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[RUSTLING]

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PROFESSOR: Welcome, everyone. So last time, we left it at saying, well, one fast way in which you could test whether or not there is a potential for a poverty trap or for multiple steady state, which is typically interpreted as poverty trap, is think about the mechanism that would lead to a mapping between income today and income tomorrow or between asset today and asset tomorrow. And then look at whether this mapping is sufficiently strong. So that's the exercise we did at the end of last time.

When we're looking about the effect of income on nutrition, we spend most of the time on that. And I asserted that the effect of nutrition on productivity and therefore future income is there, but not so strong that it would outweigh the fact that the effect of income of-- the elasticity of nutrition with respect to income is not one. So we left it as thinking that maybe the nutrition-based poverty trap, as described as the fundamental phenomenon of Dasgupta and Ray, is not there.

But nonetheless, my response to I think a question by Recca, I pointed out that this is a paper that everybody reads. This is like a modern classic. And so people read it not so much because you necessarily believed in that very mechanism, but because the whole idea of poverty trap based on this funky mapping between income or asset to the optimal always is something that we keep finding in different areas of the field.

Now, so then you could go to another approach, which is what we're going to do today, is to say, well, irrespective of the mechanism, is it just there? Can we find a setting where we can find a relationship between today's assets and assets next year, for example, where there is not only that nonlinearity, but that crossing of the 45-degree line from below, which ensure that there is an unstable, steady state and two steady state, one low, one high.

Another thing is, of course, no matter what we think as economists sitting in these rooms, there is also the policy-making world, the activist world, the NGO world. And some aspect some part of this world believes in poverty trap and has, in fact, created programs that responds to the idea that there might be a poverty trap.

And the two ideas met together nicely in the context of the ultra poor programs, which are very much a program inspired by the idea that there might be a poverty trap, which were designed and rolled out in the last few years, and which have given an opportunity to actually directly present evidence for against a reduced form view of, is there a poverty trap? So that's what we're going to do today.

So the idea that there might be a poverty trap is at the heart of a relatively recent set of antipoverty programs, poverty-fighting programs, promoted very much from the South itself, called a graduation approach, graduation as in graduating out of poverty. And so the idea is that you are undergraduate in poverty, and then you graduate, and you're out of poverty.

So the graduation approach was designed by a microfinance organization slash NGO called BRAC in Bangladesh. So unless you're very familiar with development economics, you've surely heard of Grameen and not necessarily of BRAC. But BRAC is actually the bigger and more comprehensive organization of the two. Its founder just passed away a couple of years ago, and was a wonderful, wonderful individual.

And so they have a microcredit program. They also run schools. They run health center. They run antidiarrheal program, et cetera. I'm sure they're doing a lot of COVID. And they've now, in fact, expanded in a lot of other countries in the global South. But they started in Bangladesh.

So their idea, what happened with them is that when they were running the microcredit program, they realized that there is a set of people who are not even interested in getting a microcredit loan. And those people are some of the poorest in the villages.

So they decided that they need to do something to reach out to these people. And they designed a program which they conceived as potentially a first entry into the world of very, very small business for people, and potentially a steppingstone into microcredit or, in general, in the world.

So their idea was that by giving people an infusion of capital as a gift, not as a loan, and helping them start it, you can help them escape poverty in a durable way. So the question for today-- I'll describe the program in a bit more detail-- and the question we'll try to answer today is whether this works, whether there seem to be prima facie evidence for a poverty trap, and how we build such evidence. And then do we know where the poverty trap might be coming from?

So let me cut short to that in case we don't get at the end. And the answer is, not completely. I think we'll have pretty good evidence that it works. It has worked, both in the short run, but also in the long run, which is prima facie evidence of poverty trap, probably because of a phenomenon of escaping some kind of too small assets. But we don't know exactly why. And there is a lot of recent research to try to get into that and into various mechanism.

So how does the graduation approach work in a nutshell? The community is brought together and identified what they think of as the poorest of the poor in the village. So that's what sometimes call as a participatory resource mapping. They all go together.

They rank people. There is a whole exercise with cards involving ranking people by wealth. First half of the people are cut in half by wealth, the richest and the poor. And then the poor are cut again and then cut again and then cut again until you arrive at four or five people, maybe. It depends on the village. But on average, it's four or five people in the village who are considered by their fellow villagers to be the ultra poor.

And when you look at these families, there is often something pretty clear in their history. There are a lot of female-headed households, where the husband died, or the husband is alive but incapacitated somehow. A lot of households like that or other households where there was-- you can almost always identify the tragedy that brought them to an extreme set of poverty.

Typically, a lot of these women-- these are often women, a lot of these. Didn't have to be, depending on the problem, that it's a woman who is targeted. But it's often happen to be women who are begging or working as casual laborer, living maybe on the outskirts of the village, moderately included in the social fabric.

Once these people are identified, they are visited by the worker firm BRAC. Or now it's been diffused in a lot of other countries, as I'll show you in a moment. And they receive income. They receive an asset. It's a productive asset, which they choose from a menu.

But it often happens to be a pair of cows or a few goats. In Peru, Guinea pigs. Bees in Ethiopia, like honeybees.

It could also be a sewing machine. It can be a beginning, like some money to start a small business, to buy the first set of things to sell, that kind of thing. So in principle, there is a menu, and people choose. Although in practice, there is a huge preponderance of cows.

Then they get income support for a few weeks, depending on the assets, depending on the country, depending on the program to make sure that they don't sell-- they don't dispose of the assets too quickly, I think, is the idea is that, OK, we just got them in. So they get a stipend for a few weeks. They get technical support to take care of their assets.

So if it's a cow, the cow get vaccinated. If it's a small business, the employee will go to the market with them to help them buy things, teach them to take the bus, whatever it takes to help them with the activity.

Then there are group meetings, where there is literacy, small health component. They are also encouraged slash forced to have regular savings. So it's a very multifaceted intervention.

And the additional components are so intensive in labor that it roughly doubles the cost of the transfer program. The asset program is about-- the cost of the assets turned out to be $1,000, so $250 PP, depending on the countries. And then that's about double by the human cost.

AUDIENCE: What's the prediction that you want to prove the poverty trap model, as opposed to just giving someone capital increases their income the next period. Is there some key result that we [INAUDIBLE]?

PROFESSOR: Yeah, so the question is, what are the key predictions? So what form a poverty trap, as opposed to-- money is good. Money is better than no money. So let me turn the question back to you, if you don't mind.

AUDIENCE: I guess for the last week, there's super high returns. Got that elasticity, I guess, out of poverty.

PROFESSOR: Yes, so that's one. What's another one that's pretty clear? Because this is a program that lasts-- the entire thing until graduation lasts about 18 months. You get your assets. And you're supported by this group meeting and all that for about 18 months. And then it's shuts down.

AUDIENCE: Poverty trap?

PROFESSOR: Right, exactly. So the idea is that if it's poverty trap, the effect should not diminish. Otherwise, you would see them slowly diminish over the longer term as shocks happen and other people get positive shocks, our guys get negative shocks. We see the effect slowly receding, whereas if it were a poverty trap, and they were permanently pushed out, we would see the effect to persist or even to increase over time. I think that would be a first-order reduction.

And then as well is what you're saying, which is you should see that if it's a poverty trap effect, since it's the same asset for everyone, but some people are even poorer than other, even in this set. So if we have a poverty trap model based on this low return to capital at very level, followed by a steep increase, followed by low again, what would we expect in terms of heterogeneity of asset accumulation post the program or potentially at the originity of program impact? Yes.

AUDIENCE: As for the poorest individuals who receive the transfer, would the poor and those who are closer to what we see as this steeper for transcapitalism?

PROFESSOR: Exactly. So potentially, you would see a divergence, with the program effect being much larger for households that are sufficiently not so poor that the base level of asset plus the asset bring them above the poverty trap.

And we could see that by looking at long-term program effect. We could see that even by looking at the dynamic of the asset accumulation for households that end up post transfer to be above or below. So we have a chance, in a sense, to see that S-curve in action.

AUDIENCE: Rather than just the traditional.

PROFESSOR: Rather than just the traditional, this is increasing because more assets is better than less assets, but declining. So those are the two that we are hunting for today. Of course, first the short-term effect, which are not obvious to start with, and then that. Yes, Aaron, and then--

AUDIENCE: Typically, when this type of program is introduced, and there's this participatory method-- process, does that take place with a knowledge that the results of the process are-- the people who are identified are going to receive this very valuable program? Because we could imagine there could be some kind of capture. Is that taken into account when [INAUDIBLE] designed this?

PROFESSOR: Yeah. That's a great question. So the answer is yes, everybody knows this is what's going on. I mean, you could maintain the surprise maybe the first time. But in any case, that's not sustainable. Or it's visible when someone gets something like that. So the news would go in.

So this really is done with the view that people know when they are doing the exercise that this is going to be used for resources. So that's definitely a concern that people have expressed. And I'm going to ask you to suspend your disbelief until you get to Ben's part of the section, where he's going to talk about targeting.

So in this particular instance, what they do as well is a verification ex-post to check that-- to actually go to the household. And then they actually bookend some of the households that are not poor enough.

Another thing that-- because this was replicated in India. I was involved in the India study. That we found in India is that a lot of people also refused the transfer. In our setting, about half the people refused to transfer. But that was a bit of an Indian-specific thing. So when you visit them, it's like, what are you talking about? I don't want this, which is surprising since it's a lot of money.

So I think the long and short of it, you're going to, again, talk about targeting in a lot of detail because it's a key issue, is that when everything is said and done, these problems of capture and potential manipulation are probably not that big. They are not inexistent, but they are not enormous, less first order than the economist, a knee-jerk reaction happens to be. Yep.

AUDIENCE: I have a twofold question. The first one is going back to last class. We can't have poverty traps without financial frictions.

PROFESSOR: Yeah.

AUDIENCE: Conditional on that, is this a test of financial frictions and/or financial friction can induce poverty traps to join?

PROFESSOR: No, I don't think it's a test of financial friction. And in fact, I think we can almost rule out that financial friction at the heart of this in this setting because this program is run by a microcredit company who is already present in these villages and would like nothing more than to give money to these people. And in fact, the inspiration for this program was that these guys are not willing to go.

They don't have a project. They don't want to do it. So they're not willing or slash able to go.

So I think, in a sense, we can rule out the financial friction with a certain amount of hand waving. This is a market where credit would be available for these people should they want it. So in this setting, that's not what we are testing.

And most of today is going to stay terribly useful, which is I'm going to show you that-- previewing the result-- I'm going to show you that these programs are large effect that are very persistent over time, over 10 years, suggesting that there might be a poverty trap at play. Furthermore, we are going to see that there is, in fact, in this setting, this apparently, this S-shape relationship between capital today and capital tomorrow, which could underlying this persistent effect.

What I'm not going to tell you today is where it's coming from. I'm willing to rule out nutrition because of what we discussed last time. I'm willing to rule out credit markets because of what I just told you. But what it actually is I think is very much open for her for discussion. And it's not going to be solved today.

So this is, at this point, just like-- but already knowing it's there. And then we can go back to mechanism and say, where is it? Where is that map incoming that is so strong to come-- to lead to that, lead to that outcome?

So for this intervention is super expensive, compared to doing almost everything, including giving just cash to people, which is another popular alternative, or including doing other stuff that could directly affect the health or education of these guys. So of course, it's an interesting question, whether or not it is effective, not only in the short term. It's only valuable if that idea of poverty trap is there, and it's, in fact, justified.

The theory of change underlying it is that it's one big push. It's super worth it. It's super expensive, but it is worth it because it's going to be paid off over the lifetime of this person and potentially their children as well.

So the first question is, does it work? So here we're going to take you a small step aside to think about how we even answer this question. For some of you, it's going to be a review. For some of you, it's going to be new material.

But even the review, I find that-- every year, I teach this. And every year, I learn something by just staring at this number. So I hope that even for people for whom it's a review, you're going to find it useful.

So it's a review of what it even means to have an effect and what impact means. So I'll consider our treatment. We want to know whether our particular treatment has an impact. And let's call it W and an outcome Y.

For example, the outcome here is whether you got the ultra poor program is the treatment, and then whether or not you are able to feed yourself, food consumption or anything. Actually, there could be a range of outcomes. And we'll go back to this question is the outcome.

So the way that Imbens and Rubin hold this up is to say, well, the outcome that I'm observing is something that I'm going to call potential outcome if treated, which is opt out only if you happen to be treated. So it's multiplied by this dummy, which is equal to 1 if you're treated.

And then it's your potential outcome untreated multiplied by a dummy, which is 1 if you're untreated, which is 1 minus Wi, which is dummy indicated that you're treated. So everybody is only observed in one of the two situation. But the dummies of treatment shuts things on and off.

And so I'm not-- at least conceptually, I can always define the potential outcome for both situation. Before the automation occurred, you could have been a treated person or a control person. You have a potential outcome for these two, for these two settings.

Before I move further, this might be familiar to a lot of you. But there is an assumption that's buried in there already in the idea that I can write this this way. Can someone tell me what the assumption is buried in there? Yeah, Recca.

AUDIENCE: You assign the treatment to one group that's not effecting the experience of the other group so there's no network effect, for instance.

PROFESSOR: Exactly. So this is what goes under the jargon of Stable Unit Treatment Value Assumption, or SUTVA, which basically says, if the fact that someone else is treated doesn't affect my treatment. And this is the only thing that allows me to write the potential outcome as just my treatment as opposed to my treatment and that of everyone apart.

Otherwise, suppose you had two people. And one is treated, one is nontreated. But the treated person gets upset when the other person is untreated. Then the happiness of the treated person is a function of their untreated status and the treatment status of the other person. So just with two, we already have four potential outcomes, not two.

Again, we are ruling them out here, which, as Recca, I points out, means we are rooting out network effects, for example, the possibility that someone would help someone else. We are ruling out invidious comparison effect, like I just did. It might make me upset that someone else is helped. We are rolling out learning. We are rolling out straight, externality, contagion, this type of things, which become more relevant for medical treatment and that kind of thing.

So we are not ruling out because we think they never exist. We're ruling out because we found out we are ruling out. And then we'll bring them back at some point.

So that's an important thing to say. It becomes hard to tackle if we don't have to SUTVA. So when we don't have SUTVA, we try and model in what way we are affected by the other units.

So if we can write it this way, then there is a well-defined idea. What's the treatment effect? It's my potential outcome if I'm treated versus my potential outcome if I'm not.

Are we able to estimate that thing for any single person? No because we are never observing both. So that's not happening because ex-post, we only observe one of the two potential outcomes-- the individual ends up being treated and ends up not being treated. So we'll never know the individual treatment effect for each individual.

We might get close to that. If we have enough variation in, say, observable characteristics about an individual, and we have sufficiently much data, we can try to get to somewhere in saying, well, the treatment effect for someone who looks like Recca, in terms of being a young woman who is studying economics, who grew up in eight countries, that tends to be that. But we would have to have enough of people like that to be able to say, this is the treatment effect for someone like that. So this, we might be able to do.

So we could be interested in the average treatment effect for the population, which would be the expectation across the globe of the difference of the treatment effects. We could be interested in the one for whom received the treatment. So that's the average treatment effect. That's the treatment on the treated.

And then as the discussion, I just had said we could be interested in the impact of the treatment for those who have some characteristics observed or unobserved. So this becomes still an average treatment effect, but for a person who has some vector or whose characteristics are some vector x.

So when we talk about personalized medicine, machine learning, et cetera, that's really what we are talking about, which is trying to bypass this thing that we cannot do personalized medicine because we only observe the same person ever once, by exploiting the old information we have about them.

So that's things we might want to do. This is all a flavor of average treatment effect. We might want to know how the treatment affects not only the mean across groups, but the distribution of the treatment effects. So that's we are going to get from quantile treatment effect by comparing people, by comparing the quantile. Could be the median. Could be the 90th percentile, the 5th percentile.

We might be interested in the quantile of the treatment effect, which is not the same thing as this because unlike the expectation, which are nicely walking out in both sense, the quantile of a difference is not the difference in the quantile unless you are willing to make another assumption.

So I'm pointing that out. You already heard it. But we forget it all the time because if you add a convenient assumption that the treatment doesn't affect people's ranks, then this start to look very much like that. So under this assumption, which is very strong, then you can get at something that looks more like the quantile of the treatment effect. But we have to remember that unless you're willing to make that assumption, that's not necessarily a very good-- that doesn't work that way.

For example, if you go back to the ultra poor program, it could well be that the program not only affects people's wealth level, consumption, et cetera, but also their rank in where they stand in the distribution. There is nothing that assures us that we have this rank and variance assumption.

So now, suppose we have that. We go back to the simpler problem of trying to know what's the average treatment effect. And we have a group of treated people and a group of control people.

So, well, the best we can do is to start by looking at the difference between the mean. We would like the difference between the mean, which we replace by the sample average. So that's the expectation of Yi treated for the treated minus the expectation of Yi control for the control.

I'm going to add and subtract something, an object that is not observed but conceptually exist, which is the average of the untreated outcome for the treated population. So I'm subtracting it here. And then I'm adding it in the part that is missing.

So what does it give me? Well, now that I have that, I can just rearrange a little bit. And I'm getting the expectation of Yi1 minus Yi0 for the treated. So what is this? What's that quantity? Is it an object that we are potentially interested? Yep.

AUDIENCE: It's the average treatment effect on people who were treated.

PROFESSOR: So treatment effect on the treated. That's great. But then we have that. And what's that difference?

AUDIENCE: Whatever differences already existed between the people who were selected for treatment.

PROFESSOR: Exactly. So it's the potential-- rather than already exists, that would have existed anyways or something in the absence of the treatment because it could be captured by a baseline, or it couldn't be. It's not necessarily a temporal thing. But it's saying that in term of potential outcome, these two groups are potentially already very different.

So take within the village, if I run the ultra poor program, and I run my participatory resource exercise. And they identify 10 people. And then I go, and I survey these 10 people. And finally, the survey staff decides they're only going to take 5 out of the 10 people because the other one don't quite make the cut.

Someone could say-- and someone has said and done a first analysis of this program by saying, let me compare the five people who got in the program to the five people who didn't get in. The five people who didn't get in were presumably quite poor because they were selected by their fellow villagers. So if I still find a difference between the two, it must be the fault of the poor.

Of course, what we don't know is whether there is something different about them that means that the staff of the NGO decided not to take them. It could be observable, or it could be unobservable. And that's the problem we have. So that's the selection problem.

So basically, we have three cases. Either we are in the best case scenario from the point of view of this problem, which is that the program was randomly assigned.

So for example, a frequent thing in the ultra-poor study, there are two kind of random assignment. Either the villages were randomly selected, or the villages we are not randomly selected. The identification was done. And then within the people who were identified, people were or were not selected randomly within qualified people. In that case, there is nothing systematically different about the people who got selected once by lot, once I do all of this thing.

So in that case, the selection should be 0. And I should get an unbiased effect estimate of the effect of the treatment on the treated. That's what we are going to study today.

Another setting is one where it's not a known function of covariates. But I'm willing to say that once I know everything about them, enough things about them, something happened, which means that some people got treated, and some people didn't. And I'm willing to say that conditional on the caveat that I observe, despite my not knowing exactly how the sausage was made, I am willing to make the assumption that the potential outcomes are independent. The two potential outcomes conditional on the x are independent of the treatment status.

And that leads you to-- that would make you confident to try and control adequately for these different observables. As long as you control fully for this set of x, even though you don't really know why someone with this particular characteristic was treated, another was not, you are reasonably, sufficiently satisfied.

So if you do that, for example, that's the example I gave you with this idea is that conditional had been selected by the people. You're willing to say that you don't know exactly how the staff chose. But whatever they did, that's as good as on them.

Then if I'm able to control fully in this case, that would be easy because I have the selected and the nonselected people, I can control for that status, and within that, proceed almost as things had been randomly assigned, even though they weren't. And I know they weren't.

So that's, of course, the standard justification for a lot of [INAUDIBLE] regression, which adds on top of that a bunch of functional form assumption; matching regression, which adds less strong functional form assumption; or sometimes no functional form assumption. For example, if the number of cells that you have of excess is not that large, and the number of observations is very large, then you can calculate the difference between treatment and control in each cell. And then take the weighted average of this cell. And there you go with your treatment effect. All your assumptions are correct.

So Josh, for example, has a very well-known paper on the effect of the military service, where he's saying, I'm controlling for everything that could potentially enter in the decision in why someone does the military service within the cells. I'm looking at each cell. I'm looking at different between has done it versus has not done it. And then I'm reaggregating. So that's a matching estimate.

And you have different type of matching estimates that are more or less parametric, and therefore more or less believable. But the underlying assumption is always this of unconfoundedness once you know enough.

And then third method with the same idea is a double machine learning estimators, double post-lasso, that Victor Chernozhukov and Belloni developed and has become quite popular in the applied literature, and the more generic double machine learning method that Whitney, Victor, me, and a bunch of other people worked on. And that is also quite nice. But it's conditional on this assumption. So it adds bells and whistles and sophistication in terms of the econometrics analysis. But at the end of the day, that's always the same thing, which is not really a testable assumption in most cases.

So this is your observation-- observer-- I would call it without prejudice no research design strategy because my strategy is no strategy.

And then there is a strategy where you're saying, well, you're worried that the potential outcome depends on the assignment. You're not willing to make the assumption that there is a set of x that solves it. Then they have to do something to try and find a corner of the world who are not subject to this. And that's where you get to the research design strategy.

So we'll spend a lot of the class looking at examples of that difference-in-differences one, regression discontinuity design, and instrumental variable. A special kind of instrumented variable is one where you have a latent variable that might be randomly assigned itself. And you use that to form an instrument for the treatment that you're actually interested in.

So there is no Wi that is randomly assigned or as good as randomly assigned. But there is a Zi that is potentially randomly assigned, affects Wi, and unless you're willing to make some additional assumption to recover the effect of the Wi. So that's what we are going to-- short preview of how the different strategy that we are going to see in the course of the semester line up.

So today we are starting with the easiest one, at least the conceptually easiest one, which is random assignment, which, so by definition, random assignments solve the selection bias in your sample because it's a known assignment, known assignment rule, which is either fully randomized or randomized conditional unbiased, but randomized anyway, so you can compare things directly without worry about the selection bias.

That said, that's still not like a panacea. There are issues with randomized evaluation that many people have mentioned. And we are going to keep seeing those. And we're going to see some of this today. What are some of the things that people worry about in terms of validity of results in the case of randomized evaluations? Recca.

AUDIENCE: You can conduct an RCT in a specific setting. But you might think that if you wanted to apply that within a different country, for instance, that so would we have that effect elsewhere?

PROFESSOR: Exactly. So that's the first issue. And in fact, one that we're going to talk today is the extent to which the results are context dependent and would be heterogeneous from context to context. And therefore, the result of one is predictive or not of what would happen in another setting. So that's the first concern that's definitely present. What are other concerns people have? Yeah.

AUDIENCE: When people don't behave following the assignment that we're giving and treat it as if they were controls. And controls act as if they were treated. For example, if you give them a pill, I can take the pill, or I can decide not to take it.

PROFESSOR: Right. So there are various noncompliance issue, for example, the one I was talking about in the case of the ultra poor, which is some people just say no to the program. Alternatively, some people might manage to sneak in, which means that your actual treatment is not randomly assigned anymore. So then you have to deal with that, that you lose the world of perfect [INAUDIBLE] Yep.

AUDIENCE: Following up on the first comments, there's also a more realistic issue here. In the realm of heterogeneous treatment effects, the ability to run an RCT may depend on people knowing that, oh, they are in the right tail of the treatment effects. People say, I'm incapable of running an experiment in a village. Maybe that's because the governors in that village are aware of the fact that, oh, we are actually on the right tail of the distribution. It would be very good for us. So it's not only that the treatment effect in that village is not predictable of others, it's that the the treatment in the village is the highest possible treatment effect compared to the--

PROFESSOR: Right. Excellent. So that's what Heckman called the randomization bias, which is that not only is it originating in the world. So the results of one experiment may or may not generalize.

But there is something specific about randomized controlled trial that may lead you to have, for example, higher treatment effect. And one of them is site selection, which is someone, either the researcher because they are willing to-- they would like to publish a paper-- or the partners because they would like to look good, will pick the sites where the treatment effect are the highest. So you will tend to be biased in that direction.

So there is a very nice paper by Hunt Alcott in environment which shows that the first experiment by the firm Opower, which is one where you get this little leaflet about your energy consumption. And it tells you how you compare with your neighbors. So you got them? We always look terrible. When you guys lived in a house, we were terrible plus, even worse than usual. So I wanted to tell them, but there are six of them.

So in any case, you receive these things. And it tells you, you read, please reduce your consumption. Or it doesn't say that. It just say-- so it finds pretty large effect in the first site. And then the experiments got replicated across hundreds of sites.

And then what you see is that the results in the next 95 sites are actually much lower than the results in the first few sites. And that could be exactly because of that, because the first sites were selected to be places where maybe people would be believed-- where people believe that it would have the biggest impact. So those are some of the issues.

Another issue is that when you have many, many outcomes, not everything can be really presented in a people. People have limited attention. So there is a risk of cherry picking for very many outcomes. In the case of a program like the ultra poor, you have that issue in droves because it's not that you have one thing that you really want to follow. You want to see whether it improves their lives, which is measured in many, many ways. And so how do you deal with many outcomes?

So I think these are some of the-- and then, of course, uncertainty. We mentioned compliance. But there is also just the sample size is not enormous. So they are not infinite. And therefore, you have potentially power issues, which I think you'll spend some time on that.

And a criticism against experiment is that they are generally too small. So their power is too low. And they can only get effects if the-- get results that are significant if the effects are very large, which might lead people to miss some effect.

So we discussed that, spillovers. Didn't come up, but it came up when we were discussing the SUTVA assumption. Cherry picking, and external validity, this will be with us. These are issues that are serious, that are real, and therefore that need to be addressed for each people.

Now go back to graduation. And there are two recent papers. I wrote two studies. But in reality, the second paper is like six studies, and showing a remarkable effect of this program.

The first one, by Oriana Bandiera, Robin Burgess, Imran Rasul, and other, looked directly at the BRAC pogrom in Bangladesh, the mothers of all program. And then there was a follow up. It was published before because it was published in *Science.* But in reality, this came after in terms of running the experiment by a bunch of people, including Abhijit and me, looking at a very similar intervention in six countries, which we have followed first for three years.

And then in some of the sample, and I'm going to show you the data from for India, they were followed for another, until 10 years. They have been followed for 10 years. And these guys also, they lost the control after four years. But they continue to treat, to follow the x control and the x treatment until 10 years. In India, we never lost the control. We continue to follow them. So that gives you some--

So let's look a bit at the results first and then discuss about some of the features of this experiment, which address some extent some of the concerns that you guys might have. So these are the results for the Bangladesh study.

The Bangladesh study was randomized at the geographic area level. So first, they decided where to go. And then they did the whole intervention. And they treated everyone who emerged from the process. So it's a good situation in terms of the spillover situation.

Most of the studies in the *Science* paper were randomized at the individual level within village, although a couple were randomized at both levels. There was randomization first at the site level, and then within site, at the individual level. So what's the nice thing of having to randomize first at the site level and then individually within site? What does it allow to check? [INAUDIBLE]

AUDIENCE: Whether there are spillovers.

PROFESSOR: Exactly. So you can directly check for the spillovers because you can look at the untreated people in the treated villages. And you can see whether their outcomes are any different. So long and short of that, and that's why I'm going to treat all of these studies the same now, is that there is no evidence of spillover in this case. Doesn't mean that they don't exist in general. They're just not present in this case.

So here is the result from the Bangladesh studies. So these are some of the few outcomes that are used-- below poverty line, consumption expenditure per capita or per adult equivalent, value of household assets, savings, whether or not they get loans, whether or not they give loans.

Each row is a treatment effect, with the standard error of the treatment effect and stars to indicate significant. So that's for the first two lines. So each row in the first two line and first column is from a different analysis. That makes sense? We are every time comparing treatment versus control.

So for example, if I read this, can someone-- Vishen, can you read that-- construct a sentence with that number, with that--

AUDIENCE: So four years after program impact, the treated households had on average-- what is this-- 8.4% lower people below the poverty line, percentage points.

PROFESSOR: Percentage points. So they were 8.4 percentage points less likely to be below the poverty line. And then generally, this type of table, we are going to provide the control group in order to know what we are looking at. So here, sometimes set at baseline, here they show us the control group at the four-year follow up. That was 62%. So it's 8.4 percentage point on the basis of 62 percentage point. So that's a 13.5 percentage reduction.

Then we are testing whether the impact are-- remember we are interested in long run. So we are testing whether our impact-- really, they could have done one-sided test to see whether the impact were decreasing. But they are testing whether they are different over time. In this particular instance, they seem to, if anything, increase over time.

Then the R-square. I don't know what we do with R-square ever, but that's there. And then the number of women, the number of observations in their households, and the number of clusters.

AUDIENCE: Professor.

PROFESSOR:

AUDIENCE: In reference to these, is there ever a decision when randomizing or choosing a sample to make sure that we don't treat more than a certain percentage of the village to make sure that the total capital stop does not change or something like that? I'm thinking about how you would argue that GB effects are not that bad.

PROFESSOR: So there could be. Or you, in fact, could go the opposite direction. There has been a lot of-- several studies in the past few years trying to vary the fraction of people who are treated in order to see whether you start seeing equilibrium-type of effect on prices and things like that by randomizing at that level.

So sometimes you want to rule them out because you're interested in the individual effect. Sometimes you're very much interested in the market effect. So you want to make sure that you saturate enough on the contrary.

So for example, Lauren, who is visiting here, she's doing a lot of work on making markets, in a way, creating a relationship between traders and potential sellers. And there she is looking almost for the opposite. She wants to make sure she has enough saturation. So yes, absolutely. It's something people have in mind to see where they are.

So in fact, in the case of the ultra poor studies, the reason why I think we see no externalities is that in every village, very few people are treated, and not tremendously socially integrated people, either. So that's the research they find encouraging. Impact on pretty much everything, especially after four years.

Then the *Science* study came about and, in a sense, was a response to the external validity point or a way of addressing the external validity point, given this super promising program. There was a lot of hope that this could be a strategy to address poverty.

And but given how expensive and, in a sense, specific the program is, with all of these various component to it that you can't really easily untangle mechanism or understand why it works, there was really a desire to make sure that the program is replicated in as different context as possible in order to assess the external validity of the context dependent.

So this is a project that was led by Dean Karlan, who is at Northwestern now, was at Yale now, and started IPA, which is an organization that conducts a lot of randomized controlled trials around the world. And he and the Ford Foundation and CGAP, the World Bank, microfinance, ultra poor arm, kind of got that project together. And I don't want to underestimate what this means to get six different organizations in six different countries with six different research teams to each conduct their project together.

And what is interesting here is that there was really-- the goal was really to estimate the same project. But what does it mean to be the same project? You can't force people to do cows everywhere because the whole point is that people can choose their assets. So you need that to have a definition of same. What is same?

And here, the way they enforced that sameness in a way is by having people from all over the program meet in Paris every year. I attended a few of these meetings, where they were discussing exactly what they were doing, the process they were following, et cetera. So same is like a sameness of spirit, and then embodied, in a sense, in different ways, in different places, with keeping, of course, the same element in place-- identification, the assets, the multifaceted support to go with it.

But then people, for example, the staff is paid very different amounts in different places. The value of their assets is different in different places and so on and so forth. But to arrive at the same program given the context. So that's the first thing that is worth mentioning.

And then the other thing that happened is that all of these different teams were conducting the project separately. And instead of each writing their own paper, a decision was made to write one paper, which would have all of the data together precisely because that was supposed to be looking at what are the effects of variety, heterogeneity, and so on. And therefore, you needed one kind of template.

And the nice thing about that as well is that addresses another aspect, which is the cherry picking of results. So of course, there you also have a lot of results. But everybody has to-- everybody was running their own analysis separately, and they had to follow the same template. So the template is given in advance.

So even though there are many outcomes, and here you have outcomes that are already there. For every one, they are pre-established. This is what the outcomes are going to be. And it leaves very little place for cherry picking.

Despite that, you have a lot of outcomes. So to have something that is reliable, you end up doing something that is frequently done, in particular, in this type of project, where you have a various variable on food security, for example. Did everybody eat yesterday? Did kids miss any meal? Did adults miss any meal? Et cetera.

So you form an index that indexed this component by standardizing each of the variable and then taking the average of them. And I'm not saying this is necessarily the best way to form an index. But this is how the literature have evolved. And this is the standard way of doing it.

So here are the index that were used. This is a first in line, 15 month. Second in line is at three years. So now it's expressed in standard deviation of the mean directly, even for things that are in money to start with.

And here again we find effects across the board on pretty much all of the outcome, with effect for consumption is about 0.12 standard deviation. So it doesn't make them Bill Gates, either. But it's an increase in consumption that's stable over time.

This is a q-value. So what's the q-value to comparison to the p-value? Yep.

AUDIENCE: It accounts for false discovery rate.

PROFESSOR: Right. So it's one way to account. There are many ways. But it's one of the methods to account for the fact that you have more than one thing that is being tested. And each of the tests are connected to each other. So that's a false discovery rate adjusted p-value. And here it doesn't mess up too much with you because all of the outcomes tend to have similar impact.

So that conforts the results of the BRAC study, showing impact at the 1.5 and at three years that are not-- the impact don't seem to be decreasing from one place to the other. So we already discussed that. It addresses some of the-- so that study addresses some of the shortcomings from the point of view of pure impact evaluation, of being confident that that program might work not only in those sites, but maybe in other sites.

If we look at, then, since we have many countries, I showed you the effect for-- as if it was one giant regression here. But in reality, we have a lot of countries. So one thing we can do to start to look at external validity is ask ourselves, how do the results vary from country to country? And you can see that they are actually pretty variable. They appear to be pretty variable from country to country.

In particular, in Honduras was a bit of a disaster. You can give an ex-post explanation for that. In fact, I can give you the ex-post explanation for that. The main asset was chicken. And all the chicken died because of bird flu. But that's ex-post. But that's true. So that's docile. But you see a variability in the--

But funny story about the assets. We were doing it in India. I was running the study in India. And my Indian co-author and I-- Ragha Chattopadhyay, his name is-- we went to-- I mean, I guess Abhijit is Indian too. But the guy who actually works in Kolkata. We went to the field together. And they were about to hand chicken.

And he was like-- he went screaming, saying, you cannot-- this was before bird flu-- but you cannot hand chicken. They die. And he really convinced them. Almost went to the CEO of the organization to say that chicken should be ruled out. And so they didn't do chicken. They didn't really put it in the menu because they had been so popular, but he thought they were dangerous. And then this Honduras result came out with all of the chicken having died. Yep.

AUDIENCE: I wonder how cash transfers and asset transfers differ conceptually in the center. Why would you expect [INAUDIBLE]?

PROFESSOR: Yeah. So that's a great point, which I will generalize a little bit. So the question is whether a cash transfer-- why do assets and not cash? How do they compare? Do we know that?

This is, of course, this is a huge bundle thing. So I'm now I'm going to-- I'm showing you this reduced form effect and of doing everything together, and showing that there is an impact. And then we'll talk a bit about country heterogeneity.

But then there is the-- once we satisfy ourselves that there is an impact and maybe it's doable is, where is it coming from? Could we take out some of the component of the program that are less effective? Could we go to cash and avoid this chicken issue or maybe increase this chicken issue?

So we know some about that, which I'll come to in a bit. And then some is still not fully unpacked. But you're completely right.

And it's not a criticism of the validity of experiment. But it's potentially a criticism of this kind of experiment, which are telling you, this is my treatment. That's its effect. Now, this is all I can tell you right now. And then it's kind of whet your appetite to say, well, what's the mechanism? Why does it work the way it works?

And do we really need to do-- so from a policy point of view, do I really need to do all of this? From economics human behavior perspective is, what have I learned once you told me that all of these things matter together? Yeah. So that's the ultimate bundle experiment in that way.

So that's the assets. That the income and revenue, a little bit more-- less diverse per capita consumption. So that's the question of interest. In the paper, we just showed some of these different-- some of these effects by country. And we present them in presentation. But we leave it at that.

Now, the question we wanted to answer, if we want to answer Recca's question, which is, do I learn from one experiment what's going to happen in the next site? Or, in fact, your point as well. Well, your point adds this homogeneity in the experiment. But what's the predictive power of one site to another one?

It's not really sufficient to look at those type of graphs because when you run an experiment in different places, so here we have six sites. Each site is different for two reasons. Number one is the treatment effect might be different fundamentally. It might be higher in India than in Honduras.

Number two, there is noise in the world. And the sample that you happen to have found in India might be different than the sample in Honduras, both in treatment and control. And every estimate comes with its own standard error.

So once I saw different treatment effect, this country-by-country treatment effect is conflating these two sources of uncertainty. So if someone shows you and said, those treatment effects are quite different. So maybe the next country would also have a very different effect. It's really attributing the entire difference to heterogeneity in treatment effect when, in fact, there is also the underlying noise in the world. So this is a bit-- that's not the answer you need or want.

So the way to-- so you can't really solve this problem without being willing to put a little bit more structure to the problem. And one way to do this is by a method called Bayesian hierarchical analysis, which Rachael Meager, who was a student here and is now at the LSE, started to do first for microcredit studies. And then I'm going to show you her result that she just sent me off of the press to look at this very program.

And the idea is to hear the structure-- the basic structure on the problem. And I'm not going to go to the detail of how you conduct the analysis. It's just computationally a little bit involved. But what's important for now is that we understand the concept.

The concept is going to say, let's assume that the treatment-- that there is a distribution of the possible treatment effect, and that distribution is normal. So the treatment effects are themselves drawn from a normal distribution. And I'm trying to find out, for each site, the treatment effect for that site as well as the variance of the treatment effect across site in the world in general. Well, that's the idea.

So if the variance in treatment effect is very, very small, of the distribution of the treatment effect, this suggests that the treatment effect itself is quite constant. And therefore it's, for example, it's legitimate to pull all the data and estimate one pool treatment effect.

Once you know that, it's convenient because you can use information from all of the other countries to increase the precision of your estimate in any given site. So the precision of the estimate in India will increase a lot if you know that you can use Peru and Honduras, et cetera. And also, you can form credibility interval, which will be different from confidence interval, but are the same context, in prediction for the same-- for next effect.

If, on the other hand, you find that the treatment effect are-- there is a huge variance in the treatment effect itself, and not just the outcome, then you know that there isn't heterogeneity. It's not very predictable. And maybe you can try to predict the heterogeneity itself. But otherwise, you know there is variation. So that's the idea.

What she found is that both in terms of the-- so these are the effect using-- just the effect from the pooled regression using the linear model. So that's the result I already give you, assuming there is one treatment effect for the whole world. And this has a treatment effect in the sample with the hierarchical Bayesian model.

And you can see in this instance, the confidence interval going up, reflecting heterogeneity in the site. This is making the assumption that everyone has the same treatment effect. This is not making that assumption anymore. And that decreases my confidence in the results.

And now if I want to predict the next site because this is the predictive effect. So if I want to say, what's my best estimate of the effect, not in my sample, which is the effect here, but in any future sample, the predictive effects are quite wide. The credibility intervals are quite large, which reflect a large variability in the treatment effect from site to site.

So here the fact that we saw already in the graph that the treatment effect seems to be going all over the place reflects some real heterogeneity, as opposed to noise in the data. In other words, each treatment effect is estimating relatively precisely for that country, but it happened to be different from site to site. So my predictability of the next site is not that good. That said, despite that, most of them are positive, even with that heterogeneity.

So this result, by the way, are different from what she found for microcredit, where she found a lot of pooling, where she found that, basically, it's a nice big fat 0, regardless where you are. Here, we have positive effect, but viable. Yeah.

AUDIENCE: Why does the [INAUDIBLE] estimate change?

PROFESSOR: Here, you're making the assumption that the effect is the same everywhere. Therefore, one observation is one observation is one observation, whereas here, you're not making that assumption. So when you estimate your effect, you're not making the assumption that you can just run the regression all pooled. You are basically getting an average of the different treatment effects, where the weight don't become any more, like one paired observation.

So in the rest of the-- so far, an existing paper, but I can send you the slide deck to start with, what they are doing is to try and see whether they can explain some fraction of that heterogeneity by covariates. Is the fact that-- in other words, can you use the heterogeneity in treatment effect by covariates to predict heterogeneity in treatment effect across sites based on the fact that the covariates of the sample was different? Does that make sense?

So they are basically saying, well, the treatment effect, do they differ by people? To what extent do they? And then to what extent does that explain my difference in treatment effect? And the answer is not that much simply because we are not very good at using the covariate to predict different treatment effects in this case, even within countries. That I know for having tried.

So if you're interested, with her permission, I'll diffuse the slide. I think she'll give me permission since she sends me the slide for this. And you can see a little bit what she does. It's not complete, but it will be at some point.

So anyway, even despite-- so that's an interesting point, I think, for heterogeneity because this is really the first paper where I see a lot of heterogeneity being demonstrated, even in a program that, in general, works.

In terms of cost-benefits, this program is super expensive. So it's only worth it if the benefits persist into the future. If you look at the first paper, the consumption increase is about 11% in Bandiera's paper. So in order to do cost-benefit analysis, they assume that then this effect lasts.

Of course, the depression rate of benefits is key. And so whether or not this is a poverty trap becomes key also for whether it's a program worth doing, not just because we're interested in it.

This is from the science paper, where we are looking at the sensitivity to different discount rate, time horizon, and benefits, and calculating for how long the benefits need to last in order for the program to be cost effective. And the point of that table, which you can look in more details at your leisure, is that if the benefits were to persist, despite the program being extremely expensive, it's very cost effective.

But if they were to decline after four years, then it would not be, obviously. So then that becomes a very interesting empirical question, and it's tied to our poverty trap, is, do they persist?

So we looked at this with Abhijit and with Garima, who is in the program here in West Bengal. After the three-year survey, we observed it at seven years and then at 10 years. And here is one of the graph for consumption, where you see that the treatment effect actually grows between year 3 and 7, and then is persistent-- mostly persistent between your year 7 and year 10.

So this is in a context where there was economic growth. So the control grew where the treatment grew faster. And so the treatment become faster. So the control level grew from 1.8 to 2.9. And the treatment effect in terms of standard deviation actually jumped between year 3 and 7 to 0.7 standard deviation, and is still 0.6 after 10 years.

So that shows some persistence of effect, which means that when we actually reduce the cost-benefit analysis for India with this new data, it's actually much more favorable than it was done at year 3 because the treatment effect didn't vanish and didn't decline. So in fact, by year 10, even if the effect were going to disappear fully, the program is worth-- has paid off for itself.

So this is for income. We're also looking across outcomes using, again, nicely. We have a nice pre-analysis plan given to us by the fact that we are following an existing paper. So we don't invent anything. We redo the same paper. So the paper also nicely writes itself.

And you're finding the treatment effect to persist over time, with the exclusion of-- with the exception of financial inclusion, which is mainly whether people borrow or not, where we never really had much of an impact in the first place.

In that paper, we discussed-- so interestingly, the effect on physical assets that people have, the clients have had, were first increasing, and then went back down. And another thing we see is that these people are getting quite old who got the first transfer. So a lot of them are also slowly retiring and winding down their business.

What we saw in India between the first and the second in line, and then the second in line and the third is people diversifying out of cow, into nonagricultural businesses, like small business and stuff like that. What we saw them doing between this wave and that wave is winding down the business and sending migrants out there, and in particular, sending migrants faster.

So to answer the question about what's the mechanism, we make an assumption. We make a guess here, a conjecture, which we are-- it seems to be borne out by the data we have, is that the way in which you actually have persistence of this program is first by diversification from agriculture to nonagricultural businesses, and then from small business to actually more stable wage work via the following generation, which moves for a longer period, for further away, and earns more money as they do that.

Now, these programs have highly heterogeneous effect. We saw it by country. I already showed you that by country, they have super heterogeneous treatment effect.

We also see it by people. So these are now within-- this is Bangladesh. So this is within a particular country, the quantile treatment effect. So remember, it's not the effect on the-- it's not the distribution of the treatment effect. But it's how the distribution affect the distribution of assets or consumption expenditure.

And what you're seeing is that the effect at the low quantile are not very large, and they keep the effect on the highest quantile higher. We find the same thing in India, and people find the same thing everywhere, that the treatment effect tends to be towards the highest quantile, with less effect at the lowest quantile of the-- moving the lowest quantile of the distribution.

On the other hand, as I said before, an effort to link that to observable is not very successful. So the effect of heterogenization is not very clear initially why.

So a key question for the future from a program perspective is, how do you identify people which have very high return? And that's the same thing as in the microcredit business, where, even though they are, on average, as we are going to see when we study credit treatment effects of programs, are 0, they are quantile treatment effect that are very different at different quantile. Some people seem to benefit. How do you know who they are to target them? And in a way, the ultra poor have that as well.

In our case, this is a project that is a question that is even made even more interesting by the fact that this is motivated by poverty trap. And we kind of have an idea of maybe something that could sell that you get huge effect or you don't, which is do you get out of the poverty trap for this persistent effect or not?

So that crew who did the Bangladesh program, along with Clare Balboni, who is teaching in our department, as you know, and Maitreesh Ghatak, who joined them, tried to look at exactly that question. So it's really one of the first serious attempt that I know of to draw a kind of S-curve or to see whether there is S-curve at the individual level.

And some people have been quite skeptical that those existed. There is a paper by McKenzie on small businesses showing, I don't see any of that. But with observational data, of course, what's the main problem with trying to draw an S-curve with observational data? And why is it that you might not find it in observational data? Yep.

AUDIENCE: So if you do have this S-curve and the curve intersecting the 45-degree line, that steady state is unstable. So in a cross section, you actually had very few people who were there, so you wouldn't get them.

PROFESSOR: Exactly. Nobody is there. It would be like an accident that you find even a few people there. And therefore, you shouldn't be able to capture it in observational data in the real world if you really believe the model. Unless the S-curve was actually individual specific, and people were moved, it seems that, in fact, you should never see people with a very, very high return range because they would have moved out of it by now.

So in the real world, you will be able to draw a curve linking capital today to capital tomorrow. But that curve is going to reflect the heterogeneity of where those threshold might be located, as opposed to whether that threshold exist or not. That's pretty big problems. So why the ultra poor program helps us there pretty nicely? Yep.

AUDIENCE: You follow them over time. So [INAUDIBLE].

PROFESSOR: So there is that. And in addition, so-- yeah.

AUDIENCE: Since you start off with giving money to people who are ultra poor, you might put them into that middle area. And then you can watch them transition to the other side.

PROFESSOR: Yeah. So what would have been the ideal experiment if testing this S-curve had been the ultimate goal? Yeah.

AUDIENCE: Give different amounts of money or goods to different people.

PROFESSOR: Exactly. So different amount of money to people who start from the same assets would have been the ideal thing to do. Or this is not what was done here, that there are two life program. And they were probably not about to do that.

But you have something that is getting a little bit close to that, is people who are distributed whichever way. And then they got these assets. And then the asset would have been small enough of-- large enough or not, depending on how poor they were, to put them over the threshold or not. So in order to do this exercise, you have to really believe the model because you have to believe there is one threshold out of which-- that people can jump over or not. Otherwise, it'd be hard to find. Yeah.

AUDIENCE: But then in the model, there's also one steady state and one threshold. And now we are allowing people to have different steady states, but assuming that they still have one threshold to pass.

PROFESSOR: So I think in the model, there is two steady states-- one low one, one high one, one unstable steady state, or two or three, including an unstable one. And I think in the view of the real world, also, there are only-- every one faces the same thing. But then they are shocked, which means that at the given point in time, they end up-- they have end up one way or the other.

Remember, in fact, they have a distribution. This is a distribution of assets. And I would say, no, no, they are two. They are in one unstable steady state and two stable steady state in the real world. So what I should see is that there are a lot of people at each of them, and then not too many people in the meantime.

AUDIENCE: Well, if there are shops that are growing, I don't need to experiment to begin with.

PROFESSOR: Yeah. But the point is that they are shock combined with the fact that-- so it's not saying that you'll never observe anyone in the unstable steady state. But you won't see very many people. So it's going to be very difficult to catch them.

So here is the curve. And I think that that's really a beautiful graph, in a sense of-- I don't know if it would have occurred to me that you can even try to do that, which is to say, well, let me take the theory super seriously. There are two steady states. Therefore, I should see people with two levels of assets, the high level and the low level. And let me draw the distribution. And sure enough, they have this bimodal distribution.

So they live in a world where all-- this seems to be a world where that thing actually exists and is relatively clear, what is the statistic. And then they are shocked, which means that you don't see nobody, but you don't see very many people there. And then once we have that, then I think we went through already. This is small. We can now add the value of the asset. This is quite small.

So we add the medium value of a cow. And now, suddenly, we find a bunch of people in the middle. And in a sense, they were lucky because the transfer was such that given from the people who were very, very poor to start with, there are a lot of them who are just to the left or just to the right of the threshold.

And now we can run that cow. So this is a treatment group. And this is saying-- that's the curve we already saw in the first class, exactly [INAUDIBLE]. This is pretty impressive, which is, let me show-- this is a nonparametric regression. There's no magic there. No rabbit has come out of the hat. That's the data showing that this is the baseline productive asset in the treatment group and in the baseline predictive asset in the control group.

So the treatment is the asset they had at baseline plus the value of the transfer. And now you're able to draw this curve. And you can draw it because you have sufficient mass on both sides.

And by contrast, if you try to do that in the control group, you only find one unique steady state. Maybe there is that other steady state that we can't catch it because there is nobody there. And then there is-- so you can turn that into a regression exercise and to say, define delta i as the difference in assets. And then k star is the threshold level of capital, which they estimate from that regression.

And is it the case that people grow faster when their level plus treatment is above the threshold in the treatment villages compared to the control villages? Because the asset level plus treatment is just their asset level, so they don't benefit from that.

So that gives you a regression, which exploits your experiment. So you're saying that in treatment, if you end up being-- if your own asset level plus the transfer puts you above the threshold, you have a fast growth in capital, but not in control because the treatment you didn't get. So you are above, but you didn't get it. And then we can put the two sample together and run this as an interaction.

So that's where I'm going to leave it for today. The next question, of course, is, where does that come from? And we know it's not food. We know it's not credit. So the question is what it is.

We're going to continue talking about that. In fact, we're going to continue talking about next week because you are going to talk to your-- Frank is going to come for two lecture to talk about behavior. And one of the potential explanation is that it has more to do with psychology, with various aspects of the psychology of poverty, which might have impacts that are functionally equivalent to the ideas that were in the nutrition-based poverty trap that comes from people's reasoning, as opposed to something else.

And then we are going to see it again when you talk about labor markets because some of this could be productivity of labor. But we now know that, at least for the ultra poor, there might be something like that which is really at play. And so that should give us even more energy and motivation to find what it is that can be at play, hence the unpacking. That is also going to come up in other-- at several points, we're going to have a chance to unpack the ultra poor program.

Thank you. That's it for today. So Monday-- next week will be Frank, and I'll see you again in the following week.