Six Questions with Ted Miguel

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Edward (Ted) Miguel (http://emiguel.econ.berkeley.edu/) is the Oxfam Professor in Environmental and Resource Economics and co-Director of the Center for Effective Global Action (CEGA) at Berkeley. After a famous early paper with Michael Kremer had him known as "that worms guy", Ted has gone on to study a wide range of topics on African economic development, including health, infrastructure, ethnic divisions, violence (and even witchcraft), agricultural productivity, and vocational education. He has also been a leading voice in the movement towards greater transparency (helping popularize pre-analysis plans), and done important work on the environment and development before it was even trendy to do so.

1. *We normally start by asking about how the interviewee got into development economics. I saw a nice recent interview* (https://econreview.berkeley.edu/interview-with-professor-ted-miguel/) *you did for the Berkeley Economic Review in which you mention an early interest in global poverty and international relations, but a variety of fields you thought about that could take you there, and then the experience of working as a research assistant for Michael Kremer when you were an undergrad as really helping seal the deal. Despite the rise in pre-docs and field positions at IPA/JPAL, getting this type of first-hand experience of what it means to be a development researcher is hard to get, especially for students outside of the top programs. Part of it*

is not even knowing what could be possible – one of the things that surprised me most when starting teaching at Stanford was just the belief that students had that after an introductory class that they had something to offer to the U.N. or Grameen or an NGO, whereas as an undergrad in New Zealand the thought would have never occurred to me. What advice to you give undergrads trying to figure out whether development economics is a good fit and potential career for them?

EM: I don't think that working in development economics is for everyone. It's a very different field than many other parts of economics (think finance or industrial organization, which tend to be tied much more closely to corporate parts of the economy in rich countries for the most part). Spending time traveling internationally in low- and middle-income countries also doesn't appeal to everyone. But I do think that a pretty decent share of our undergraduate economics majors could develop an interest in a career in international development if they can experience the intellectual vibrancy of the field – which has been one of the most edgy and innovative corners of the social sciences for the last 30 years – and also understand that its an area where their talents can translate more readily into positive impact for other people around the globe. That combination is pretty hard to compete with, at least for a lot of my Berkeley undergrads (and it sounds like it was similar for you at Stanford). In terms of the point about when and how undergraduates would have something to offer a big organization, my general view is that they do! I know that energetic, smart and determined undergraduate students have added a ton of value to many of my projects.

2. *While I joked that you have now moved on from being known as "the worms guy", you have continued to follow that sample to now look at 20-year impacts*

(http://emiguel.econ.berkeley.edu/research/twenty-year-economic-impacts-of-deworming/)*, as well as had that work re-analyzed and critiqued as part of an infamous "worm wars* (https://blogs.worldbank.org/impactevaluations/worm-wars-anthology)*" debate. These are obviously two separate issues, so the first question is whether it is possible for an academic to move on and let their work stand for itself, or do you feel there is a responsibility to engage with and answer every query raised and critique? What have you learnt from the worm wars experience that affects how you engage with your other studies going forward?*

EM: I think replication studies and debates over interpretation are a normal part of the scientific process and I am happy that it is becoming more standard in economics and other related fields. The work of Abel Brodeur and others at the Institute for Replication (I4R) with their "replication games" is really moving the needle and increasing this work in a productive way. At the same time, that doesn't mean that every replication study is "right" or every academic critique has merit, and that is something that Michael Kremer and I (and our coauthors) experienced with discussion of our earlier work on the worms study in Kenya. While it was certainly not super pleasant at the time (especially given the media spotlight), I am glad that we took the scientific debate seriously and invested the time into our responses that the topic deserved, and I hope at the end of the day the scientific record is better as a result. As for the long-run results, the Kenya Life Panel Survey that gathers information on long-run deworming continues, and in fact we are in our 5th round and the 25 year mark at this point! And we intend to keep it going for a while.

3. *The related question is when do you think it is worth investing in long-term follow-ups? You have an Annual Review paper* (http://emiguel.econ.berkeley.edu/research/using-rcts-to-estimate-long-run-impacts-in-development-economics/) *on using RCTs to estimate long-run impacts in which you note that "long-run impacts may exist even in the absence of clear-cut short-run effects". But it seems extra risky to both funders and researchers to put in the effort to look longer-term at something that didn't seem to work as well as hoped in the short-run, unless there is admin data or some cheap way of doing so. But not doing so will give a biased view of long-term evidence. What's your current thinking on when to do a long-term follow-up?*

   EM: You've raised a super important but very tricky issue: ideally there would be tons more money for development economics research and we wouldn't have to make as many tough choices about where to allocate resources for long-run follow up studies. I think most funders and researchers think the way you lay things out here – basically, if there are no short-run effects then the likelihood of long-run impacts must be tiny – but our literature review brought up several cases where this was definitely not the case. So when should we invest in these studies? I think one rule would be to focus on studies that have high levels of importance for public policy choices, or key scholarly debates, and choose to fund long-run studies in those cases regardless of what short-run impacts look like. But perhaps it is naïve to think that this is how research funds will be allocated. At the same time, there are cases where a lack of short-run impacts – for instance, on program take-up or on a short-run outcome that is absolutely necessary for longer-term effects – could make long-run analysis far less interesting, so perhaps at least in those cases we could rule out some follow-ups.

4. *In your Berkeley Economic Review interview, you note that you decided to work on sub-Saharan Africa because it was the poorest continent, and had received relatively less research attention than Latin America and South Asia. This situation has changed a bit, and in part thanks to your efforts Busia is sometimes seen along with the ICRISAT villages as the most studied places in development. But we still see very few studies taking place in the poorest and most fragile African countries like the DRC, Sudan, Somalia, Burundi, Chad, Central African Republic – relative to Kenya, Ghana, Uganda, Tanzania and Ethiopia – and relatively little in Nigeria given its population size and number of poor. One view is that the marginal return to most micro-development research is higher in slightly more stable places with a functioning government, in which policy advice on health, education, and industrial policy may have an impact, whereas first-order questions of security and state capacity that we have less to say about may be more important in some other countries. A second view is that the lessons from the poor countries we do work more in will translate well to these other countries, and so working where there is an established infrastructure for research is the efficient outcome. But another view is that the set-up costs of new research are just too high in these places, but that development economists would have a lot to say if we worked more there. What is your take on this, and should the profession be rewarding more work done in understudied places?*

EM: I do think that, from the social planner's perspective, there could usefully be more coordination on where scholars work to prevent there being, say, 20 times more research on Kenya than on the Democratic Republic of Congo (DRC), despite the latter having a population that's twice as large. (That's just a guestimate of mine on the quantity of research articles published on the two countries but it may not be too far off!) To get there, I think that large research funders – and here the World

Bank could be an important mover – could allocate additional research funding to under-studied contexts. That could make a difference: when research donors want to fund something today, there are often large numbers of ambitious younger scholars willing to take up the challenge. I think it doesn't happen on its own for some good reasons, though. First off, building up research infrastructure is challenging – and risky. Who wants to spend three years cultivating local relationships, permissions, hiring, training staff and the like only to get everything wiped away when an insurgency sweeps through your town? And second, there are real returns to scale and scope from building on existing research infrastructure. The example of Busia and Kenya more broadly is a great one. At this point, there are multiple research organizations for scholars to partner with, thousands of trained enumerators, and scores of academic researchers with good publication records and experience with international research partnerships. If you're a graduate student who wants to put together a job market project in finite time, it's almost a no-brainer to work in Kenya rather than DRC unless you are explicitly interested in conflict or instability related topics, or particular historical episodes relevant to DRC. (One last thing – some of the scholars who have succeeded in setting up research projects in the DRC and other politically unstable societies have been some of the most ambitious and accomplished development economists of recent cohorts. So there can be very high returns to doing something different in a new setting. But it is professionally risky.)

5. *One of the recent areas you have been working on concerns scaling up and general equilibrium effects. Your recent Econometrica paper* (http://emiguel.econ.berkeley.edu/wordpress/wp-content/uploads/2019/11/ecta200500.pdf) *impressively*

studies the general equilibrium effects of one-time cash transfers in Kenya by randomizing over 10,000 $1000 cash transfers to poor households across 653 rural villages, finding a large local transfer multiplier of 2.5 and little inflation. While a fiscal multiplier is something we don't question much in the abstract in macro, it seems a bit like magic when we start looking closely at a micro level – where somehow in a mostly closed economy, the whole village economy grows in real terms because poor people have more to spend and more seems to get magically produced from the same inputs. You speculate that one possible reason is that there is a lot of "slack", where businesses are just sitting around waiting for customers. Did you see anything qualitatively that provided a specific example of money multiplying? Do you see this as an argument for expansionary fiscal policy, or is there something specific about the form of these cash transfers that prevents them causing inflation when more government spending would do so?

EM: This is one of the projects that I am most excited about at the moment, and we have a bunch of follow-up projects in the works. In particular, we are working hard to understand the "slack" or under-utilized capacity in small firms in rural Kenya that features so prominently in our recent 2022 Econometrica paper. There were certainly lots of anecdotes and examples of slack that inspired us: for instance, the most common type of light industry in the region of western Kenya that we study is basic food processing in a grain mill. Basically local farmers walk or bike sacks of the corn (maize) that they've grown over to a local mill and grind it up for maize flour, which they then turn into their staple food. But there was clear evidence of lots of slack in those mills: the mills typically have a large machine, perhaps larger than they really need, and there is usually someone sitting at the mill waiting for customers even though there is often a gap of 15 or 30 minutes between customers. But the worker can't easily go off and do something else or else they'd miss customers. So basically both capital and labor are often underutilized. But when transfers land in the local area, boosting local agricultural production (as well as purchases of grain from elsewhere) all of a sudden that grain mill is much busier and can produce a lot more without reaching the point where they would have to raise prices. It's still just one machine and one worker but they can easily grind 10 or 20% more grain in a day. That's the idea behind the slack finding, and there are other related examples in retail and other sectors. In terms of how portable the results of that study are, in other words, related to external validity, that is really an open question in our view, a point we make the conclusion to the original paper. The underlying slack in the economy could be very different in urban areas, for instance, where there is a much great density of economic activity and lower transport costs, leading to more of an inflation response than we find. Consumers there could also have lower marginal propensity to consume the cash, which macro models would also tell us would lead to a lower multiplier. In all there is a lot still to learn about the broader spillover and GE effects of cash transfer programs in other settings, and I hope we can contribute to that in our future work. For instance, we are in the field right now trying to measure the extent to slack in urban Kenyan settings, and will compare it to what we find in rural areas.

6. *What are you most excited about in terms of your own research over the next 5 years? What should we be on the look out for?*

EM: I am someone who likes to move into new areas every few years, and I've been lucky to be able to do so. I love learning new literatures and tools, and in fact it's one of the privileges of our job that we are able to do so. Right now I have two new research agendas that I'm trying to move forward a lot in the coming years. The first is on the economics of aging in Africa. People don't realize just how many more elderly folks there will be in Africa, and also in other low and middle income regions, in the next few decades: in Africa there will be three times more people over age 60 in 2050 than in 2020! But there is little research on policy relevant questions, and that's part of what I would like to contribute to with my team. Second, I have become really passionate about working on humanitarian issues related to refugees and displaced issues. Once again, the research literature has traditionally been small in this field: humanitarian organizations had to work fast and often didn't invest much in learning rigorously what worked or didn't with their programming. But that is changing fast and I'm really excited to be part of a new wave of research in this field, bringing in RCT methods as well as longer-term longitudinal data collection, so far in both Jordan and Kenya. But I think much more should be done in this area, especially given that refugees in low income settings are among the people with the most difficult lives and life changes in the world.

Here are our previous *Six Questions with* interview series:

- Six questions with Chris Udry (https://blogs.worldbank.org/impactevaluations/six-questions-chris-udry)
- Six questions with Rohini Pande (https://blogs.worldbank.org/impactevaluations/six-questions-rohini-pande)
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