[SQUEAKING]

[RUSTLING]

[CLICKING]

ESTHER DUFLO: OK, so today I want to talk about endogeneity and one new method, which is the method of instrumental variables in two stage least squares.

So after our nice interlude of machine learning where we didn't think about-- we didn't have to think about causality anymore, or even about coefficients because we were all about prediction, I want to bring us back to our efforts to establish causality, which sometimes is really what we are interested in.

So suppose that we start from a model like the one we had finished with at the end of last lecture where we have some x1 variable that we actually observe. We are interested in the causal effect of x1, and then we have some x2 variables that we may or may not observe. And we discussed a bunch of stuff that we could do in order to make sure that the coefficient of beta 1 is in fact indicating a causal relationship going from x1 to y.

But there are occasions where the regression based methods that we saw, just controlling for as many x2 as we can, or a difference in different strategies which is another form of regression control if you want-- or even regression discontinuity design-- is just not going to cut it.

Because we don't have the control variables, for example. Because there is no regression discontinuity that we can use, or because a plausible case can be made that the variable x2 actually could affect y. The variable x1-- sorry. X2 also, by the way. But here we are interested in causal effect of x1. The variable x1 might affect y as well, in which case, it is really not an easy control approach to that problem because if the variable x1 affects y and y affects the variable x1, there is no way-- we cannot easily control for that.

So Sara gave me a very nice example of this, which is this kind of breaking news of a few years ago-- "Unhappy People Watch More TV: Study." An extensive new research study has found that unhappy people watch more TV while those who consider themselves happy spend more time reading and socializing.

Now the question is that from this title, do you think they are trying to claim that it is TV that makes you unhappy, or that it is unhappiness that makes you watch TV.

AUDIENCE: The latter.

ESTHER DUFLO: The latter? Who is voting for the former. No one. So interestingly, they are actually thinking about the former and the latter.

[CHUCKLING]

It is a spectacular study--

[LAUGHTER]

--that actually in the same study, which is based on correlation, makes both causal claims. TV does cause people to be less happy. In other words, the causal order is reversed for people who watch television. Unhappiness leads to television viewing.

So I think one could actually make a reasonable case going both ways. So it is not necessarily a stupid thing to think about. The problem is that, intuitively, you feel both could not very easily be established from the same causal regression. Because if I can write the model both ways, clearly the coefficient is going to be biased if I first attribute the entire causal effect on x, and then I reverse the regression-- I attribute the entire causal effect on y. It seems to be giving myself double credit.

So that's the main problem for endogeneity is any situation where-- they're making the argument, actually-- a theory argument-- why TV caused people to be less happy, which is the idea that it's too easy, and it's passive, and you don't kind of-- other activities have more benefits.

And then the other-- so that's why TV might make you unhappy. And in other words, the causal effect is reversed. Unhappiness leads to television viewing. If you are unhappy and not very energetic, you are going to watch TV. So both channels are plausible a priori. So this is when we have an endogeneity problem.

And so we talk about endogeneity when there is this mutual relationship. What are other examples that you can think about where you'd have-- it's not an omitted variable bias. It's really the fact that one could sit down and write a causal model going either way. So can you think of other examples where we would be in the same situation? Yep?

AUDIENCE: A lot of conditions associated with state failure which might have mutually reinforcing effects-- whether it's a type of government needing to see failure-- or see failure being associated with two other types of government.

ESTHER DUFLO: Right. So for example, you could have-- that's a great example. So you can have a huge crisis leading to a government collapsing, or you could have the government collapsing leading to a huge crisis. And those two things-- we will observe them together, and what caused what? Maybe both caused each other. So that's a great example. Yep?

AUDIENCE: Any system with a lot of feedback. If it's a technical [INAUDIBLE] problem, for example, [INAUDIBLE].

ESTHER DUFLO: Right.

AUDIENCE: For example, if you say that a loss of economic status leads to-- tends to be associated with lower education, then lower education is also sometimes related to lower socioeconomic size.

ESTHER DUFLO: Right. So one thing here is that it might not be-- if you're interested-- the economic status of the parents might lead to low education of the kids, and then the low education of the kids will not immediately necessarily feed back on to low education of the parents. So in that case, that might be more of an omitted variable bias.

But you could think of a version of that, for example-- an older teenager running into trouble at school, getting kicked out, which only brings them to more trouble, which is-- Yep?

AUDIENCE: Practicing some things, say an instrument, makes you better at it, and being better at it makes you want to practice it more.

ESTHER DUFLO: Exactly. Perfect. Perfect. So these are all great examples. So related to the state failure, sort of democracy and growth. Or maybe growth makes it more plausible that the democracy takes hold because there is more interest in a participatory process of government. On the other hand, maybe democracy is good for growth. There is a paper on the flashing everywhere in the Department these days that makes exactly this claim.

Health and exercise is another one. You could think that exercising regularly keeps you in good shape. But if you're in bad shape, you might not feel like exercising. Recent one that I heard about is that people who do crosswords are less likely to exhibit signs of cognitive decline like pre-dementia. On the other hand, they are saying that people who have pre-dementia are not so excited about doing crosswords. So that's--

And of course, one that, as economists, we love, is prices and quantity. When prices are low, people want less of it. On the other hand, when people want more of something, the prices tend to go higher. So in this case, it's actually the relationship goes in the opposite direction, but there is also a causality running both ways. We can run the equations. There is a supply equation and a demand equation where price and quantity appear on both sides.

So these are examples of endogeneity. So whenever we have endogeneity, we have a problem, and it's going to be difficult to come up with a control coming from outside-- using the strategy of adding a lot of x2's or even a different indifference once there is endogeneity.

And then there are also many cases where there is no, strictly speaking, endogeneity, but there is just a very thorny omitted variable bias. And we know that there is no data set in which that variable is there. It's not-- it's not an endogeneity-- we could even name and pinpoint the variable if we could, but we just don't have it.

And I'll give you one example of that that's going to be our sort of running example for today, which is the benefits of education. Exactly the example that you mentioned. We know that there is a strong correlation between education and many outcomes. Of course, knowledge, earnings, fertility-- that's mostly in developing countries-- but health, et cetera.

Of course, if we interpret-- if we run a regression of any of these outcomes on education without controlling for a number of tasks, there might be bias, exactly as you pointed out.

For example, the family background might affect those outcomes later in life. Kids who come from poorer backgrounds may have trouble finding a job for reasons of nothing to do with education, but, say, because of networks. And for this reason, they'll have lower earnings. So that's going to create an omitted variable bias, and it might be very difficult to observe everything we need to know about the kids.

Another one is just what we were discussing in the case of the applying to college. Sort of spunk, and drive, and energy-- what economists sometimes put under ability, which is a little bit vague. But people who are more energetic, et cetera, smarter, or whatnot, will find it easier to get an education. They might also do much better in life anyway, even without the education. There are cases like the cases with the college application where we have a variable that's a pretty good proxy for that, but it's rare. We were quite lucky to have that.

So in this case, my first instinct to any such problem is to see whether we could run an RCT, but in this case, assigning education to people is not going to be possible because an education is really a choice. You can't really force kids to stay in school past a point. And on the other hand, you can't force parents not to send kids to school. So it's closely linked to your personality, and your choice, and who you are. So let's say we cannot assign randomly, you get three years, you get five years, you get to go to secondary school, or not. OK? Let's say we can't really do that easily.

So what can we do? Well, instead of trying to assign education randomly, we could try and assign students to a program which may lead her to get more education. So what would be an example of an education-- of an intervention like that, that could be randomized, that is going to lead some people to get-- on average, is going to lead some people to get more education than if they had not received that particular intervention?

AUDIENCE: After school programs where enrollment is assigned at random.

ESTHER DUFLO: Yes. So it could be an after school program. I want you to keep this thought in mind, because-- for a reason we're going to discuss in a minute-- it's not going to be a great instrument, but it's still going to-- it could still affect whether the kids stay in school. So let's keep after school education. Yeah.

AUDIENCE: What about, like, free preschool?

ESTHER DUFLO: Right, so free something. Free preschool or-- whatever level you're interested in-- free preschool or free school. Yeah.

AUDIENCE: Free meals.

ESTHER DUFLO: Free meals. Exactly. So let's keep that in mind as well.

AUDIENCE: Campaigning and advertising.

ESTHER DUFLO: Campaigning, information, et cetera. Trying to convince-- for example, there is quite a lot of evidence that parents may not be aware of the benefits of education, so letting some people know what the benefits of education are could be a way. Yeah?

AUDIENCE: Access to technology, like a free laptop program.

ESTHER DUFLO: Right. I want you to keep that one in mind because it is also going to create problems for us, but that could affect education. Holly?

AUDIENCE: I'm not sure if this one fits-- and that's why I'm asking-- but what about regular public school versus a charter where you have to get in by lottery?

ESTHER DUFLO: Right, so that is going to give us the effect of going to charter school as opposed to the effect of regular public school, which is a very interesting and important question on its own, but would probably be a different question.

AUDIENCE: So that's not exactly what you're--

ESTHER DUFLO: That's not exactly-- because now I'm thinking about something that's going to vary whether the years of education, literally, that some kids get.

AUDIENCE: Rewards for attending school or for high performance.

ESTHER DUFLO: Right. You could give incentive. There, again, I want you to keep that in mind. We'll go back to all these instruments. So I hope you can maintain a mental list of all of those ones, because I want to go back and re-evaluate them.

The one I want to study today-- so for now, the one I'm going to keep a track on immediately is Lisa's idea of scholarships-- or making education free for some people.

So whatever the intervention is that is going to affect education, if that intervention has no direct impact on earning, if we are willing to make the assumption that intervention in and of its own would not affect earnings, or cognitive scores, or whatever you look at, but you see that it affects earning-- that the people who got the program have higher cognitive scores, or have higher earnings, or have lower fertility, then you can infer that, because you're willing-- if you're willing to make the assumption that it has no direct effect on education, then you can infer that if it affected earning, it has to be because it moved education.

So in other words, there is a link between the intervention-- so here I'm going to call it scholarships effects education, which affects final outcomes. I'm going to write a number. Cognitive score, earnings, attitude, et cetera.

Suppose that you are willing to make the assumption that that link is not there. And suppose the scholarships were randomly assigned such that you can really look at the effect of the scholarships on cognitive scores, or earnings, or attitude in a way that you're really convinced it's a nice causal effect.

Then you can say, well, I see that scholarships affect earnings-- that the people who got the scholarships have higher earnings than people who didn't-- I'm willing to assume it's not because of the scholarship themselves. Then it has to be because of education And because of that I can infer what the effect of education is without really using any endogenous variation in education.

So that's basically the name of the game. Now, I'm going to formalize this insight. So if it went fast, don't worry about it. We're going to now go very slowly through this. We are using a scholarship as an instrument for education, which is something that makes education move, that we can exploit in order to look at the causal effect of education. That's the plan.

And we are going to do that with today with an example that I worked on with Pascaline Dupas and Michael Kremer, where we actually randomly assigned scholarships for secondary school. So the question we want to look at is whether education affects cognitive scores and wages with Yi as the outcome for historical reasons. I call Ai education. So that's our education outcome.

So in this case, it's going to be whether the kids attend secondary school or not. The dummy variable equal to 1 whether the kids attend secondary school or not. And Y is earnings or cognitive scores. I'm going to do it with both cognitive scores and earnings. And I'm not willing to make the assumption that epsilon i is independent of Ai.

That's the big difference between the setting we have now and what we looked at before, because in there there is an omitted variable problem. I don't have the variable, which is ability, or spunk, or network connections, or whatever goes with the background. So in the absence of that, I know that if I estimated this equation, it would be biased. Beta would be biased.

One note-- this assumes that the effect of education is the same on everybody. So there is a coefficient beta. We may not like this assumption because education might be more effective for some people than some others, so we'll discuss how to interpret IV when that assumption is not correct a bit later in this lecture. But otherwise, this is a model that should be familiar after all the [INAUDIBLE] we've seen. And we're really interested in a causal estimate of beta.

So what did we do? We went to Ghana where primary and junior high school is free. And there is a big gateway exam after junior high school, and you can only go to secondary school if you qualify. But in addition, it's very expensive. So you go from free primary and junior high to very expensive secondary school for people who qualify.

So what we did is we went to a bunch of kids who had qualified but had not enrolled by September. And we could know that by looking at-- by going back to all the junior high schools where they attended and find out information about them. We had a big set of kids who had qualified for secondary school but didn't go.

And it's pretty common in Ghana to show up late in secondary school-- to show up in January instead of September because you finally manage to gather the money. So what we did is we went to see them in September and say-- so we took our sample of about 3,000 kids and we randomly selected about 1,000 of them, and we went to say, hey, if you want, we have a scholarship for you to go to secondary school-- for you to use it going to cover the four years of secondary school.

So I want to call this variable Zi, which is a dummy equal to 1, if you are assigned to the treatment group and were offered a scholarship, and 0 otherwise. So this Z1 is randomly assigned because we randomly selected in our little office who we would go with saying, hi, can we collect some data about you-- or, hi, can we collect some data about you, and then, oh, by the way, we have a scholarship for you.

And then what we did is that we followed them. So we did that in 2008, and we've been following them since then. So first we had to wait till they left school, which they did in 2012, and then since 2013 we've been interviewing them regularly. Both the losers of the scholarship-- the people who didn't get it-- and the winners.

So getting the scholarship does not translate into a one to one relationship between going to school because most of the people who got the scholarship did go to secondary school, but not everyone. Some of them had, by that time, decided that they were not interested anymore.

And on the other side, a bunch of the kids who didn't get the scholarship managed to find some other way to pay for secondary school. So they cobbled the money together, or they got some other scholarships, or something like that. So what we have here-- and what's going to be the structure of the table for a lot of today-- is we are looking at boys and girls separately, and we are looking at their outcomes.

So this is the sample average in the treatment group and the control group. The difference, with the standard error-- the sample average for the treatment group, control group, the difference with the standard error.

And what you see is that-- so first of all, it's not going to mean anything to you, but give me a moment of admiration for the fact that we found back 97% of the kids in 2013-- we found back 97% of the kids that we had identified in 2008. And remember, adolescents are mobile because this is the moment where they will leave and go work elsewhere. So that was a pretty good tracking rate.

It's important because when you don't track people, especially not equally, the people who remain are not necessarily representative of the initial population. OK? So here we don't have that problem. So we can now forget about it. And then we can see that 78% of the treatment group ever enrolled to secondary school and only 45% of the control group. Of the treatment group-- 58% of the treatment group completed primary school. 24% of the control group completed primary school. So that gives us a difference of about 33% for the women and 38% for the men.

You see that more women didn't go, even though they got the scholarship, than men, and almost all the boys basically took a chance to get the scholarships. So we get this difference.

And this is really the causal effect of getting the scholarship on going to secondary school because that was randomly assigned. So that's nice.

Now I want to combine this equation, which is my causal effect model, with this data, which is the fact that the scholarship did cause some people to be more likely to go to school. So let's combine the two with the expectation notation that we introduced when we talked about causality.

So the effect of the treatment on participation can be measured by the expectation of going to school given that you got the scholarship, minus the expectation of going to school given that you didn't. So for example, for girls, what is this difference? What is this number? This difference?

AUDIENCE: 33%.

ESTHER DUFLO: 33%. Exactly. So now we can look at the effect of the treatment on cognitive test scores, or whatever you're interested in. I'll show that to you right away. This is still cognitive outcomes. That's cognitive scores. Total standardized scores over here-- this is standardized to be-- so it's in standard deviation of the overall distribution.

And you can see that for women in the control group, they perform less well than boys. So in the control group they have a standard deviation of -0.17. In the treatment group it's about 0. So the difference between the two is 18 standard deviation of test scores. It's just a unit of test scores. 18 standard deviation for the women. And for the men, likewise, we see that the treatment group is .30 standard deviation. Control men is .18 standard deviation, so the difference is 0.125.

So that gives us the effect of the scholarship on test scores. And we know, again, this is causal effect, because our scholarships are still randomly assigned. So this really is the causal effect of winning the scholarship on test scores. But of course, that's not the effect of education on test scores, because the difference is not one for one between getting a scholarship and going to school.

So we could be interested in just that. If I give people-- if I go around and offer people scholarships to go to secondary school, what is going to be the final impact on test scores, or wages, or anything you're interested in. Sometimes that's what we are interested in.

But it is not necessarily the question of interest because, for example, the government might be interested in other ways to expand access to education. For example, building more schools, or making it cheaper, et cetera. So you might be interested in the causal effect of actually going to schools.

So now let's see how we can combine the fact that we have an effect of scholarship on going to school and an effect of scholarship on test scores to look at whether we can get a good estimate of the causal effect of going to school on test scores.

So for that let's combine these two expressions with our expression for Yi. So we know that Y is alpha plus beta Ai plus epsilon i. So I'm just going to put expectations around that for the treatment group. So the expectation of Yi given Zi equals 1 is alpha plus beta, the expectation of Ai given that Zi equals 1, plus the expectation of epsilon i given that Zi equals 1. OK? I can do that.

I'm going to do the same thing for the control group. So just putting expectation around the structural equation. E of Ei given-- Yi given Zi equals 0 is alpha plus beta E of Ai given Zi equals 0, plus E of epsilon i given that Zi equals 0. OK? Makes sense?

Now I'm going to take the difference between the two, which is going to be our causal effect of test scores. What is it equal to? It's equal to beta times E of Ei given Zi equal 1, minus E of Ai given Zi equals 0, plus E of epsilon i, given Zi equals 1, minus E of epsilon i, given that Zi equals 0.

Now I want you to assume that this is 0. And we will comment on this assumption in a minute, whether it's a good assumption to make or a bad assumption to make. But for a minute, I admit that this might be 0. If that is 0, then beta is simply the ratio between the causal effect of the scholarship on the outcome of interest, and the causal effect of the scholarship on going to schools.

So this is what we have here. Beta hat is E of Yi given Zi equals 1, minus E of Yi given Zi equals 0, divided by E of Ai, given Zi equals 1, minus E of Ai given Zi equal to 0. In other words, what I'm doing is that I'm scaling up the effect of the scholarship by the fact that not everybody is induced to go to school because of the scholarship. Some would have gone anywhere, and some didn't go, even despite that.

So in our example, this would be for women 0.125, if I remember correctly, divided by 33%. So basically multiplying by 3. So it's going to be about 0.6 for test scores. So it's telling me that people who have gone to school have cognitive test scores that have about 0.6 standard deviation higher than girls who haven't gone to school. OK?

So the key, of course, of all this calculation is this assumption over here that the expectation of epsilon i given Zi equals 0, minus the expectation of epsilon i given that Zi equals 1 is in fact 0.

So why is that possibly true? And that's going to tell us, basically, for an instrument to be a good instrument, we need two things. This better not be 0, otherwise this is going to go to infinity, and with it, the standard error. We need something that effects the first stage. We need a relationship between scholarship and education.

That's easy to check. If it's not there, you just try for another instrument. So that one is pretty important, pretty fundamental, but easy to check.

The other one is this assumption that the expectation of epsilon given Zi equal to 1 minus Zi equal to 0 is in fact 0. And that really can be unpacked in two things.

One is that the effect of Zi on Yi can in fact be estimated well. So that's going to be-- in this case, it's given to you free of charge because the instrument is randomly assigned. So we do know that the difference between the outcome for Zi equal 1 minus Zi equal 0 is, in fact, the causal effect of getting a scholarship on test scores or the outcome. So that's one aspect.

When we use instruments that are not randomly assigned, that are given to us by nature or by natural experiment or whatnot, it might have to be argued that this is, in fact, true.

And then there is a second part of the assumption that is not given to us for free even in randomized control trials. And that's very important to remember, that there is no direct effect of the scholarship on cognitive test scores. Otherwise, this wouldn't be true anymore because part of the effect of the scholarship on the test scores comes not from education, but from another channel.

So in other words, in here if Zi really belongs to this equation, then we would have an omitted variable bias if we don't include it. And therefore, epsilon E of epsilon i, given Zi equals 1, is not going to be 0 in this model. I can't assume that. So we need to be able to assume that the scholarship doesn't affect the final outcome.

AUDIENCE: When you assigned the scholarships, did you say that they were randomly distributed? Because I'm just thinking, if a child receives a scholarship, they may think, oh, well, I'm receiving this scholarship because somebody sees potential in me. Then they see potential in themselves, and then they get [INAUDIBLE].

ESTHER DUFLO: Yeah, exactly. So I believe we did say so, but we were concerned about that overconfidence, spunk, et cetera-- that people feel like they've been chosen somehow by us, or by God, for that matter. And then that would have an effect, independently, of the scholarship. So we did look at-- we tried to get at that.

But more generally you're putting your finger on exactly the really hard part with randomized-- with IV is that you need to go over any number of those stories, and unlike the second assumption, which in this case is true by construction-- or can sometimes be checked-- this assumption is has to be argued. And you will never be able to prove it. You can just think about all the stories you could possibly think about and try to see whether there is evidence for them. So you could prove yourself wrong, but you're never going to be able to prove yourself right. So that's exactly that.

And this is exactly what we need to-- never forget to check both conditions that both the scholarship is randomly assigned, or as good as randomly assigned, and that there is no direct effect, and try to go over in our head all of these conditions. The second condition is often not verified, even in the first example here.

So you ask the first question, which is, could the scholarship, per se have, an effect on cognitive score? Maybe the scholarship make people confident or happy, gives them drive, and spunk, or energy. So that would be an issue. What else could be another reason why scholarship have a direct impact on test scores? Holly?

AUDIENCE: Well, actually, [INAUDIBLE]. But I was thinking, if the family has to scrape up money, but now they have a scholarship, they can use the money to-- for better nutrition, for example.

ESTHER DUFLO: Yeah, exactly. So a lot of kids in the control school went to school anyway. And presumably they had to scrape the money. So now the control-- some of the kids have gone to school anyway, but the treatment school didn't have to pay for it. Therefore, that money may now be used for buying books, or buying better food, or something like that. So that would be another reason why a scholarship will affect your test scores, potentially, over and above any effect on education-- on years of education, which is what I'm measuring. Yeah?

AUDIENCE: Do you assign any value to these [INAUDIBLE]?

ESTHER DUFLO: Yes, exactly. The students know that-- you scrape exactly one year, but you know that you need to keep thinking about it. It might stress you out. If you go to Sand Hills [INAUDIBLE] today it's basically all about that. We only have so much bandwidth to think about things in our life, and, therefore, if I take some bandwidth away because you're thinking, will I ever be able to manage the money, you have less bandwidth to think about making good grades.

AUDIENCE: Did any of the kids who didn't receive scholarships work while going to school?

ESTHER DUFLO: Not really, no. Because it's like-- if you go to school, usually it's residential. So it's not that you have much of a choice. But if they had, then that would have taken time away from their study, and that would also be a concern. Yeah?

AUDIENCE: What types of survey questions have you been asking them in order to get at some of these other stories?

ESTHER DUFLO: So anything and everything. As many as possible. So first of all-- I think it's a great teaching example, but actually in the paper, for a long time we didn't even present the idea because we thought there was a lot of things that one could be worried about. But since we were always doing it implicitly we finally put it-- but then we tried to put a lot of stuff to try to get at that.

So for example, the other children-- the education of the other children, for example. It could be that because you went to secondary school for free it allows the other kids to get educated. Any notion of the budget constraint of the family being relaxed. We try to get at the other confidence question by asking kids in advance of the cognitive test how well they think they are going to do. People are very overconfident. Boys more than girls. But it seems that the treatment doesn't affect that.

We have questions on health, we have questions on how people paid for schools, et cetera. So this is all kind of always a little bit circumstantial. So that's the key. Yeah?

AUDIENCE: So in this process, or this experiment, we're testing the effect of more education on test scores, but that's within people who were qualified for these--

ESTHER DUFLO: Within this group. Exactly.

AUDIENCE: Does that mean that we have-- anytime we use this, it has to be with the caveat that for people who are accepted to high school [INAUDIBLE] a way to generalize to the entire student population?

ESTHER DUFLO: So that's a great question. Strictly speaking, no. You've done the experiment here among a set of kids who qualified for high school anyway. So if the government is thinking about improving education by relaxing the requirement instead of making it free-- so this could give us a good idea of keeping the exact system in place but making it free or cheaper. That's basically going to cover it pretty much.

What it would not necessarily cover is to, say, keep the same amount of money, but relax or remove the admission requirement. Then you're going to get new kids coming in, but they're going to be different kids compared to this one.

So that's a question of the external validity. Here the estimate is valid for this question in this sample. But if we don't assume that the returns to education are the same for everyone, they might not tell us the effect of another manipulation in another sample.

And in particular, for example, you could think that admitting less qualified kids, they would go to less good schools. It might lead to lower returns to education. So what we did to try to address that is that we have kids over the entire distribution because we have kids-- of admitted students. But some kids were at the very bottom and some kids are the very best because they were randomized at all levels.

So what we look at to be able to say something to that is the effect of the scholarship, or the effect of going to school-- once you have one, you have the other-- at all level of the distribution. Is it the case that we have lower effects for kids who are just qualified, versus kids who are well above? And we're not finding that. We're finding that it seems to be a shift up. So that doesn't necessarily mean that it would extrapolate beyond, but it gives you some sense that it might. Yep.

AUDIENCE: This is a general question, but how do you synthesize all the information that you get from surveys when

You are constructing a model on [INAUDIBLE]?

ESTHER DUFLO: So that's a great question. Generally, when you conceive of a study and you're going to ask questions to people, you're putting a lot of questions in the survey. And, therefore, you might have a lot of-- although it's less true for us because a lot of the surveys are done on the phone and have to be kept short. But one of them is in person and has lots and lots of variables. It has all of these.

And then a very legitimate question is, what do you report? And the danger being that if you report many, many, many, many, many things-- I think we discussed that if you run enough tests, a few of them are going to be significant anyway. So the literature is, at the moment, in general in economics and in social science, sort of grappling with this problem.

One way to go is to-- presumably you're running the experiment because you have a model in mind. You can write it up and, therefore, say in advance what you want to look at. So people call that pre-analysis plan or registration. So you can say, out of this whole, big wealth of data, I'm going to really focus on these four or five outcomes. That's what I'm mainly going to look at. And the rest is going to substantiate my case. But these are my litmus test. So that is one way to go.

And the other is to do the same exercise, but ex post, which is, write down the model ex post to justify what you're looking at, and then try to organize all of the things that you look at to make that case.

For example, in our case, we really want to basically look at the effect of education on a range of outcomes, separately for women and men. And then all of the rest is going to be circumstantial-- that it's really the effect of education, so we can check for possible direct effect of scholarship on test scores that might not go to education. It sort of gives you some discipline for what you want to do. But it's a great question.

So that's it. So this thing is called the Wald estimate, the simple division between the effect of the scholarship on the final outcome and the effect of the scholarship on going to school. The first numerator is the first stage relationship. The denominator is the reduced form relationship. We call it the reduced form relationship. And the beta hat is the Wald estimate. And it's the simplest form of an instrumental variable estimate where Zed is our instrument. And it's simple, because it is binary, so we can just compare treatment and control.

If you have understood this, and all the subtleties that go with it, you're done in the sense of having understood IV. And the rest is kind of building up on this insight.

We went through this quickly but without the benefits of the table. But remember, this is the effect-- for women, for example, this is the effect on having completed secondary school. 33% percentage points-- girls are 33% percentage points more likely to complete secondary school if they receive the scholarship than if they don't. This is the effect on cognitive test scores. The cognitive test scores of girls is 0.18 standard deviation, larger if they have received a scholarship than if they haven't. And so all I need to do is to divide one by the other to get the effect. So I would divide 0.185 divided by 0.33, and it's going to be approximately 0.6. A little less than.

So you can play with that. You can do it for man, you can do it for years of schooling instead of completing secondary school. You can do it for earnings. The tables are all there in the slides so you can do anything you're interested in.

So you can see that even a small violation of either condition for the validity of the instrument can result in a large bias. You can see it in the equation. It's because it's below. So it's divided by something which is less than 1. So any small bias in epsilon would be blown up by the size of the first stage. So it can result in very large bias for beta.

So it's pretty important to check the equation, and people tend to forget that, actually. Often. Or they will only look at the first half. Especially when-- both with people who start from [INAUDIBLE] and people who start from naturally assigned variation will be very careful to say that, indeed, we really have a good instrument because we have something that affects education. It also affects cognitive test scores. I'm quite confident about this estimate, so I'm confident that my reduced form is well estimated. But then they forget that intermediate link.

So what I want to do before going to look at formulas is to go over some examples and say whether it seems to you to be valid or not valid. And just to make sure there is both in here-- there are some that I-- at least, I think is OK and some that I think are not OK.

So here's the first example that's from a real paper by Guido Imbens. Doctors are randomly selected to receive advice to remind their patient that it's flu season and they should take a flu shot. I use it as an instrument for taking the flu shot on sick days-- for the impact of taking the flu shot on sick days. So that's often called an encouragement design, basically. My randomized variation is this letter that I send to the doctors, and I want to use the receiving letter-- the doctor has received the letter as an instrument for I have received a flu shot, and then my final outcome will be sick days.

So is it valid? Is it a good instrument or not a good instrument?

AUDIENCE: If the doctor reminds a patient to take the flu shot, then there could be a way that the patient takes less sick days because they feel confident they have a doctor looking after them.

ESTHER DUFLO: So that could be one. What would be a less stretchy one? Yeah.

AUDIENCE: If you tell the doctor to remind the patient, unless it's telling them to call them up, if the patient is there for another reason and they remind them of it, the patient is already sick for reasons unrelated to the flu shot because they went to the doctor who reminded them.

ESTHER DUFLO: Yeah, so that would be fine because it is still the case that if the patients have come anyway-- and it's patients that tend to be sick that will come anyway-- it is still randomly assigned whether or not the doctor has the letter or not. So it's in this pool of reasonably sick people at the time where they went to the patient.

AUDIENCE: So sick days for instance would just be the people who had gone to the doctor?

ESTHER DUFLO: Yeah, it would be among-- it would be in the set. Even if we took the whole set, the instrument might be weak in the sense that you might not have a very high predictive power because lots of people didn't go. But as long as we select a sample where there is actually effective variation-- so you've gone to the doctor once, but your doctor has that letter or doesn't have that letter. So the pool is good. Comparable.

AUDIENCE: I don't know whether-- if you're told, oh, it's flu season, you should get your flu shot and you're thinking about calling in sick, I feel like that [INAUDIBLE] be more likely to call in sick. You're like, oh, I guess lots of people are sick.

ESTHER DUFLO: So that could be one. I would still put it in the stretchy one, but maybe a little less stretchy. Yeah?

AUDIENCE: I think that in flu season you tend to get a lot of reminders anyway, even if your doctor wasn't assigned to remind you. You would just-- I mean, I know [INAUDIBLE].

ESTHER DUFLO: Yeah.

AUDIENCE: So that would affect how effective your instrument is.

ESTHER DUFLO: Yes, exactly. So we might not have a first stage. So the nice thing is we're going to be able to see that. So if we don't have a first stage, we don't have a first stage. So that's-- Yep?

AUDIENCE: Is there something to be said about doctors actually following your advice? Like do would you need to measure how often doctors follow my advice?

ESTHER DUFLO: Yes, so they might-- So exactly. That will also affect the first stage. They might or might not follow your advice. So if they don't-- which doctors are known not to-- then that would also make the first stage weaker. We'll be able to check that by the first stage. Yeah?

AUDIENCE: What if the doctors you randomly select and you advise them to do this, they don't follow your advice anyway. I feel like doctors are always [INAUDIBLE].

ESTHER DUFLO: Yes. Then you wouldn't have a first stage. So for all of these reasons, it might not be such-- the first stage might not be very large or might not be there at all. So if it's not there at all, you just go home and do something else. But if it's not very large, you might still tempted to use it. And then we really need to make sure that there is no-- that there is no other violation.

So you've tried. And in the stretchy ones, they are stretchy, but they had the right idea, which is, is there an effect of the letter over and above the fact that it leads some people to get the vaccines?

And what they show in that paper is that there is an effect of the letter, which is the doctors also say, by the way, it's flu season. Remember to take orange juice and wash your hands often, and all of these things. So all of these things might make people less sick. Not just with the flu, but with other things. They might be less likely to get the common cold, or pneumonia, or whatnot, that are not the flu. And any of these things will affect the outcome of sick day. Not through the flu vaccines, but through these other things.

And in particular, if it's compounded by the fact that the first stage is not very large, then we would kind of mistakenly attribute all of this reduction, which is due to any number of causes, to just a flu shot, which would lead to bias.

I want to follow up on that charter school example. So in Boston and many other places there are too many kids who want to go to charter school and there are not enough charter schools. So when a kid applies to charter school, the schools have to randomize who they take in.

So suppose you want to look at the impact of going to charter school. You could say, well, I'm going to focus on the kids who have applied to this school and use the lottery. I assume the lottery was well run to look at the impact of going to charter school. How does that look? You like it. Any opposition? Any-- Yeah?

AUDIENCE: [INAUDIBLE] they'll be using?

ESTHER DUFLO: In Boston, for example, charter schools are oversubscribed, so they have to randomize. They have to run a lottery.

AUDIENCE: And we are using it as an instrument--

ESTHER DUFLO: We're using winning the lottery as an instrument for attending a particular charter school, or charter school in general. Yep.

AUDIENCE: [INAUDIBLE] are the two groups you're looking at, the kids who wind up at charter school and the kids who entered the lottery but didn't?

ESTHER DUFLO: We're going to look -- exactly. We are going to look at the kids-- all of the kids who apply to charter school. Is there-- think, for example at a particular one. It makes the thinking easier. Imagine a particular one. All of the kids who applied to Bridge Academy-- Bridge Academy is oversubscribed every year. Not Bridge. It's called-- Bridge is what the-- well, let's assume Bridge Academy.

All of the kids applied, then some of them get in, some of them don't get in by lottery. And I'm comparing the people who win the lottery to people who lose the lottery. Yeah.

AUDIENCE: The students who apply are not representative of the entire population. And, out of interest, knowledge of the charter school is just generally more confidence that they'll be able to succeed or get in, as opposed to people who are-- as opposed to students from more disadvantaged backgrounds. There's no point in applying.

ESTHER DUFLO: Yes, so this is related to the question we had before. If it gives me a causal effect, it's going to give me the causal effect for the pool of applicants, which may or may not be representative of the larger pool. That is life. That's kind of what we have with samples. That's the sample. So we'll just take that as given. But that's a very good point. And that's something that has been leveled as a criticism against all of these studies.

AUDIENCE: The students who lose the lottery, there might be some psychological effect because they didn't get into the school. So then they're under performance at a non-charter school [INAUDIBLE] the fact that they go to charter school, seems to effect them psychologically as well as culturally.

ESTHER DUFLO: Yes, so this is kind of back to exactly to the point about winning the lottery might make you happy, and here, losing the lottery might make you depressed. So that might be an issue.

Other than that, it seems to me to be a pretty good instrument. And actually it's been used frequently. I think that needs to be taken into consideration, and the fact that it's a specific set of kids also has to be taken into consideration. But I don't see a big issue with those at all. In fact, Josh Andre [INAUDIBLE] both from the department, are working with those a lot. So that one would be, I think, largely OK, with your caveat.

Another one is kids are in schools that are randomly-- kids are in school and-- hold on. The sentence is not very well constructed. So a study that is randomized at the school level-- and kids are either in a school that is assigned to receive the pill or not, but not every kid actually gets the pill because they were absent on this day, or something like that. So let's say only some of the kids actually get the deworming pill, and others don't.

So first I estimate the effect of being a deworming school on my cognitive scores-- present in school, later earnings, whatever you're interested in. But then I say, well, let me actually instrument this. This is not the effect of being dewormed because only about half the kids end up being dewormed. So let me use the fact that I was in a deworming school as an instrument for actually being dewormed. In other words, I'm going to multiply all my estimates by 2. Yeah?

AUDIENCE: Wouldn't you need some kind of variable over-- if the school is dewormed, what percentage of students were actually there that day--

ESTHER DUFLO: Yeah, so suppose I know it. Suppose I know-- I have an individual level data set where I know whether the kid was actually dewormed and whether they are in a deworming school. So I can calculate for each school the fraction of kids who actually were dewormed, or I can calculate for the entire treatment school, what fraction of the treatment kids actually got dewormed. And let's say no one got dewormed in the control school. Then I have enough to do my reduced form and my first stage.

AUDIENCE: So to clarify, when we say dewormed, we just mean they got the pill?

ESTHER DUFLO: They got the pill.

AUDIENCE: Or they don't have any worms in them?

ESTHER DUFLO: They got the pill. That's the effect [INAUDIBLE], that's what we are interested in. Which is actually one for one, because the [INAUDIBLE].

AUDIENCE: You get the pill and you're dewormed, but you don't measure it immediately then, and then some kid who wasn't there that day when they were giving out pills comes back and is highly contagious and gives you the worms again.

ESTHER DUFLO: Exactly. So that would be the problem which is that-- on the other end, even the kids who did not get the pill actually get much better if they are in a school with other kids who get one simply because worms are so contagious that if you have worms, you give them to the neighbors. And, therefore, being in a treatment school has an effect irrespective of whether I got treated myself, because worms are contagious.

So that would not be OK here. It's pretty clear. It's an example-- any example with externalities, et cetera, are examples that are going to violate that link. Because there is a direct effect of being in treatment school and getting the pill. Yep.

AUDIENCE: So you asked me to keep the laptop example in mind.

ESTHER DUFLO: Yes. I want to go back to the laptop example. So now think of the laptop example. Would it be a good instrument providing the laptops?

AUDIENCE: No.

ESTHER DUFLO: So why would it not be a good instrument?

AUDIENCE: Feels like there are a lot of externalities associated with it. External effects-- like, it's not just effecting education. There's also a bunch of effects on how much time do you spend on the laptop as opposed to studying.

ESTHER DUFLO: Exactly. Odds are it might make you spend more time at-- it might be more likely that you actually go to school and stay in school-- doesn't drop out-- But also it might help you study, it might hinder your studies, it might do all sorts of stuff directly. So a laptop would not be a great example. Which are the ones that we went over? Sorry, go ahead. Go ahead.

AUDIENCE: Oh, just if you have a school where only a few kids have laptops, they're going to share them with the other students.

ESTHER DUFLO: Yeah, and they might also be-- if we randomize these laptops at the individual level, there might be an effect on the other kids. So that will create extra problems. Yeah.

AUDIENCE: So if there are just those two issues, what if we just gave them a laptop with really hard coded functions, like you can only do x, y, z, on it. They're all education. Would that solve some [INAUDIBLE] in terms of effect?

ESTHER DUFLO: Would it? Or would it not? For the effect? It would make sure it doesn't hurt them, but it would presumably help them. In fact, the channel through which you would have kids more likely to stay in school is that they will understand their lessons better and the like.

So you will have two effects going on from the laptop. You will have kids stay in school for longer, but also, if you want, the quality of their education improves. So it would still not be a good instrument. It might be a better program, but it would still not be a good instrument for going to school.

It might be a fine instrument if you're interested in the effect of cognitive scores on future earnings. You might say, well, the laptop, really the only thing they did is to increase cognitive scores, then you could use it as an instrument for cognitive scores. Yeah.

AUDIENCE: If we want to talk about meals-- so in that case meals would also be problematic because they might be of different nutrition levels than what kids would get otherwise.

ESTHER DUFLO: Exactly. So meals is another example that would be problematic. Certainly it would bring a lot of kids to school, but it also puts something in their stomach-- typically the meals have vitamins, and iron, and the like-- it'll make the kids more alert during the school day. They will learn better and we will have these effects smooshed up. Exactly. So meals would be another one. Thank you.

AUDIENCE: So in that case, could we have a direct estimation of having meals on--

ESTHER DUFLO: Exactly. Same thing with the laptops, by the way. Nothing prevents you from looking at the effect of a laptop on test scores. But you wouldn't want to do the IV. All it says is that's going to give you a very good estimate of laptops on test scores, or meals on test scores. That's pretty interesting, but you can't extrapolate from that that that's the effect of education. Sometimes you can, sometimes you cannot.

So in this case I don't think this would be a problem, because these are interventions that we'd be interested in anyway. In other cases, it might be a problem because they're not so interesting interventions. You just use them as a way of getting more education. But that's always the thing we have to go through.

So I mentioned at the beginning-- and we already sort of circled around this issue-- is that in reality often we are not really willing to make the assumption that the effect of going to school on test scores is constant, for example. So ideally we would like a sort of little i next to the beta-- beta i-- so as we know, we are not going to be able to estimate the causal effect for each person.

If we simply look at the reduced form effect, we know the interpretation of it is still the average treatment effect. But in this case, the simple calculation we did to show that beta is, in fact, the effect of going to school on test scores doesn't apply anymore. So what can we do?

So it turned out that the Wald estimate still has a causal interpretation, and there are fairly additional minor assumptions which I'll show to you in a moment. In that case, what the Wald estimate does, or the IV estimate when we go to IV estimate, is that it captures the effect of the treatment on those who are compelled by the instrument to get treated. So we call that the Local Average Treatment Effect, or LATE.

Just in case that is like, what? So "what" doesn't refer to the LATE thing. What refers to-- what did I just say?

So let's go back to our school example and consider the 1, 0 decision of going to high school or not. In principle, you can have four groups of kids. You can have the kids who would go to high school anyway, so in fact, we know what fraction we have. We had some kids who went anyway. You have the kids-- and we are going to call those the Always Takers. They will go to school regardless. They'll figure it out. OK?

We could have the kids who would not go to high school even when they are offered a scholarship. So this is our bench. There are very few among boys. We saw that most of them went. But among girls there are many people who just declined the offer of scholarship and don't go. Let's call this one the Never Takers. Regardless, they're not going to take the treatment.

And then there are a group of kids who would go if they are offered a scholarship, but they would not go if they are not offered a scholarship. Call these guys the Compliers. They are complying to your treatment. And then we have a fourth group of kids-- what is this fourth group of kids?

AUDIENCE: Those who don't go with offer?

ESTHER DUFLO: Yes, those who don't go with the offer, but who would have gone otherwise. Who are spiteful. So those who would not go if offered, but-- so the opposite. Not go if offered, but go if not offered. So please correct that in your notes. Those are the Defiers.

So that group is kind of weird.

[LAUGHTER]

So the relatively mild assumption-- sometimes it's not mild, but in most cases, it's relatively mild. But it's an assumption. Assume it doesn't exist. OK? Assume there are no Defiers. In that case, you can write a few lines of algebra-- that I will spare you-- to prove that the Wald estimate is the effect of the treatment for the Compliers.

The intuition is very easy, and the trick to the proof is very easy. It's that the first stage for the other two groups are 0, and you assume that group doesn't exist. So the first stage is entirely kind of moving these guys out. And, therefore we have the effect for them.

So to the point that you and you both made, which is when I look, for example, at kids who qualify for secondary school anyway, or when I look at kids who apply to charter school, you have to make one additional comment-- which is important-- which is, within this group-- I'm not even getting the causal effect for all of them. I'm getting the causal effect for the type of kids who is moved by me.

So with a charter school, for example, some kids are very determined. And if they don't get in, they will camp-- their parents or themselves will camp out next to the principal's office unless and until they get a spot. So this most motivated of the motivated will get it anyway. And then they're not part of it. So maybe the effect is still not for the most motivated of the lot.

There are very few-- in the case of charter schools-- there are very few never takers, because once people apply, presumably they are planning to go.

In our case, it's the same thing. The kids who manage to scrap the money anyway are presumably quite motivated. So my group is this kind of-- these are the poorest of the kids. And so the question you have to ask whenever you do an IV is, is it a group that I'm interested in? Because you know they exist. You know what the fraction is, but you don't know who they are in your sample. Nothing tells you that someone is an Always Taker or not because you don't know what would have happened to them if they had not received the scholarship. You just know it one way.

So the question is, is the group in the general line of interest, or not particularly? And that has to be argued. In a lot of cases it happens to be the group we are interested in with an instrument because we are sort of-- the instrument is often of the form that you have facilitated some group to get the thing.

So for example, in the case of scholarship, they are the people who are moved by my instrument, by having free school to actually go to school-- so consider a government. What could be their policy to improve secondary school? Well, it could be to make it cheaper. So the type of people who would go to school because of that policy would be very much like the type of people who go to school because of my instrument. So that would be nice.

But when you're using a very exotic instrument-- which happens more with using natural variation as sort of instruments-- sometimes we are like, well, do we care about these people? Like, that doesn't really-- is their treatment effect-- OK, I know I have an excellent estimate of the causal effect for them. But that might not be representative of the causal effect for the rest of the world. And I'm not so interested in them.

So that's the type of question that you have to ask yourself. So why don't we stop here. And then what I will take-- we'll take up exactly from there on Wednesday before going to experimental design, going over the formula, linking it to two stage square model more generally, OK?