recent story. I'm doing some work in Columbia, and talking to the policymakers there in their student testing agency.

And they're very concerned because a lot of the same problems in the US. There's a rapid growth of for profit higher education, and a very heterogeneous quality. And a lot of students they're choosing without a lot of information. And the first and they're the first in their family to go on to higher education. I'm not talking about making the wrong decision between brown and Michigan.

I'm talking about their choosing between should I be an X-ray type-- those people who are choosing between brown and Michigan. They're not perfectly informed but they've got a lot of information. The people who are choosing whether to be an X-ray technician or a dental technician. They often have very little information to go by. And there are people who are setting up schools to rip them off.

And trying to provide some of that information is very important. And this is something that the National Student Loan Agency in Columbia is very, very concerned about. And obviously the student loan agency is very concerned about it, because they don't want the bad debt. But the student testing agency and the Ministry of Education. So I recently went to Columbia a few years ago and I went trying to talk about some research that I've done on school choice.

And found that the policymakers wanted to talk about this other problem. And realized that was a very important problem. And one where we could potentially make progress. So I'm hoping that we will the policies actually-- we're talked and the policies moving ahead. And I'm hoping that we'll be able to do some exciting research as well.

They're doing some very exciting things there that I hope will be useful for millions of students in Columbia. But I think we could have maybe could eventually lead to by other countries. I think by the way, let me just say. I think the whole area and the approach uses technology and chat bots, to try to communicate with students. And I think the whole idea of digital development.

Obviously there's going to be a lot of failures as well. But I'm also working on providing information digitally on agriculture. Through an NGO I helped found with Sean Cole. Who at the business school as well as a number of-- Dan Biocker-Green who was a grad student here. Brown and has come from a business background. But we're working now with governments in multiple countries.

And reaching millions of farmers with new ways of trying to convey agricultural information. So those are two examples. But I think there's a-- people are asking for what are areas for the future. I guess I focused on the more academic side of it. But from the policy side and from the research side. There's a lot that can be done in digital development and lots of opportunities there.

AUDIENCE: Hi Professor Michael this is a question from the second floor. So could you elaborate a bit more for the potential for the cross disciplinary collaboration. In the International Development field, especially for public health and economics. Because RCTs idea is originated from medicine, and as you said behavioral economics.

And the public health the fields have already shared a lot of works. And what might be the open topics or opportunity window for the cross disciplinary collaboration in those two field. I have this question because I'm a grad student in School of Public Health and I'm interested in this topic. Thank you.

MICHAEL KREMER: So I think there's all sorts of exciting opportunities. Asim was referring to global public goods. I don't know what I would say this is a global public good. In my spare time one of the things I've been involved in. Is helping the US Agency for International Development create a unit called development innovation ventures, which supports innovation.

And I think is one of the things I've been arguing today. Is that this area as one a lot of the work is about innovation. And so Development Innovation Ventures is focused on innovation. So what development innovation ventures does. Is it takes applications, it could be from a social entrepreneur. Could be from a mayor somewhere, it could be from a researcher, for innovative ideas.

And it has some funds to pilot the idea. Because before you can go to RCT, you want to pilot the idea. If the pilot works out, you can go to a second stage of rigorous testing. And then for the most successful things, there's greater funds available. To try to transition things to scale.

So I'll just give an example of one thing in the public health area, that I think really is interdisciplinary. This actually came from a couple of researchers in Georgetown. James Habyarimana, who is a student here, and Billy Jack. James is from East Africa, and if you spend time in East Africa you'll know that there's lots of mini buses.

And they drive like crazy because the drivers pay a fixed rent to the owner. And they don't make that much, they lose money at the end of the day. And if they make \$7 more they go home with \$7. So the idea was just to put a sticker in the minibus, for the passengers to see. Saying if you're feeling uncomfortable slow down.

And that sounds like a somewhat crazy idea. But DIV provided a small grant to them. They actually came in with some evidence. They had done a small RCT, had a few problems, but they'd done an RCT. It looked very promising, the results. They then went back and redid things very carefully. In the iterative approach that's key to innovation.

And then they were able to demonstrate and they actually published their economist. But they published in proceedings of National Academy of Sciences. With data from insurance companies, showing a huge reduction in accidents. They then applied for a further grant, to help them transition this to scale. And for that, they worked with the insurance companies on the one hand. So for private sector element to the scaling.

And the insurance companies wanted the minibuses to be safer. So the largest insurance company in Kenya signed up. And then on the other hand, with the government. So that when people renew their licenses they had to put in stickers. So this is one example but there are many, of things that DIV has supported, which have scaled up.

And I think several of them are actually health examples on. So I don't want to predict what the next idea is. Precisely because that's not an idea. If you ask me about that idea I would have said it's not going to work. Luckily it did work and I think that trying to have resources and DIV does this.

To try out new ideas, to rigorously test them. And then to take the most successful to scale. I think that's a very powerful approach. And I think that there are probably lots of examples in health. And lots of examples trying to bring in ideas from multiple disciplines.

AUDIENCE: Dr. Kremer thank you for being with us tonight, down here. I'm a student at the Harvard Kennedy School. And I'm interested in the relationship between research and program implementation. In particular your work has shown us that randomized control trials have tremendous capacity.

To give us insights about very complex behaviors and decision making. But as we've seen this methodology applied in different contexts, to different challenges. It's also clear that randomized controlled trials are expensive to run. Logistically challenging to coordinate, and the most effective ones take a long time to measure results longitudinally.

My question to you is, how can we convince skeptical policymakers that this diagnostic effort is worth it. When so many policymakers are under pressure to deliver results quickly and at scale. So I think that.

MICHAEL KREMER: I think that often they are expensive and difficult to run. But sometimes they're not always expensive and difficult to run. So for example in this agricultural context if you want to see what the impact is on yields. You're going to have to wait season, no question. But if you can also do A/B tests maybe it's an interactive voice system.

And your measure which you can get very, very quickly. Is you try one approach, you try another, do people drop off the line immediately or do they stay on the line? So that's almost the opposite. It's like the type of A/B tests that Google or Facebook would do. Where they get results very quickly and very cheaply. But I take your broader point.

I think that when a policy-- I think a key thing to recognize. Is that when a policymaker decides and some of them aren't going to want to do it, some of them will. But when a policymaker says I'm going to give this a try. They're creating knowledge not just for themselves, but much more broadly for the world as a whole. And I think why Asim used the phrase, global public good.

And that means that we should, in terms of the way we set up our institutions. And one of the reasons why I mentioned Development Innovation Ventures. Is you shouldn't expect the policymaker-- if a policymaker in Ghana says, I want to try this. To expect Ghana, let's say this is an expensive RCT. By the way, just to be clear this

say this is an expensive RCT. By the way, just to be clear this expensive RCT might \$1 million, that would be on the higher. Many of these are a couple of hundred thousand dollars.

If you talking about affecting billions of dollars of spending, this is small relative to that. But it might be a lot relative to what that policymaker can easily do. So having some central source of funds that's available. Like from DIV, but World Bank or Difford or many other organizations. Are now starting to provide that. I think that can be very valuable, I think there's a lot of scope for more of these innovations, social innovation funds.

And certainly when we did a study looking at the social rate of return to the investments. In Development Innovation Ventures what the US taxpayers put into it. We're finding benefits five times as great as the cost. And that's a super conservative methodology. So I think this is an area where governments and philanthropists they're trying to encourage more experimentation like this. In support of those policymakers who are more willing to undertake it is very valuable.

ASIM KHWAJA: So and we know we're supposed to end at 6:30, so we're hitting pretty close to that time. What we'll do is we'll take two more questions from the audience. And then I requested Michael if he could stay around for another 10 minutes or so.

You guys can walk up if you want to question which you didn't feel like asking publicly, you can ask him. I know he has to go to dinner so I'm respectful of his time as well. But why don't we take two questions from the audience, and in another 10 minutes.

AUDIENCE: Thank you for your time professor. My name is a Athish and I'm a junior studying economics. My question is about RCTs as a method. It seems pretty clear and convincing that RCTs are an extremely powerful tool in answering the question does x equals y . But could you elaborate on how our cities can be helpful in actually understanding the black box of why x causes y .

MICHAEL KREMER: Sure very much so. And I think a lot of research is doing that and is aimed at that. So often when you find that initial results, then you can design further work to try to elucidate the mechanisms. And that's very standard right now, let me think of-- I'll give an example. This is work that was done on--

Let me give an example from education and talk about how much talked about textbooks already. But we found that they had a program in which textbooks were provided. And we found that those were not, this was in Kenya where I taught. And we found that the average test scores in school didn't go up. And we stared at the results for a while, and I was actually pretty shocked.

Because even people who are skeptics about more resources for education. Typically think that textbooks are going to make a difference. And then having spent time teaching in Kenya, suddenly the light bulb went off. And I thought, hey these kids are learning in their third language. Schools taught in English once you get to even pass the first few grades in Kenya.

That's most kids' third language. They're missing a lot of school from

worms then from malaria, from the parents having HIV. And needing to take care of their siblings and so on. The teachers are missing school often, it's very easy to fall behind. And then once you fall behind, the curriculum is just marching ahead.

And Kenyan schools are very much focused like many developing country schools. It's not No Child Left Behind, it's we care about our star performers. And we want the person who is going to make it to University, and come back with a good job. And that's the goal, the entire education system is structured around that.

So if you fall behind it's very hard to catch up. So I said that was a hypothesis that maybe most kids weren't in a position. Where they could necessarily benefit for the textbooks. So how do you? First thing we did, was we looked within our data. And we saw that the kids who scored well on the pretest, they did not see an impact.

But the kids who are in the top quartile of the distribution, saw actually a big benefit. Suggesting the kids at the top of the distribution could benefit from the textbooks. Some effected the second quartile, but when you average it all together you weren't seeing much.

So that was a clue about what the mechanism that was driving this apparently mysterious result, the textbooks aren't making a difference. Now this was a place where there was one textbook for every 14 students, I believe, at the beginning. So then that was one step, but then additional work was done. So Esther Duflo, and Abhijit Banerjee and Sean Cole, and Lay Linden did work in India, on remedial education.

And what they found was that kids who are falling behind, can actually be brought up. Quite quickly to a point where they can then benefit from the class instruction overall. So that's not more your test of that hypothesis further. And since then there's been a bunch of work done on trying to actually have adaptive instruction.

For example, with technology, to see if by adopting targeted instruction to where the kid is, can have an impact. So we're seeing development over time to understand the why. And to use that understanding to improve people's opportunities, so they can fulfill their potential.

AUDIENCE: Thank you so much, good evening professor. My question is as much as this new field of economic understanding RCTs brings to the frame. And more and prickled us scientific and effective method of understanding. What enables growth in the poor nations. The question the problem of accountability seem to persist.

Is the impetus of alleviation of global poverty relies on the shoulders of selfless economists, or maybe careerists, globalists. Or is another way of ensuring accountability on these developments programs possible? Especially of the accounts of impoverish themselves.

MICHAEL KREMER: So again, let me be clear. I think economists are going to have-- I hope we can have a useful role. But I don't want to claim it's more than that. Much like any other professional, a doctor can be useful for their patients, but they're not going to-- and a medical researcher may make progress on a particular issue. But there are much larger issues that are--

People ask me who's made the most difference to world poverty? It's clearly, I mean in my view, it's very Deng Xiaoping. Because the fundamental reforms to China that are Deng Xiaoping introduced led hundreds of millions of people out of poverty. So but that said, you raised the issue of accountability. This is an area where, again, turns out I would never-- I started doing work on textbooks.

I shy away from super politically controversial topics, but other people aren't so shy. And there's really exciting work on accountability that very small, again-- there are big problems of accountability that go-- I don't want to claim that the research is going to save the world here. But I'll give an example, Indonesia has a big anti-poverty program.

Provides a lot of benefits to Indonesian citizens in poverty. But a bunch of it doesn't get through because of corruption. And so there are people in the Indonesian government who are sincerely concerned about this problem. They talked to researchers, including Rema Hanna here at Harvard and Ben Olken at MIT. And what they decided to try was let's let the people who are entitled to receive these anti-poverty benefits.

Let's let them know about it. And they saw that led to a 30% increase in the amount of food that was getting to people. So that's a huge, that very simple stuff and improved accountability. There are other examples that go even more deeply. There's work in which voters were provided with a simple report cards.

Giving them some information or-- graduate student recently did work where to let citizens in India. Know about the criminal records of people running for office. That led to substantial changes and substantial improvements. There's work even on what's the impact of debates. So in some constituencies, in Sierra Leone an NGO sponsored debates.

And we saw that led to more people voting across ethnic lines. So there's a lot of interesting work on exactly this issue of accountability. And some of it I think, I'll give another DIV example, there was a study to try to reduce some types of fraud in counting of votes.

And that was scaled up by actually one of the political campaigns in Afghanistan. And that reached millions of people. So there's exciting research going on in this. And I think it's actually improving, it is improving accountability already.

ASIM KHWAJA: Great so with that I think sadly we'll have the discourse keep going on. But each time I listen to Michael, I learn something new. I did want to mention two observations, just to reiterate what Michael said. And I think it's important to recognize this year's Nobel really celebrates those two things. And Michael said this, but I want to emphasize them again.

The first is this idea of research meets practice. When I first came as a junior faculty member. They used to be this idea that you could either be a researcher or you could be a practitioner. I'm at the Kennedy School and so this dichotomy was always there. And I think what this Nobel has done in many ways for our field. But I hope for

development in general.

Has made it clear that you don't have to have this trade off. You don't have to have to have this dichotomy. You could actually be very much in the world of practice, as Michael is. And at the same time, reach the highest honor that an academic can, which Michael has. And so that's something deeper than just our RCTs or not RCTs, or anything like that.

And I think that's really worth thinking about, emphasizing. And also for you students out there, you don't have to face this trade off versus tension. You may have different weights in these two areas. But the fact that they can coexist. And not just called exist, but complement and support each other, is a really powerful message.

The second message which I think is equally as important. This is something when you hear Michael, you hear Esther, you hear Abhijit. It in fact, you should have heard, when Michael talks about his work. What I find fascinating is how many times he's talking about other people's work. This idea that what we're producing is not a singular production.

It's not some person in an office having this brilliant epiphany. Which a lot of Nobels are, and hats off to them. But this is a collective production by a whole bunch of people. Some of whom are in the room standing up here, some are in the audience. And some are frankly and most in fact are out there in the world in places that we really, truly, deeply care about.

And want to make sure we recognize that aspect of this joint production. Because that's another message as we produce work, about how we think about that work. And Michael said this earlier, the fact that when we approach individuals, and you know Harvard is a place of privilege. You can think of that privilege as let's pat on our back, each person on our backs. Or you can think of this as a point of responsibility.

As something you can take to the world. But when you take it to the world, Michael said this earlier as well. Is you go with a sense of humility, and a sense of learning from the world, not just teaching the world. And that's a critical thing. And again in all of these are things that I notice about Michael when he speaks. And I can't help reiterating them in case, I'm sure you pick them up but I want to emphasize these things are critical.

As you leave, like I said we have another 10 minutes or so, feel free to come up say hi to Michael. The other thing I'd request you to do is I'm struck listening to all of you. Which as I said introduced from where you are. We've had a range of people in this room. The one thing which is worth doing is turn around.

And look at the person who you don't know and you didn't come up with as you leave, and just ask them what they're doing. You would be amazed at the wealth of knowledge, and expertise, and experience here. And you should at least, if we believe in the message we're talking about co-learning from each other. Take that opportunity co-learning from each other as you exit. Thank you so much for being here, it's a real honor, thank you Michael.

MICHAEL. KREMER. Thank you very much

MICHAEL KEENER. THANK YOU VERY MUCH.

[MUSIC PLAYING]