Berkeley Economic Review

VOLUME XI

| Content

Professor Interviews

Research Papers

Cover

| Policies

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| From The Editors' Desk

Dear BER Reader,

When Berkeley Economic Review began in 2016 as a humble student organization at UC Berkeley, none of its ten members could have imagined the astronomical growth it would experience in only a short time. BER has since become a global phenomenon: our 90+ staff members, tens of thousands of monthly readers, and countless journal submissions come from every corner of the planet.

BER has changed immensely over the last five years, but the world around us has changed even more. No event in the post-war era has reshaped and challenged the global economy as rapidly as the COVID-19 pandemic. In contrast to the optimism of continued economic growth in 2016, the world today continues to experience substantial unemployment and dramatic loss of human life due to the virus.

Nevertheless, undergraduates around the world have continued to produce exceptionally relevant and original economic research during this time. Our Peer Review team had the honor of selecting two of the very best of such research papers, which can be found on the pages that follow. From the influence of foreign aid on preferences for democracy in Tanzania to the impact of famines on long-run development in India, these papers rigorously answered questions of great importance to economists today and made invaluable contributions to the existing literature within their fields.

It is with great excitement that we present to you the 11th volume of Berkeley Economic Review.

Sincerely,

Charles McMurry & Parmita Das
Editors-in-Chief
Berkeley Economic Review

Professor Don Moore

Interviewed by ***Peter Zhang***

Professor Don A. Moore is the Lorraine Tyson Mitchell Chair in Leadership and Communication at Berkeley Haas and serves as Associate Dean for Academic Affairs. His research interests are confidence and overconfidence, with a focus on forecasting, judgment, and decision making. BER Staff Writer Peter Zhang interviewed Professor Moore over Zoom on March 17th, 2021. Interested readers may learn about research opportunities on Professor Moore's website.

Interviewer: I would ask you for your personal background but I know the readers can find a wonderful "Self-Aggrandizing Autobiographical Sketch" on your website. Could you talk a bit about what brought you to your research?

Moore: Sure. I could tell you a long winding story about my personal development, teenage angst, and all of that, but I try to tell an interesting version of that in the book. I'll give you the short, nerdy version:

Most of my dissertation was sort of boring, but there was this one curious finding that emerged from it. Chasing that one down, and figuring out what was going on, led me down the rabbit hole that became my fascination with overconfidence. I've been pursuing that mystery ever since. I think I've solved the question that my dissertation posed, but that led me to other questions, which have proven a professional obsession.

Interviewer: Let's talk a bit about confidence. You've recently written a book on it called Perfectly Confident. For the young Professor Moore and for most of us, we look at Elon Musk and come to see confidence as an unequivocal good, a prerequisite even, for success. What's wrong with this view?

Moore: So many things are wrong with that view! Thank you for that question.

The first thing that is wrong with that view is that it selects on a dependent variable. It's worth asking: what is the population of entrepreneurs or would-be entrepreneurs from which Elon Musk was drawn? If it is the case that the most confident are the ones that choose to dive into this risky endeavor and their confidence is not perfectly calibrated with their promise or prospects, what we will see is that there is variation in outcomes.

Of course, there's a lot of difficulty in predicting outcomes. The entrepreneurs don't know, the venture capitalists don't know—if the VCs knew then they would have a better hit rate on their investment when in fact nine of out ten of their companies don't go IPO and don't make them lots of money, there's room for them to improve—it's hard to predict how entrepreneurial ventures are going to perform.

We've got variations in outcomes, with a lot of new ventures going belly up, failing. Those who are confident enough to get in and give it a try anyway will, on average, just due to adverse selection, be overconfident. If, from that set, you just pay attention to those who hit it big, you're neglecting the entire group who gave it a try, most of whom fail.

So if you aspire to be like Elon Musk, if you could jump straight to the multi-billionaire part, that'd be fine. But there are steps ahead of there that you have to achieve, and if you're not well-calibrated in your confidence you're running the risk that you're making a mistake. That you're sinking too much of your precious time and money into a venture that will ultimately fail and that will tar you as having been overconfident.

Interviewer: I want to explore the idea of confidence and the dangers a bit more. Some of your recent research suggests that confidence is contagious. My question: can people with malicious intent—casinos, advertisers, swindlers—manipulate our confidence?

Moore: Oh, man. Nefarious actors attempt to manipulate our confidence all the time. They run the gamut.

In the talks that I give, I often show a slide with a report by the National Lottery of the United Kingdom on optimism. If you're going to sell your customers negative expected value bets, you would like them to delude themselves about their prospects of winning. You'd like them to keep sinking money into this losing prospect that's profitable to you as the seller. You want them to keep being optimistic. That is an institutionalized swindler.

But there are also confidence men who attempt to gain our confidence so they can make their way into our wallets, making us believe them and pretending to be more confident then they deserve to be. That is one of the ways that they play this social game of confidence, where they try to display more confidence than the situation actually warrants, that they deserve to have. They're putting on a show in which they're attempting to gain our trust and build our confidence in them.

Interviewer: I think most of us would agree that, at a personal level, overconfidence is dangerous. But someone might reasonably ask: why does this matter at a macro level? Surely, institutions like companies and governments aren't vulnerable, are they?

Moore: They're highly vulnerable.

The cemetery of powerful corporations that have gone bankrupt is full of confident leaders who were sure that their positions were unassailable. Kodak and Blockbuster Video were once flying high and didn't think that they had to bother with pesky rivals in digital photography or streaming video. Their overconfidence proved their demise.

Lots of corporations get themselves in trouble with bad beliefs, bad theories about confidence, and its role in their success. Like the rest of us, corporations often fool themselves into thinking that more confidence is better. That's because in so much of life we observe a correlation between confidence and performing, they think: "If only I can jack up the confidence of the people at my company, with big, hairy, audacious goals, then we will triumph. Mmm!"

They wind up overcommitting to customers who are ultimately disappointed by what they failed to deliver; they introduce aircraft to market ahead of the requisite safety checks and wind up killing their passengers and customers. Companies and governments get themselves into all sorts of trouble by pretending to be confident when a wiser, well-calibrated judge would not have been so confident.

Interviewer: Could you elaborate a bit more on the danger for governments? And maybe here you can also tie in your work with the Good Judgement Project.

Moore: I see that at a couple levels.

One has to do with contests for leadership, most notably election campaigns. Voters often have very little to go on and will attend too closely to what a candidate says. When a candidate sounds confident, when they brag about all they can achieve or Making American Great Again, or even bringing a new tone of bipartisanship to Washington—pfft—it sounds great. And if it is the case that those who are more confident on average are more capable and can deliver more, it might not be crazy for voters to choose the more confident candidate.

But in doing so, we will also guarantee that we're selecting the overconfident candidate, that they will disappoint us, and that they can't actually deliver all the hope and change they make us believe is possible.

So that's just part of the political game. Voters could get smarter about it if they attended more closely to candidates' track records of achievements and took their campaign promises with a little more of a grain of salt, understanding the complex political dynamics at work.

The other level has to do with not the elected officials who are running the show but instead the mid-level managers—those who are running agencies—in their attempts to help the government get better at what they do.

Like every institution, effective policy planning depends on good forecasting. You made reference to the Good Judgement

Project. That was an attempt by IARPA—the Intelligence Advanced Research Projects Activity, which is to the intelligence agencies what DARPA is to the defense Department—to help get better at forecasting. They were interested in forecasting geopolitical events like the fall of foriegn presidents and financial crises around the world.

But the truth is that every policy decision—in fact every decision—depends on a forest of its consequences. My involvement with the Good Judgement Project came through my interest in confidence and overconfidence and the value of having well-calibrated confidence from making good confidence.

Have you read Superforecasting?

Interviewer: Yep.

Moore: Then you know the central role that self-doubt, questioning, and humility played in the superpowers of the superforecasters. One of the things that made them super was their willingness to doubt themselves, to question their assumptions, to go back and revise forecasts, and to pay close attention to the evidence even when it suggests that their assumptions are wrong.

Interviewer: Daniel Khaneman, an enormous figure in behavioral economics, has said in interviews that overconfidence is the most harmful of the biases, often precisely because of the bureaucratic misjudgements that you mentioned. But he also seems to be a lot less confident in a solution. In concluding Thinking Fast and Slow, he bemoans how our slow-thinking rationality fails to adjust our automatic biases in precisely the moments where we need it the most. So, he directs his book towards third parties—towards the critics. You seem to direct your book to the decision-makers themselves. What is your prescription for correcting overconfidence? Do you agree with Khaneman's pessimism?

Moore: As someone who is down in the weeds on overconfidence, I have many thoughts and they relate in part to different forms of confidence.

I think the type of overconfidence that Khaneman identifies as getting us into the most trouble is overplacement—when we think we're better than others and we're not. That is the overconfidence that leads would-be entrepreneurs to cash out their investments and ruin their marriages for a venture that will ultimately fail. It's what gets us into losing wars. It leads to enormous amounts of wasted efforts and tragic outcomes where we undertake projects that are collectively destructive of value.

I think there are useful antidotes to overplacement, and overplacement is not universal. There are predictable circumstances in which people think that they're worse than others. When I ask my students how they think they're going to perform on a test of Russian literature or plants of the Sahara desert, everyone in the class thinks they're going to be worse than average. So overplacement is not universal.

By contrast, I am much more ready to concede Khaneman's claim to hardwired universality on overprecision—the excessive faith that we know the truth. That is the sort of overconfidence that gets forecasters into trouble, when they're too sure that they're knowledge is correct. And even though overprecision is pervasive—in my research I get it almost every time I look—even there I'm not ready to throw in the towel.

I think of people as flawed but corrible. There are big differences in the size of the bias depending on how you ask the question. You can elicit people's confidence in a way that really exacerbates overconfidence. If you ask, for instance, "we're going to make our targets, right?," the people who work for you will say "yes, boss."

Much better is to say, "how likely is it that we will complete project X by this date, by this date, by this date?" You get people to think through the full distribution of probabilities. You force them to think, "how likely is it that I'm wrong about this forecast?" You still get some overprecision, but it is so much less. And, you get so much more useful information out of the process.

Interviewer: Making ourselves quantify our beliefs is one way to mitigate overconfidence. Are there other ways that we, as individuals, could adjust our own confidence?

Moore: The simple prescription there is to ask yourself why you might be wrong. There are a lot of ways to do that.

One of the easiest is to accept the gift offered by our rivals, critics, and enemies. Listen to their critiques. You have something to learn there. It's possible that they're just haters trying to tear you down, and there isn't substance to their critiques. On the other hand, it's possible that they're on to something, and understanding their criticisms can provide useful input that helps you get stronger.

Companies do this. When the boss has the courage to call a premortem to discuss why their favorite plan is likely to fail, it gathers people with the explicit purpose of discussing "what's wrong with my plan, and what are its greatest weaknesses?" Thinking hard about those criticisms and whether you can somehow insulate your plan or hedge against your greatest risks—that will help you better calibrate your confidence and better protect yourself from weaknesses.

Interviewer: In the book, you tell the story of Alfred P. Sloan, a CEO of general motors that purposely sought out disagreement. You also mention early in the book that you did debate in high school so maybe you can relate to this. In my experience, debaters—some of the people most exposed to disagreement and diversity of thought—tend to also be some of the most stubborn and absolutist people. Open-ended question: what do you think is going on here?

Moore: I remember those people in debate too!

Debate should be good for helping people think about different perspectives. You're assigned to a position and have to argue as persuasively as you can from that position. It should help people consider the opposite perspective. But it also develops in them a penchant for passionate argumentation which isn't necessarily compatible with a balanced view.

There are a number of ways for me to come to your invitation. One is to wonder about how one can persuade those who disagree. In particular, I've thought a lot about this in the context of political partisanship. You encounter someone who has beliefs who seem so foreign and so misguided, but you have the chance to talk to them: what's the right thing to do?

Telling them that they're insane and QAnon is a baseless conspiracy theory is not going to win you a lot of allies, even if it's true. The more successful interpersonal strategies are cousins of the effective intrapersonal strategies, where you come at the problem sympathetically, in an attempt to understand rather than persuade. The other person thinks of themselves as rational and well-intentioned just like you do. They're living in a different information environment.

To understand is to forgive. Understanding what they're paying attention to and the information they are basing their judgements on can clarify why they've come to their judgements and may open the way to useful dialogue.

Interviewer: Shifting gears a little bit—economics in recent years has undergone a big shift in becoming more rigorous and empirical. Could you tell me a bit about an experiment you've performed? Maybe a personal favorite?

Moore: Wow, lots to talk about.

I could tell you about my analysis of the survey of professional forecasters, identifying overconfidence in forecasts of economic outcome, but that's not an experiment I ran.

If you want me to tell you about an experiment, I might tell you about one I just ran with a doctoral student, here at Haas, named Sandy Campbell. We began with some real world data from a game show [the Million Dollar Money Drop] that provided high-stakes decision-making that made it possible for us to look at overconfidence in the wild. Were these game show contestants overconfident?

The answer appears to be yes. That's evidence with high stakes. It's not a purely incentive-compatible mechanism that they're using, it's a linear payoff scheme which has some problems.

I have other data. The stakes aren't quite as high, but in my MBA class I give multiple-choice exams where I invite students to report the probability that each of the given options is the right one. I reward them with a quadratic scoring rule that makes it optimal for a rational student who wants to maximize their grade to honestly report their confidence on each of the answers, so I can analyze those data. Much like the Million Dollar Money Drop, that also makes it look like people are overconfident—overprecise in their judgement, too sure that their right.

But, there are all sorts of differences between the Million Dollar Money Drop and my class. Sandy and I wanted to understand the degree to which the payoff scheme might have influenced the ways that people responded. So we just ran an experiment where we played a quiz game with people in our study, and we randomly assigned them to either the linear payoff scheme like the Million Dollar Money Drop or the quadratic rule like in my class and asked the question: does their degree of overconfidence vary with the payoff scheme?

If they were perfectly rational it should. In reality, it doesn't. It made almost no difference. That's helpful for making the case that the data from the game show and the data from the class are indicative of people's beliefs. They are not driven as much as rational actor theories would imply by the payoffs scheme.

Interviewer: I know you're involved with BITSS and I see on Twitter that you are a big fan of reproductions. Can you tell me a bit about research transparency and reproducibility?

Moore: I am inspired by the changes going on in socal science and the ways in which we're stepping up our game to pre register our studies to post our materials. I think that it portends good things for the future of science and I try very hard to abide by the highest standards of reporting and transparency in everything that I do. That includes: pre-registering studies before I run them; posting all the data and materials afterwards; sharing the analysis code when I can; using a rigorous threshold of statistics significance (I'm on this paper arguing that everyone should use a significance threshold of 0.005 rather than 0.05 and I try to do that in my studies)

I have played the gadfly role within my institution, the Haas School of Business, where it's my job when someone comes up for promotion or tenure, to note whether they have observed good scientific practice in their work. Whether they're posting data and pre-registering their studies. I see the trends as positive, but we have a long way to go.

Interviewer: In thinking about the dangers—p-hacking or overlooking confounding factors—how do you think that relates to your research on confidence? Do you think it's an example of wishful thinking?

Moore: Eh, yeah. I can make that connection. I'm not sure it's a real strong one. I do think that many of the doubters and skeptics of the need for change—the "old guard," who have stood in the way or put the brakes on moves towards open science—are too sure about the reliability of prior published results. My willingness to ask, "how might I be wrong?," makes me skeptical of my own results and skeptical of other people's results. Recent replication efforts should lead us all to suspect that the published literature has entirely too many false positives in it. It would be overconfident and gullible to think that just because it's published, it's true.

Interviewer: Since this is probably going to get published around finals week, for students like me, what advice would you give to help us calibrate our expectations and succeed?

Moore: When it comes to expectation and performance on exams and grades, that's a place where the best students—students who get into institutions like UC Berkeley—often motivate themselves with what psychologist Julie Norem has called defensive pessimism. That is, imagining failure, envisioning catastrophe, thinking about how embarrassing it would be if you failed, and thereby motivating yourself to work hard to study and prepare for the exam.

So, take heart, calibrate your confidence, study as much as you need to. Disaster is not imminent. But also don't party too much the night before!

Professor Ellora Derenoncourt

Interviewed by ***Ria Bhandarkar***

Professor Derenoncourt is an Assistant Professor at the Department of Economics and the Goldman School of Public Policy at the University of California, Berkeley. Her work focuses on the role of the federal minimum wage policy on racial disparities in earnings and Black mobility during the Great Migration.

Interviewer: Thanks so much for agreeing to meet with me. My first question is what made you decide to study economics? Why does the intersection between economics and public policy interest you?

Derenoncourt: Great question. I got interested in studying economics from being involved in the labor movement and labor organizing. I was involved in college in these student labor solidarity groups. I got very interested in the distribution of resources and power between workers and their employers. I didn't know that that was economics. I didn't know that economics could be used to study inequality so it took my many years of finding the right mentors and reading the literature to realize that.

Interviewer: How did labor organizing influence your research today?

Derenoncourt: It gave me an insight into low wage work and all of the ways that power determines people's experience of their jobs. The vast majority of people don't have a lot of power in their workplace and that actually feeds into wages, benefits and the quality of jobs people have. If you just study economics in the Econ 101 sense of supply and demand you might miss that because there, the wage is just set based on supply and demand and there isn't any room for power. But now we see more and more that labor economics recognizes that role that employers can have in wage setting power over their workers so I feel like I got an early start to understanding that just through organizing.

Interviewer: A lot of your research is specifically about racial wage inequality and the federal minimum wage. In Econ 1, students learn the very basics about the minimum wage and are assigned to read your work to go deeper into the issue. Minimum wage policy has become a very politically controversial issue in recent times. Are there any key aspects of it that citizens and young economic students should know more about?

Derenoncourt: Yeah that's a very timely topic. It was funny to hear you say that it's become politically controversial whereas I would actually say that it's actually become a lot less controversial. You used to have economists say maybe 60 years ago or so that it is defying the law of gravity to suggest that a minimum wage wouldn't destroy jobs and wouldn't result in massive job loss, that that is a scientific law that would have to be overturned to have a minimum wage not be catastrophic. That is based on economic theory but then we've had this empirical revolution in economics where people went and tried to look at actual experiments and used data to answer the question: when a minimum wage comes in is there job loss? Time and time again the answer seems to be if anything not much. The effects of the minimum wage on employment are small. That means we have to rethink our basic models of the labor market.

Interviewer: Do you believe that the COVID-19 pandemic, since you talk about the minimum wage becoming less controversial, has further highlighted racial disparities in wages and employment?

Derenoncourt: I think that that came up in the policy conversations around the relief bill where even Democratic senators did not want to see the minimum wage in the relief bill saying how can you do that if there's job loss. Of course there was a massive reduction in employment during the pandemic at the same time. Another component of my research studies minimum wages that companies set in their own firms and that apply across the entire country wherever their companies operate. Amazon is a big example of this, as well as Target and Walmart. Those companies have their own minimum wages and many of them continue to increase the minimum wage during the pandemic. Best Buy and Target, they both moved to a $15 minimum wage in the last year or so. So it kind of suggests that there's probably room to go in terms of increasing the wage if these companies that are always sort of saying Amazon is for minimum wage but other companies have been against it and yet they're also increasing their own minimum wages to way above the federal.

Interviewer: What research are you working on now? How are you collecting data?

Derenoncourt: One research product that I'm doing right now that I'm very excited about but has a very painful collection process is a question on the racial wealth gap going back to 1860. A lot of people talk about the racial wealth gap. It's many times bigger than the income gap. So the median white to Black wealth ratio is 10 to 1. The typical white person has ten times the wealth of the typical Black person. But a lot of studies of the racial wealth gap have only looked at it in the last few decades. In this study we're going all the way back to before the start of the civil war when you still had slavery in the United States and we're trying to create a survey over that full time frame. The purpose is we can already show with the data collected so far is when you start with one group with 0 which is how bBack americans were. They didn't have a right to accumulate property when they were enslaved and you compare them to a group that has had decades and decades to accumulate wealth. That starting difference, it's like a wealth ratio to infinity. Even under ideal conditions where both groups have equal opportunities to accumulate after emancipation and freedom, you would still have a 3 to 1 wealth ratio today meaning like it would still be really really big. What does that mean that means that you, actually we, can tinker around with all kinds of policies and say there's a savings rate difference so how can we incentivize better savings among Black Americans or how can we incentivize more of them to invest in stocks or how can we close the home ownership gap. You can do all that and you still won't come close to closing the racial wealth gap. It's almost a law of growth that under those conditions we won't see convergence for 100 plus years more. That actually gives motivation for thinking about reparations. That's one policy that tries to address the vast differences in starting conditions. For this project, we are going back to tax records from individual states and digitizing these. In many states in the South during the Jim Crow period when everything was segregated, they actually collected statistics separately on Black and white populations so you have wealth statistics for Black populations in the South and white population in the South in the 1890s and early 1900s, and we're actually going and digitizing all of that. It's kind of an amazing resource coming out of very bad practices and institutions, but it's data we can use and think about what we can do in terms of reparations.

Interviewer: So you talk about reparations and certain incentives as policies. Do you think some bigger institutional policies relating to education or healthcare would also be useful for closing the racial wealth gap?

Derenoncourt: Absolutely. Health disparities are also one of the most striking. I think the way I would frame it is it's useful to think about a set of policies that close inequality in general. Those policies are going to close racial gaps because of where Black people are in the distribution. So Black people occupy the lower part of the income distribution, health outcome, and education so anything you do that brings up people who are disadvantaged and at the bottom of these statistics is also going to disproportionately benefit Black people. So there are examples of these policies like the minimum wage or greater accessibility of health care or things like a wealth tax that would work on the opposite end where you have mostly the top part of the distribution as all white. Their returns to wealth are extraordinarily high and that would help with the racial distribution as well.

Interviewer: Thanks so much for your time and insights. What resources do you recommend for students who want to learn more about economics and inequality?

Derenoncourt: There are various groups that highlight research on inequality so I would look into those resources. One way is through social media because a lot of these organizations have social media like Twitter for example but I would mention the Economic Policy Institute. They are a group that highlight research on inequality, as well as the Washington Center for Equitable Growth and this new group that is called Economists for Inclusive Prosperity. They're a group that is also connected to this awesome open source textbook called Core and hopefully they will replace the standard Econ 1 textbook because it would talk about things like wage setting power of employers and why a minimum wage wouldn't actually cause unemployment.

Interviewer: Thanks again for your advice!

Graduate Student Nicholas Otis

Interviewed by ***Chazel Hakim***

Nicholas Otis ("Nick" throughout this interview) is a current graduate student in health economics at UC Berkeley. His research interests revolve around developmental and behavioral economics. I recently had the opportunity to meet with him and discuss his current work on forecasting and other topics of interest.

Interviewer: First off, could you briefly discuss your educational and professional background before coming to UC Berkeley?

Otis: I did my undergraduate studies at McGill University in Canada and also a master's degree there. After that, I went and worked as a research assistant for Johannes Haushofer at Princeton University. I was based in Princeton and then also spent time living in Kenya working on his project.

Interviewer: What got you interested in going to Kenya during your research assistantship?

Otis: It was kind of a coincidence. Johannes had a bunch of projects in Kenya, and I had been really interested in his work. He had a project, for example, that was about looking at the effects of giving people unconditional cash transfers, and he provided evidence that you can observe the effects of the cash transfers on people's salivary cortisol, which is a biological measure of stress. I thought, "That's pretty cool. I'd love to work with this guy." I applied to be a research assistant, and I got the job. Part of my work was to just sit in a room somewhere and work on statistical analysis, and the other part was to go and get things done in Kenya. So it's kind of a happy coincidence.

Interviewer: I see. So then what drew you to the economics field specifically? And why did you choose UC Berkeley to continue your studies?

Otis: When I started undergrad, I thought I'd be a philosophy major. I remember I took a few classes, but I was having trouble seeing how these courses would result in a tractable path to applied issues in moral philosophy, which is what I was interested in. I remember reading some early behavioral economics work by Daniel Kahneman and others (Kahneman et al., 1997) that connected the utilitarian foundations of "old-timey" economics to some of the more modern uses of utility, as in utility functions. That introduced me to the world of economics. I took a few economics courses, and I was excited by how much emphasis there was on causation, as well as how rigorous and applied it was, which was kind of missing in some of the other courses I was taking. And so I was basically sold on it.

In terms of choosing to go to UC Berkeley, Berkeley has an amazing group of development economists and an amazing group of behavioral economists. I'd also never spent much time in California, so that was also a draw. So it was mostly the people that I wanted to work with here, and I know that Berkeley's economics and economics-adjacent programs were really good in the areas that I was interested in.

Interviewer: Let's move on now to your current research. Most of it focuses on the topic of forecasting, specifically the forecasting of experimental results. Could you first discuss how you got interested in this topic?

Otis: Yeah, of course. When I was living in Kenya, as part of my research assistantship, I had this side project (i.e. this article, which recently came out in PNAS). We were giving people tiny cash transfers—just a few dollars worth—and would vary the frame that accompanied the cash transfer. We would tell some people, "You get money because you're a lower-income individual and you're eligible for welfare." Or we would say, "We're giving you a few dollars because we believe in you. We believe that you can empower yourself and choose your own directions in life." Or we would say, "We're giving you money because we think that you can use it to help your community."

So we're varying the frames associated with these cash transfers, but we already had an idea of what we thought might work. And I remember we were doing some formative qualitative research where we would talk to some of the individuals that were similar to the people who would later receive the cash transfers. These individuals seemed to have pretty strong priors about which of these framings would be effective and which wouldn't be. That got me to think that it would be cool if there was information in people's predictions or beliefs about what interventions would work or not. Something like this could be valuable to development economists and other people doing applied work who often have to decide which policy to implement with limited information about what works or what doesn't.

If people could guess what will work because they have contextual knowledge, then that would be really helpful. And in that initial work, we found that people's predictions for these different framings for cash transfers provided more accurate estimates of the causal effects than moderately sized pilots with a bit over a hundred people. So, in short, asking 25 people provided a better prediction of the causal effect of the treatment than a small pilot, which people would often run as an alternative way to gather preliminary evidence. That's how the work started, and then I kind of expanded from there.

Interviewer: Your most recent working paper, "Forecasting in the Field," does try to expand on that work you just talked about. Could you discuss what questions you're attempting that answer in that paper, and how you're going about answering those questions?

Otis: Here's the kind of big-picture motivation for that paper. There was some really encouraging evidence from Stefano Dellavigna and Devin Pope, who had run this experiment on Mechanical Turk. They had people predict the results of a bunch of different interventions to try and motivate people to exert costly effort: the people had to press the keys A and B over and over for a number of minutes. And they found that, in this massive experiment with around 10,000 people and many different treatments, the people that were similar to the intervention recipients were able to do a really good job of ranking which interventions would be more or less effective. If you look at their average predictions of different interventions, those predictions did a really good job of ranking things.

And that was sort of the foundational paper in forecasting the causal effects of interventions. It was an inspiration for a lot of the things that I'm working on right now. Stefano and Devin had this really clean setting where they were running this pretty tight experiment that was in an online "laboratory-like" setting. Things are relatively controlled, the interventions are pretty straight forward, and there's this very provocative result about the accuracy of people's beliefs. And so I thought, "Let's see what happens if we try and take that same idea—that people on average might be able to predict which kinds of interventions are most effective—let's look at it in an applied setting like field experiments in Kenya."

I then spent a lot of time figuring out which projects I could work with to collect predictions. The hope was to collect predictions of studies before anybody knew the results (including the principal investigators of the project), select which outcomes we were going to have people predict, and then wait some time to see what the experimental results were. We got this really nice set of projects. The projects looked at the general equilibrium effects of cash transfers, the effect of cash transfers compared to the effects of a mental health intervention (sort of similar to cognitive behavioral therapy), and an aspirations and goal-setting intervention, benchmarked against cash.

We went out and collected predictions from academics and from people similar to the intervention recipients on the causal effects of these interventions. And we used a set of outcomes that had already been pre-registered by the authors of the paper. To sum up my answer to your question, the main motivation for me on this paper is the following: I'd like to see how well people can predict which policies are going to work in a setting where there's generally a lot of uncertainty about what's going to be effective and where it's very costly to evaluate things. Forecasting won't replace running big randomized controlled trials, but maybe forecasting can help us choose what we run in those trials.

Interviewer: As discussed in one of your other papers, forecasting also has potential benefits for the economics field, such as the possibility to mitigate publication bias. Could you talk about some of those benefits in more detail?

Otis: This is still in the early days for work looking at forecasts of experimental results, so we'll have to see how valuable forecasting turns out to be in the first place. But here are the things that I think are promising. The first is the one that I just talked about. It's great if there's a lot of information in people's predictions; we can use that information to improve the selection of interventions.

Here's the second. There's a big problem right now that's been getting a lot of attention in economics and in the other social sciences, which is publication bias. A lot of studies are often evaluated and published based on whether the results are significant as opposed to, for example, how interesting the question is—regardless of the statistical significance of the finding. An experiment, for example, might have a null result, and reviews say, "Well, that's not super interesting," and the study ends up not being published.

How could forecasts come into play here? Let's suppose you have a bunch of experts who say, "I think this intervention is going to be very effective." But after, the results show that the intervention is not effective. All of the sudden, what might feel like no information in a null result could become exciting to people, as it's something that's actually providing a lot of information relative to the priors of academics in the field. So by collecting these predictions, we may be able to help correct publication bias to some extent by putting research results in context. The context of a null finding could be that it deviates substantially from academic priors.

Interviewer: What other questions are you interested in exploring and researching in the future, both in terms of forecasting or otherwise?

Otis: What am I interested in researching in the future? Okay, so here's part one: Even if forecasts can accurately predict what will work in some situations, they probably aren't going to work everywhere. Hopefully, we can provide some bounds around the types of circumstances where forecasts will be accurate. I'm sure they won't be accurate for predicting every type of result. I think some, perhaps many, results are truly going to be unexpected. So it'd be nice to have some bounds around the domains where we think forecasting will be useful.

I think the bigger picture question that I'm interested in is a bit more challenging to answer. In the best case scenario, where we know which circumstances forecasting works in, we would have a new mechanism [forecasts] to choose things from a choice set. Forecasts will help us pick the policies from the choice set, but it won't tell us what to put in the choice set. You can think of the question: How do you get people to show up to school? How do you reduce unemployment? How do you help people pick better health insurance? People can think of many different potential policies for each question but it isn't not feasible to test all of those ways. We need research on both how to efficiently choose things from the choice set, and also on how to populate the choice set. We don't know, in my opinion, that much at all about how to design the choice set that we select policies or interventions from.

So I'd like to examine how people create the choice set, how people come up with (or produce) the policy interventions. "Let's create the choice set, and then let's use forecasts to extract the good stuff from that choice set." That's the idea behind some of the recent work that I've been doing. We have people design text messages to motivate people to learn about coronavirus. Text messages are pretty insubstantial when it comes to thinking about policies. But it's nice because you can test hundreds or thousands of them at a relatively low cost and they can, in some circumstances, be fairly effective as a light-touch, non-financial incentive.

That's an area that I'm hoping to spend more time exploring, Are people responsive to incentives and coming up with interventions? If you pay people to come up with better policy interventions, do they, or is the production of interventions inelastic? Or perhaps creativity required to produce effective messages is crowded out by financial incentives. If it is, maybe we shouldn't be paying people to come up with better policies. We need to use some other lever to expand the choice set in a useful way.

Interviewer: Could you expand on that text messaging project you're currently working on?

Otis: There's some logistical challenges, so we'll see what happens with it first. But here's the idea behind this project. We're trying to motivate people in Kenya to do an SMS-based COVID task. A task would be something like taking a quiz to learn about misconceptions of COVID. And in terms of SMS messages, you could think of a thousand different possible text messages that you could send to people to motivate them to do the task. You could send them a message like, "You have the power to help your community, opt to do this thing." Or you could think of a message like, "Don't be the one who lets your community down, opt in to receive messages." There's an infinite number. It's a super multi-dimensional "policy space" because text messages are made of words.

So we are trying to answer the question, "If we pay people to come up with more effective messages, are they able to do so?" And the other side of this question which I think is really interesting is, "If you paid people to come up with more effective messages, do they do so by coming up with messages that will be more costly for recipients, or in other words, a sort of 'negative' message for recipients?" Maybe then we would need to adjust the contract to pay out both on the effectiveness of the message and how costly the message is. I think the space of contracts to encourage people to design policies is super interesting. There's some kind of related work in R&D and innovation literature, but not a lot of empirical work. So I'm really excited about that area.

Interviewer: That's really exciting to hear. If I was a researcher or student interested in learning more about or reading the work you've talked about, where should I go to do that?

Otis: My website is one place, which is www.nicholasotis.com, and my email is notis@berkeley.edu. I'll also give a quick plug for socialscienceprediction.org, which is a platform that Stefano Dellavigna, Eva Vivalt, a bunch of other people and I have been working on to develop. It's a sort of public good to streamline the collection of predictions for social science results, so if you're interested in collecting predictions, I encourage people to check out that website.

Interviewer: Sweet. Thank you so much for your time!

Do Repeated Weather Shocks Have Long-Run Effects? An Analysis of India’s Famine Era

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Abstract

This paper seeks to examine whether the effects of famines in the colonial era persist in the long run, and specifically whether or not repeated famines in a single area over time have additional cumulative effects. We use a panel of recorded famine severity and rainfall data in colonial Indian districts to construct cross-sectional measures of famine occurrence which we compare with available cross-sectional satellite luminosity, census data, and welfare survey data. Specifically, we regress modern-day outcomes on the number of famines suffered by a district in the colonial era, with and without various controls; we additionally instrument for famine occurrence with climate data in the form of negative rainfall shocks to ensure exogeneity. We then use categorical and non-parametric kernel regression to test for the presence of cumulative effects to repeated famine events. We find that districts which suffered more famines during the colonial era counterintuitively have higher levels of economic development, yet have more of the labor force working in agriculture, enjoy less consumption and rural areas, and are more unequal as measured by the Gini index in the present day, which we attempt to explain by showing that famine occurrence is simultaneously related to urbanization rates and agricultural development. We find limited support for the hypothesis that the effect of repeated famines is not simply linear in the number of famines. Overall, this suggests that the long-run effects of natural disasters which primarily afflict people and not infrastructure are not always straightforward to predict.

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Table of Contents

1 Introduction

What are the impacts of natural disasters in the long-run, and can repetitive short-term natural disasters have a long run impact on economic development? Are these effects different in the case of disasters which affect people but do not harm physical infrastructure? While there is plenty of theoretical and empirical literature on the impact of natural disasters and famines in particular, there are relatively few studies on the long-term consequences that may potentially arise from disasters specifically striking the same region repeatedly over a period of many years. The case of colonial India provides a well-recorded setting to examine these questions, with an unfortunate history of dozens of famines throughout the British Raj, many of which struck the same regions multiple times over the course of the late nineteenth and early twentieth centuries, to the extent that historian Mike Davis characterizes them as "Victorian Holocausts" (Davis 2001, 9). The United Kingdom formally ruled India from 1857 to 1947, following an earlier period of indirect rule by the East India Company. A principal characteristic of British governance of rural and agricultural India was the high tax rate imposed on peasants, and often collected from them through appointed intermediaries, such as the landowning zamindar caste in Bengal (Bose and Jalal 2004). These land taxes imposed on farmers could be extraordinarily high, ranging in case from ninety to ninety-five percent of the final produce[1], and often around two-third to fifty percent. Many of the intermediaries, who themselves had to send up to ninety-percent of the collected taxes to the British authorities, then coerced their tenants into farming only cash crops instead of a mix of cash crops and agricultural crops (Dutt 2001). The other principal characteristic of British agricultural policy in India was a laissez-faire attitude to drought relief. Most senior officials in the imperial administration believed that serious relief efforts would cause more harm than they would do good, and consequently were reluctant to dispatch aid to afflicted areas (ibid). The consequences of these two policies were, of course, some of the most severe and frequent famines in recorded history, such as the Great Indian Famine of 1893, in which an estimated 5.5 to 10.3 million peasants perished from starvation alone, and over sixty million were believed to have suffered hardship (Fieldhouse 1996).

In general, a single natural disaster may not leave lasting long-term effects if it is not particularly severe or does not impact physical infrastructure and industry. However, if a disaster strikes a region repeatedly over a period of many years, the development of the region may be stunted over this period compared with regions that do not suffer repeated disasters, which, if the difference in repetition is severe enough, may lead to measurable differences in economic development in the long run. For example, the districts of Bijapur and Pune in Maharashtra are recorded in our data as having suffered thirteen severe famines from 1870 to 1930, whereas Saharanpur district in Uttar Pradesh suffered none. Controlling for covariates, then, we might expect that growth in the former districts would be comparatively stunted with growth in the latter, which should be detectable through regression analysis. A further question of importance is whether or not the effects of these famines are increasing in the number of famines - that is, if the total damage caused to a district by famines is not purely a linear function of the number of famines suffered.

There are a couple of ways in which the impact of famines can be assessed over the long term. Our paper focuses on the comparison of survey data for households and satellite nighttime luminosity data as a proxy for GDP at the district level. Since the survey data is more limited, we transform and aggregate historical panel data on rainfall and famines so as to be able to use them in a cross section with the contemporary outcomes. We compare estimates from ordinary least squares and from instrumenting for recorded famine severity with rainfall. Finally, we assess the hypothesis that the marginal effect of famine is increasing in the number of famines, first by treating a count variable of famines as a series of categorical indicators, then through non-parametric and semi-parametric spline regression on the number of famines.

We find for many outcomes that there is indeed a marginal effect of famines in the long-run, although where it is significant, it is often quite small. Where famines do have a significant impact on contemporary outcomes, they follow an interesting pattern; a higher rate of famine occurrence in a given district is associated with greater economic development, yet worse rural outcomes and higher inequality. Specifically, famine occurrence has a small but positive impact on nighttime luminosity, our proxy for economic development, and smaller, negative impacts on rural consumption and the proportion of adults with a college education. At the same time, famine occurrence is also associated with a higher proportion of the labor force being employed in the agricultural sector as well as a higher Gini index. At the same time, we find limited evidence that famine occurrence has a slightly negative impact on infrastructure, for example as more famines are associated with less access to medical care and less access to bus service. We do not find that famines have any significant impact on social mobility (specifically, intergenerational income mobility) or infrastructure (specifically, electrification) in districts. This contrasts with much of the established literature on natural disasters, which has mostly found large effects with often drastic and negative historical consequences. We attempt to explain this by analyzing the impact of famines on urbanization rates to show that famine occurrence may lead to a worsening urban rural gap in long-run economic development. Thus, we make an important contribution to the existing literature in finding that short-term natural disasters which do not destroy physical infrastructure may have counterintuitively positive outcomes in the long-run, which challenges existing research. However, we find limited evidence to suggest that the impact of famines is increasing or decreasing in the total number of famines suffered by a district, as our methods fail to credibly exclude a constant marginal effect; the estimated marginal effects from treating famine counts as categories do not greatly change and the non-parametric regressions mostly show outcomes as linear functions of famine.

While the instrumental estimates are guaranteed to be free of omitted variable bias, the OLS standard errors allow us to make judgements more precisely due to having smaller confidence intervals. In around half of our specifications, the Hausman test for endogeneity fails to reject the null hypothesis of exogeneity, indicating that the ordinary-least squares results are just as valid as our instrumental-variables results; however, the instrumental variables estimate help us to address other problems such as attenuation bias due to possible measurement error. Therefore, we ultimately opt to focus on the instrumental variables results.

Section 2 presents a review of the literature and builds a theoretical framework for understanding the impacts of famines on modern-day outcomes. Section 3 describes our data and the construction of our variables, and presents summary statistics. Sections 4 and 5 present our results using ordinary least-squares and instrumental two-stage least-squares approaches. Section 6 presents the non-parametric and semi-parametric analyses. Section 7 discusses our results.

2 Review and Theoretical Framework

A. The Impact of Natural Disasters

Most of the current literature on natural disasters as a whole, such as Nguyen et al (2020) and Sharma and Kolthoff (2020) focuses on short-run aspects of natural disasters relating to various aspects of proximate cause (Huff 2020) or short-term recovery. While much of the literature concentrating on famines specifically does focus on long-term effects, the results mostly focus on long-term effects on biological variables such as height, nutrition (Cheng and Hui Shui 2019), or disease (Hu et al. 2017), instead of macro-level socioeconomic outcomes. Many of the studies that do involve socioeconomic outcomes only focus on long-term effects at the level of the individual, such as Thompson et al. (2019). Consequently, our paper is the first to study the effects of famine on the overall development of an entire region, of which we describe our proxy measurements (the variables we use to characterize development) in section three.

On the other hand, while there is some literature that focuses on long-term socioeconomic outcomes, such as Ambrus et al. (2015) and Cole et al. (2019), the papers mostly deal with long term consequences of a single, especially severe natural disaster, or one that is unfortunately timed and is therefore related to path dependency effects, such as in Dell (2013). Our paper specifically focuses on the possibility of additional long-term effects purely from the repetition of such disasters, of which India's famine era provides an excellent (if unfortunate) historical case study, in section six

All of the established literature purports that natural disasters overwhelmingly influence economic growth through one of two channels, possibly both: either through destruction of infrastructure and loss of human capital (Lima and Barbosa 2019, Nguyen et al. 2020, Cole et al. 2019), or through historical consequences, possibly unintended or accidental, such as armed conflict (Dell 2013, Huff 2019). Famines pose an interesting question in this regard, since they tend to result in severe loss of human capital through population loss due to starvation, but generally result in smaller-scale infrastructure losses (Agbor and Price 2013). This is especially the case for rural India, which suffered acute famines while having little infrastructure in place (Roy 2006). We therefore examine several areas of potential consequences of famines, including infrastructure, public goods provision, and social mobility, as outlined in section three. Our results present a novel finding: while famine occurrence seems to negatively impact most outcomes, as expected, it positively impacts a few unexpected outcomes, such as while negatively impacting most others, which we attempt to explain by considering the impact of famines on urbanization rates.

Prior econometric literature on India's famine era has highlighted other areas of focus, such as Burgess and

Donaldson (2012), which shows that trade openness helped mitigate the catastrophic effects of famine. There is also plenty of historical literature on the causes and consequences of the famines, most notable in academic analyses from British historians (contemporarily, Carlyle 1900 and Ewing 1919; more recently Fieldhouse), which tend to focus on administrative measures, such as the efficacy of various famine prevention and relief programs used in Indian provinces.

Therefore, due to the heterogeneity of mechanisms by which famines can impact, in our following model we leave the exact causal mechanism unspecified and instead treat famines as generic shocks which are recovered from with an unknown speed and have small long-term effects on outcomes, but which can have larger cumulative effects if they strike repeatedly. This allows us to attempt to examine if famines have long-term effects, but without forcing the effect to take on a specified pattern relating to the number of famines. In the most general terms, our estimation in sections four, five, and six will help us to more precisely quantify some aspects of the model.

B. Potential Effects of Repeated Natural Disasters

Our intuition for the basis of distinguishing a cumulative long-run effect of repeated famines rests on a simple growth model in which growth returns to the long-run average after a shock, but state variables (i.e, GDP, consumption, etc.) only return to asymptotically is as follows, so that over the horizons we consider, there may still be measurable effects of famines. As mentioned below, this is in line with more recent macroeconomic models of natural disasters such Hochrainer (2009) and Bakkensen and Barrage (2018), which incorporate adverse weather phenomena by quantifying their effects on structural determinants of growth, such as capital depreciation.

Assume colonial districts (indexed by i) suffer ni famines over the time period (in our data, the years 1870 to 1930), approximated as average constant rates f_i[1]. The occurrence of famine can then be modeled by a Poisson process with interval parameter fi, representing the expected time between famines, even though the exact time is random and thus unknown until it is realized. For simplicity, we assume that famines cause damage d to a district's economy, for which time ri is needed to recover to its assumed long-run, balanced growth path. Importantly, this damage is unspecified; it could take effect by depreciating physical capital, decreasing human capital (although given the nature of famine, the latter is more likely), reducing productivity, etc., or any combination of these, and even be positive if it increases these factors. We make no assumptions on the distributions of d and ri except that ri is dependent on d, and that the average recovery time E[ri] is similarly a function of E[d].

The important question is the nature of the relationship between d and ri. While f can be easily inferred from our data, d and especially r are much more difficult to estimate without detailed, high-level, and accurate data. However, we can distinguish two cases.

First, if the recovery time is equal to or more rapid than the occurrence of famine (fi > ri), then we can assume that the district generally returns to its long-run growth path before the next famine strikes. Any long-term effects of famines will then be the result of the total time that the district spends returning to the balanced growth path, which in this case will simply be a linear factor of the number of famines that strike the district. Therefore, the effects of famine should be linear in the number of famines, which forms the basis for our investigation in sections four and five

1 While it would of course be more theoretically accurate to maintain variation in the rates at which famines strike and consider for each district the historical record of famine timing, the difference would be obscured by the transformation of our panel into a cross-section, which is unfortunate but necessary due to the limitations of our data in the present-day. Nevertheless, we still find some significant effects on famines despite this handicap in our analysis.

However, if the interval of famine occurrence is strictly lower than the recovery time (fi < ri), then the next famine tends to strike before the district has had time to recover from the previous one, meaning that there is a nonzero amount of damage g which represents the difference between the damage caused by the latest famine and the partial recovery from the prior famine. We further distinguish two sub-cases:

1. The possible total damage from famine is "unlimited" (i.e. very high), so that by the end of the time period, districts can have as much as ng total damage from famines before they can start recovering to their balanced growth paths without hindrance. This means that they will spend more time in recovery than in the first case, so that the impact of famines is increasing in the number of famines, i.e. each additional famine is partially more severe than the prior simply because it is cumulative, i.e. that aside from severity, the $(n+1)^{th}$ famine is automatically worse than the n^{th} famine for this reason. Thus, the impact of
2. The possible total damage is lower, so that famines cannot cause damage to a district beyond some unknown threshold (as unlikely as that may be). Such a scenario would be analogous to the effect of a severe flood, which while devastating, cannot directly impact areas outside of a floodplain. While this is irrelevant if the recovery time is shorter than famine occurrence rate – as in the first case – in the second case, this would mean that later famines simply cannot do as much damage since the total damage is capped. In this case, the impact of famines will be decreasing in the number of famines.

For these cases, we can then model the impact of famines as an unknown function h of the number of famines. To estimate the form of h we could fit some sort of increasing or decreasing model, such as a polynomial or exponential model, but it is much simpler and less assumptive to use non parametric (without controls) and semi-parametric (with controls) regression techniques to allow for the effect of famine to vary, rather than estimating several different types of parametric models which may put inaccurate assumptions on the form of h.

Therefore, we have a simple structure that may explain the trends in the impact of famines (that is, if the impacts themselves exist, which is the focus of sections four and five), which allows for a wide variety of empirical possibilities that we could not distinguish with existing data. For example, if we were to find evidence for the impact of famines being increasing in the number of famines (i.e. if we were to find that h is an increasing function), then it may be the case that districts have very low recovery rates, so that it takes a long to time to recover from even modest famines, and that famines occur frequently with small damage, but with enough damage to matter. However, it could also be the case that districts recover quickly from damage, but famines strike infrequently with severe enough damage that they are unable to recover in time. Either way, the overall fact that famines strike before districts can sufficiently recover remains the same, but the precise mechanics and impacts of famine occurrence are not assumed to be known.

Overall, our assumptions imply that the total time spent in recovery, that is, returning to the balanced growth path, is ∑itf = niE[tf], and that long-run growth is therefore a function of this quantity. Classical growth theory, such as in the Solow-Swan (1957) and Romer (1994) implies long-run convergence and therefore districts would have similar outcomes today regardless of the number of famines they underwent, an issue more salient with the consideration of other historical effects that may have occurred between the colonial era and today. However, this is at odds with most of the empirical literature as discussed previously, in which there are often measurable long term effects on natural disasters. In addition, newer models have developed mechanisms by which disasters can affect long-term growth by altering structural parameters, such as Bakkensen and Barrage (2018), in which cyclones increase the rate at which capital depreciates and lower total factor productivity. Therefore, in our case we simply assume that convergence is either very slow or that the damage caused by famines induces slight long-run changes in their growth patterns, a possibility consistent with models of path-dependency, such as in Dell (2013) and Brekke (2015). Therefore, if the district had continued on the growth path directly without the famine, absent any confounding effects, it would counterfactually have more positive outcomes today by a factor dependent on niE[tf] and thus ni.

Instead of actual counterfactuals, of course, we use the other districts in the sample. Controlling for factors such as population and existing infrastructure, each district should provide a reasonably plausible counterfactual for the other districts in the number of famines it is afflicted by. Then, the differences in outcomes among districts measured today, yi, can be modeled as a function of the differences in the number of famines. Finally, across the entire set of districts, this can be used to represent the average outcome E[yi] as a function of the number of famines, which of course forms the basis of all of our regression approaches in sections four, five and six, parametric and non parametric.

C. Estimation

Therefore, in section four we would prefer to estimate (1) below, where β represents our estimation of the effect of famine severity, measured as the number of famines undergone by the district, on the state variable outcome y, and X is a vector of contemporary (present-day) covariates.

$$y_i = \alpha + \beta * famine_i + X_i^T \gamma + \varepsilon_i \qquad (1)$$

However, it is important to note that much of the research on famine occurrence in colonial India emphasizes the fact that many of the famines as well as their consequences were exacerbated, if not downright caused, by poor policies and administration by the British Raj. If this is the case, and these same policies hurt the development of districts in other ways, such as by stunting industrialization directly, then estimation of (1) will not show the correct effect of famines per se on comparative economic development. Additionally, our observations of famines, which are taken indirectly from district-level colonial gazetteers and reports, may themselves be correlated in unobservable ways with factors influencing modern-day outcomes in the districts. To solve this problem, we turn to the examples of Dell et al. (2012), Dell (2013), Hoyle (2010), and Donaldson and Burgess (2012), who use weather shocks as instruments for natural disaster severity. Thus, another contribution of our paper is to further the use of climate shocks as instruments. While Dell (2013) for example focuses on historical consequences arising from path dependency and Hoyle (2010) focuses on productivity, the instrumental methodology itself is of course perfectly applicable to our work. This is exceptionally desirable since weather shocks are extremely short term phenomena, so their occurrence is unlikely to be correlated with longer-term climate factors that may impact both historical and modern outcomes, and because they are reasonably random, so as to provide exogenous variation with which we can estimate the impact of famines in an unbiased manner.

Therefore, we first estimate equation (2) below, before estimating (1) using the predicted occurrence of famine from (2):

$$\widehat{famine}_i = \lambda + \theta * rainfall_i + X_i^T \eta + \nu_i$$

(2)

Here, we calculate famine as the number of reported events occurring in our panel for a district, and rainfall as the number of years in which the deviation of rainfall from the mean falls below a certain threshold, nominally the fifteenth and tenth percentiles of all rainfall deviations for that district. As is standard practice, we include the control variables in the first-stage even though they are quite plausibly unrelated to the rainfall variable. This allows us to estimate the impacts of famine with a reasonably causal interpretation; since the assignment of climate shocks is ostensibly random, using them to "proxy" for famines in this manner is akin to "as good as random" estimation. The only issue with this first-stage specification is that while we instrument counts of famine with counts of low rainfall, the specific years in which low rainfall occurs theoretically need not match up with years in which famine is recorded in a given district. Therefore, we would prefer to estimate (3) below instead, since it provides additional identification through a panel dataset. Any other climate factors should be demeaned out by the time effects. Other district characteristics that may influence agricultural productivity and therefore famine severity, such as soil quality, should be differenced out with district effects. Finally, differences in administrative policy should be resolved with province fixed effects.

$$\widehat{famine}_{it} = \lambda_i + \delta_t + \theta * rainfall_{it} + X_i^T \eta + \nu_{it}$$

(3)

Unfortunately, we would then be unable to implement the standard instrumental variables practice of including the control variables in both stages, even though all of our controls are modern-day and therefore plausibly unrelated to the variables of interest, since our modern-day observations are cross-sectional. Nevertheless, our specification in (2) should reasonably provide randomness that is unrelated to long-term climate factors, as mentioned above. Finally, in order to compare famine severity with our cross-sectional modern-day outcomes, we collapse the panel by counting the number of famines that occur in the district over time, to get an exogenous count measure of famine that we can use de novo in (1). To account for sampling variance in our modern-day estimates, we use error weights constructed from the current population of each district (so that our approach in section 5 is technically weighted least-squares, not ordinary). While this should account for heteroscedasticity in the modern observations, to assure that our standard errors on the historical famine and rainfall variables are correct, we use robust SM estimators in our estimations (McKean 2004, Barrera and Yohai 2006).

For the non-parametric estimation, we first take advantage of the treatment of famine as a count variable and estimate the form of h as discussed above using similar regression specifications as in (1), but where β is a function of the number of famines. This looks like equation (4) below:

$$y_i = \alpha + \sum_{j=1}^{20} \beta_j famine_{ij} + X_i^T \eta + \varepsilon_i \quad (4)$$

Therefore, the effect of famine is allowed to vary with the number of famines, but in a discrete manner. For a continuous approach, we then estimate (5):

$$y_i = \alpha + h(famine_i) + X_i^T \eta + \varepsilon_i \quad (5)$$

Note that this is technically a semi-parametric specification, as for computational simplicity we still assume linearity in the control variables. For the specification we test that does not include controls, (5) would indeed be fully non-parametric. To estimate this practically, we run penalized spline regressions, which in essence estimate by restricted maximum likelihood a series of polynomial functions (with a penalty function to keep their degree manageable), with a smoothing parameter that depends on the degrees of freedom in the model, following the recommendations of Ruppert, Wand and Carroll (2003). Roughly speaking, this allows us to interpolate the estimates in (4) in order to produce a smooth estimate of h that makes no assumptions on its form.

The results of these approaches are detailed in section six.

3 Data

A. Sources and Description

Our principal dataset of interest is a historical panel detailing at the district level famine severity over time in British India from 1870 to 1930 compiled from a series of colonial district gazetteers by Srivastava (1968). This was coded into a panel by Donaldson and Burgess (2010) by using the following methodology:

4 – District mentioned in Srivastava's records as "intensely affected by famine"
3 – District mentioned as "severely affected"

2 – Mentioned as "affected"

1 – Mentioned as "lightly affected"

0 – Not mentioned

9 – Specifically mentioned as affected by spillovers from a neighboring district[2]

In our own coding of the data, we treat famines as codes 2, 3, and 4, and severe famines as codes 3 and 4. We compute further cross-sectional measures, chiefly the total number

2 There are only four such district-years in the entire dataset, so we simply exclude them in our analysis.

and proportion of famine-years that a district experienced over the sixty-year periods, which is equivalent to tabulating the frequency of code occurrences and adding the resulting totals for codes 2 to 4 to obtain a single count measure of famine. Our results are robust to using “severe” (codes 3 and 4) famines instead of codes 2, 3, and 4. Across the entire panel, codes from 0 to 4 occurred with the following frequencies: 4256, 35, 207, 542, and 45.

We also supplemented this panel with panel data on rainfall over the same time period. Several thousand measuring stations across India collected daily rainfall data over the time period, which Donaldson (2012) analyzes and compares with crop data. The rainfall data in Donaldson (2012) represent the total rainfall in a given district over a year, categorized by growing seasons of various crops (for example, the amount of total rainfall in a district that fell during the wheat growing season). Since different districts likely had different shares of crops, we average over all the crops to obtain an approximate of total rainfall over the entire year. We additionally convert this into a more relevant measure in the context of famine by considering only the rainfall that fell over the growing seasons of crops typically grown for consumption in the dataset; those being bajra, barley, gram (bengal), jowar (sorghum), maize, ragi (millet), rice, and wheat. Finally, to ensure additional precision over the growing season, we simply add rainfall totals over the growing seasons of the two most important food crops, rice and wheat, which make up over eighty percent of grown food crops in the country (World Bank, UN-FAOSTAT). Importantly, the two crops have nearly opposite growing seasons, so that the distribution of rainfall over the combined growing season well-approximates that of total rainfall over the entire year. Our results are robust with regards to all three definitions; for example, the pairwise correlations between the measures are never less than ninety percent. The cross-sectional famine instruments constructed from these, in fact, are almost totally identical.

As expected, there indeed appears to be significant variation in annual rainfall, as shown by the example of Buldana district (historically located in the Bombay presidency, now in Maharashtra state), as shown in Figure 1 on the following page. In general, the trends for both measures of rainfall over time are virtually indistinguishable aside from magnitude. As expected, famine years are marked by severe and/or sustained periods of below-average rainfall, albeit the existence of a few districts which have years with low rainfall and no famines. Most of the latter can be explained by a lack of famine records, especially in earlier years. There are also a few districts being recorded as having had famines despite above-average rainfall, which could possibly be the result of non-climatic factors such as colonial taxation policies, conflicts, or other natural disasters (for example, a locust plague). However, the relationship between rainfall patterns and famine occurrence suggests that it is more than enough to use the former as an instrument for the latter, especially in light of the fact that the correlation is not perfect and famine occurrence is plausibly non-random due to the impact, for example, of British land ownership policies.

Figure 1: Rainfall over time for Buldana district from 1870 to 1920

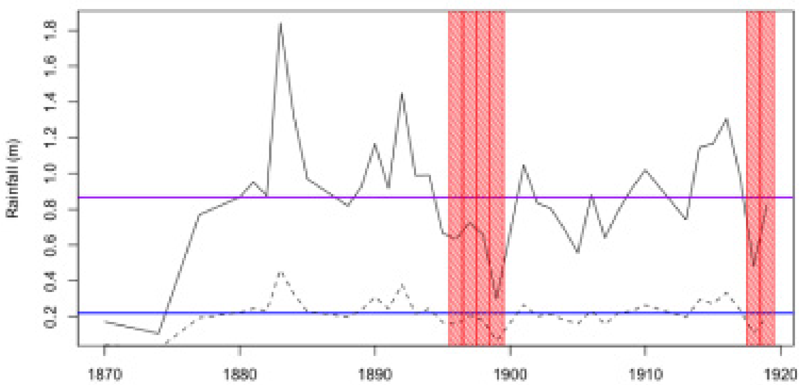


Fig. 1: The dashed line shows mean rainfall for all food crops; the solid line

shows the total rainfall over the wheat and rice growing seasons. The blue and purple lines represent the historical means for these measures of rainfall. The rad shading denotes years in which famines are recoded as having affected the district.

We construct count instruments for famines by computing for each district, the historic mean rainfall and the annual deviation therefrom, and then counting famines as years in which the deviation was in the bottom fifteenth percentile, in order to capture relatively severe and negative rainfall shocks as plausible famine causes. For severe famines, we use the bottom decile instead. The percentiles were chosen based on severity so that the counts obtained were similar enough to the actual counts constructed from recorded famines in the panel dataset above.

For modern-day outcomes, we turn to survey data from the Indian census as well as the Indian Human Development Survey II, which details personal variables such as consumption, education, infrastructure (measures such as access to roads), and access to public goods such as hospitals at a very high level of geographical detail. An important metric constructed from the household development surveys is that of intergenerational mobility as measured by the expected income percentile of children whose parents belonged to a given income percentile, which we obtain from Novosad et al. (2019). Additionally, as survey data can often be unreliable, we supplement these with an analysis of satellite luminosity data that provides measures of the (nighttime) luminosity of geographic cells which we argue should serve as a more reliable proxy for economic development, following Henderson et al. (2011) and Pinkovsky and Sala-i-Martin (2016). These data are mostly obtained from Novosad et. al (2018, 2019) and Iyer (2010), which we have aggregated to the district level.

The outcomes variables are as follows:

1. Log absolute magnitude per capita. We intend this to serve as a proxy for a district's economic development

in lieu of reliable GDP data. This is the logarithm of the total luminosity observed in the district divided by the district's population. These are taken from Vernon and Storeygard (2011) by way of Novosad et al. (2018).

2. Log rural consumption per capita. This is taken from the Indian Household Survey II by way of Novosad et al. (2019).
3. Share of the workforce employed in the cultivation sector, intended as a measure of rural development and reliance on agriculture (especially subsistence agriculture). This is taken from Iyer et al. (2010).
4. Gini Index, from Iyer (2010), as a measure of inequality.

5. Intergenerational income mobility (father-son pairs), taken from Novosad et al. (2018). Specifically, we consider the expected income percentile of sons in 2012 whose fathers were located in the 25th percentile for household income (year?), using the upper bound for robustness[3].
6. The percentage of the population with a college degree, taken from census data.
7. Electrification, i.e. the percent of villages with all homes connected to the power grid (even if power is not available twenty-four hours per day).
8. Percent of villages with access to a medical center, taken from Iyer (2010), as a measure of rural development in the aspect of public goods.
9. Percent of villages with any bus service, further intended as a measurement of public goods provision and infrastructure development.

Broadly speaking, these can be classes within three categories, with 1-3 representing broad measures of economic

3 So, for example, if this value were 25, then there is (on average) no mobility, as sons would be expected to remain in the same income percentile as their fathers. Similarly, if it were less than (greater than) 25, then there would be downward (upward) mobility. A value of 50 would indicate perfect mobility, i.e no relationship between fathers' income percentiles and those of their sons.

development, 4-6 representing inequality and human capital, and 7-9 representing the development of infrastructure and the provision of public goods. As discussed in section two, our prior is that the occurrence of famines has a negative effect on district development, which is consistent with most of the literature on disasters. Hence, given a higher occurrence of famine, we expect that districts suffering from more famines during the colonial period will be characterized by lower levels of development, being (1) less luminous at night, (2) poorer in terms of a lower rural consumption, and (3) more agricultural, i.e have a higher share of the labor force working in agriculture. Similarly, with regards to inequality and human capital, we expect that more famine-afflicted districts will have (4) higher inequality in terms of a higher Gini coefficient, (5) lower upward social mobility in terms of a lower expected income percentile for sons whose fathers were at the 25th income percentile, and (6) a lower percentage of adults with a college education. Finally, by the same logic, these districts should be relatively underdeveloped in terms of infrastructure, and thus (7) lack access to power, (8) lack access to medical care, and (9), lack access to transportation services.

Finally, even though our independent variable should be exogenous when instrumen, we attempt to control for geographic and climatic factors affecting agriculture and rainfall in each district, namely:

1. Soil type and quality (sandy, rocky or barren, etc.)
2. Latitude (degree) and mean temperature (degrees Celsius).
3. Coastal location (coded as a dummy variable).
4. Area in square kilometers (it should be noted that district boundaries correspond well, but not perfectly, to their colonial-era counterparts).

As mentioned previously, research by Iyer and Banerjee (2008, 2014) suggests that the type of land-tenure system implemented during British rule has had a huge impact on development in the districts. We also argue that it may be related to famine occurrence directly (for example, that tenure systems favoring landlords may experience worse famines), in light of the emerging literature on agricultural land rights, development, and food security (Holden and Ghebru 2016, Maxwell and Wiebe 1998). Specifically, we consider specifications with and without the proportion of villages in the district favoring a landlord or non-landlord tenure system[4], obtained from Iyer (2010). In fact, the correlation between the two variables in our dataset is slightly above 0.23, which is not extremely high, but high enough to be of concern in terms of avoiding omitted variable bias. Therefore, we ultimately consider four specifications for each dependent variable, based on the controls in X from equation (1): no controls, land tenure, geography, and land tenure with geography.

We avoid using contemporary controls for the outcome variables (that is, including infrastructure variables, income per capita, or welfare variables in the right-hand side) because many of these could reasonably be the result of the historical effects (the impact of famines) we seek to study, so including them as controls would artificially dilate the impact of our independent variable.

B. Summary statistics

Table I presents summary statistics of our cross-sectional dataset on the following page. One cause for potential concern is that out of the over 400 districts in colonial India, we have only managed to capture 179 in our sample. This is due chiefly to a paucity of data regarding rainfall; there are only 191 districts captured in the original rainfall data from Donaldson (2012). In addition, the changing of district names and boundaries over time makes the matching of old colonial districts with modern-day administrative subdivisions more imprecise than we would like. Nevertheless, these districts cover a reasonable portion of modern India as well as most of the regions which underwent famines during imperial rule. However, the small number of districts may also pose a problem in

4 For a brief overview of the types of systems employed by the East India Company and Crown administrators, see Iyer and Banerjee (2008), or see Tirthankar (2006) for a more detailed discussion.

terms of the standard errors on our coefficients, as the magnitude of the impacts of famines that occurred over a hundred years ago on outcomes today is likely to be quite small.

As noted above, the rainfall statistics presented in panel A reveal a very large variation in the annual amount of rainfall received by districts, with the median district's annual rainfall fluctuating by around one-third of a meter. There is consequently a large amount of variation in the number of famine years experienced by districts as well, with a standard deviation of over four famine years. There is less variation in the famine approximation using negative rainfall shocks, but still enough to be useful as an independent variable and instrumental variable. Finally, we wish to note that many of the geographic control variables, chiefly the dummies and proportions corresponding to coasts and terrain, are heavily right-biased, but this should not be a problem as we use robust regression techniques.

Table 1 – Summary Statistics (next page)

Variable	N	Min	Q1	Median	Q3	Max	Mean	S.D
Panel A. Independent variables								
Mean rainfall across time	192	0.181	0.856	1.168	1.592	8.648	1.584	1.324
S.d of rainfall across time	192	0.102	0.287	0.340	0.454	5.720	0.522	0.743
Number of low-rainfall years	179	3	6	6	7	9	6.223	0.963
Number of famines	179	0	5	7	10	21	7.877	4.318
Panel B. Dependent variables								
Log absolute magnitude pe capita	179	-6.13	-4.92	-4.13	-3.69	-1.44	-4.31	0.79
Log consumption per capita	179	8.27	9.32	9.43	9.54	10.07	9.42	0.28
Share of workforce in cultivation	179	0.000	0.145	0.214	0.280	0.613	0.228	0.118
Gini index	179	0.249	0.605	0.695	0.743	0.817	0.661	0.108
Intergenerati al income mobility	179	17.239	29.825	37.008	44.980	59.448	37.365	9.786
Percent adult with college degree	179	0.000	0.001	0.002	0.005	0.057	0.005	0.009
Village electrificatior	179	0.003	0.080	0.194	0.480	1.000	0.317	0.306
Access to medical servi	179	0	0.2	0.3	0.6	1	0.424	0.320
Access to bus service	197	0.035	0.185	0.291	0.626	1.00	0.401	0.270
Panel C. Control variables								
Tenure (proportion non-landlord	179	0.000	0.000	0.338	1.000	1.000	0.457	0.442
EEs								
Area (square km.)	179	1,118	3,500	5,743	10,112	39,114	7,151	5,221.66
Coastal dumr	167	0.000	0.000	0.000	0.000	1.000	0.126	0.333
Percent rocky terrain surfac area	179	0.000	0.000	0.000	0.005	0.266	0.006	0.022
Percent sandy surface area	179	0.000	0.000	0.000	0.001	0.088	0.002	0.008
Percent steep-sloping surface area	179	0.000	0.000	0.000	0.001	0.050	0.002	0.007
Primary soil type red dum	179	0.000	0.000	0.000	0.001	0.050	0.002	0.007
Primary soil type black dummy	179	0.000	0.000	0.000	0.001	0.050	0.002	0.007
Latitude	167	9.610	20.355	24.460	26.170	32.000	23.044	4.909
Mean annual temperature	167	19.799	23.970	25.489	26.351	27.478	25.090	1.613
Panel D. Miscellaneous								
Population (2011)	179	84,121	1,632,03	2,620,01	3,735,50	11,059,5 8	2,853,92 000	1,609,21 000

Source: Author calculations, from Iyer, Bannerjee, Novosad et. al, Asher and Novosad[5], Donaldson and Burgess

4 Ordinary Least Squares

Although we suspect that estimates of famine occurrence and severity based on recorded historical observations may be nonrandom for several reasons (mentioned in section two and three), we first consider direct estimation of (1) from section two. For convenience, equation (1) is reprinted below:

$$y_i = \alpha + \beta * famine_i + X_i^T \gamma + \varepsilon_i \qquad (1)$$

As in the previous section, famine refers either to the number of years that are coded 2, 3, or 4 in famine severity as described in Srivastava (1968), or the proportion of such years thereof (see section three for a description of the codes), and X is the set of contemporary covariates, also described in section three. We estimate four separate specifications of (1) where X varies:

1. No controls, i.e. X is empty.
2. Historical land tenure, to capture any effects related to British land policy in causing both famines and long-term developmental outcomes.
3. Geographical controls relating to climatic and terrestrial factors, such as temperature, latitude, soil quality, etc.
4. Both (2) and (3).

Table II presents the estimates for the coefficients on famines and tenure for our nine dependent variables on the following page, along with ninety-percent confidence intervals (we omit coefficients and confidence intervals for the geographic

5 Asher, Sam and Novosad, Paul. 2019. "Rural Roads and Local Economic Development". American Economic Review (forthcoming). Web.

variables for reasons of brevity, and because they are not so important for interpreting our results). In general, the inclusion or exclusion of controls does not greatly change the magnitudes of the estimates nor their significance, except for a few cases. We discuss effects for each dependent variable below:

Log of total absolute magnitude in the district per capita: the values for famine suggest that surprisingly, each additional famine results in anywhere from 1.8 to 3.6 percent more total nighttime luminosity per person in the district. As mentioned in section three, newer literature shows that nighttime luminosity is a far more reliable gauge of development than reported measures such as GDP, so we are forced to conclude that it seems that having suffered more famines is positively related to development. This, in fact, is confirmed by the instrumental variables estimates in Table III (see section five). Inexplicably, the inclusion of tenure and geography controls separately does not change the significance, but including both of them together in the covariates generates far larger confidence intervals than expected and reduces the magnitude of the effect by an entire order of magnitude. As expected, however, land tenure plays a significant role in predicting a district's development, with even a single percent increase in the share of villages with a tenant-favorable system being associated with a whopping 73-80% additional nighttime luminosity per person, roughly speaking.

Log rural consumption per capita: we find limited evidence that additional famines are associated with less rural consumption on a miniscule scale, suggesting that the effect of famines on development mentioned just above may not be equal across urban-rural divides but rather, for example, more apparent in concentrated cities, suggesting a possible causal mechanism that implies faster urbanization in districts that undergo more famines. Unlike with luminosity, historical land tenure does not seem to play a role in rural consumption. Percent of the workforce employed in cultivation: as expected, additional famines seem to play a strongly significant but small role with regards to the labor patterns in the district. Districts with more famines seem to have nearly one percent of the labor force working in cultivation for each additional famine, which suggests that somehow, famines may keep districts from developing other industries that are not agriculture (specifically, cultivation) related. Our instrumental variables estimates confirm this. Puzzlingly, land tenure does not seem to be related to this very much at all.

Gini Index: the coefficients for the number of famines seem to be difficult to interpret, as both those for the specification with no controls and with both sets of controls are statistically significant with similar magnitudes, yet opposite signs. However, the confidence interval for the latter is slightly narrower. Unfortunately, the only thing that seems clear is the need for more data – namely, for more of the districts in colonial India to be matched in our original sample. However, we can at least say that it is clear that land tenure has a large and significant positive association with inequality, which unfortunately cannot be 100% confirmed as causal due to the lack of an instrument for land tenure which covers enough districts of British India (however, as Iyer and Banerjee (2014) argue, the assignment of tenure systems itself was plausibly random and done at the whims of British administrators, so one could potentially interpret the results as causal with some level of caution).

Intergenerational income mobility: similarly, we do not find evidence of an association between the number of famines suffered by a district in the colonial era and social mobility in the present day, but we do find a strong impact of land tenure, which makes sense to the reported institutional benefits of tenant-favorable systems in encouraging development as well as the obvious benefits for the tenants and their descendants themselves. Each one-percent increase in the share of villages in a district that use a tenant-favorable system in the colonial era is associated with anywhere from ten to thirteen percent higher expected income percentile for sons whose fathers were at the 25th percentile in 1989, although the estimates presented in Table II are an upper bound.

College education: we find extremely limited evidence that famines in the colonial period are associated with less human capital in the present day, with a near-zero effect of additional famines on the share of adults in a district with a college degree (in fact, rounded to zero with five to six decimal places). Land tenure similarly has very little or no effect.

Electrification, access to medical care, bus service: all three of these infrastructure and public goods variables show a negligible effect from famines, but strong impacts of historical land tenure.

Ultimately, what we seem to find is that famines themselves have some impact on long-term development, despite also being associated with a greater share of the workforce employed in agriculture as opposed to activities more directly related to developed economics (such as the service sector, etc.), whereas land tenure has strong impacts on nearly all of the development outcomes. This suggests that the relationship between land-tenure and famine is worth looking into.

However, a potential problem with these estimates is the presence of bias in the recording of famines, as well as the potential presence of factors that both cause famines while simultaneously affecting long-run outcomes. We have already attempted to account for one of those, namely historical land tenure systems. Indeed, in most of the specifications, including tenure in the regression induces a decrease in the magnitude of the coefficient on famine. However, as the effect of famine tends to be extremely small to begin with, the relationship is not always clear.

Other errors are, of course, not impossible. For example, if a given district experienced a severe famine in a given year, but insufficient records remained by 1968 with which Srivastava constructed the coding, leading to the district receiving a code of 0 for that year. Indeed, as described in section three, a code of 0 corresponds to a code of "not mentioned", which encompasses both "not mentioned at all" and "not mentioned as being affected by famine" (Donaldson and Burgess 2010). While measurement error in the dependent variable is usually not a problem, error in the independent variable can lead to attenuation bias in the coefficients since the ordinary least-squares algorithm minimizes the error on the dependent variable by estimating coefficients for the independent variables; the greater this error, the more the ordinary least-squares method will bias the estimated coefficients towards zero in an attempt to minimize error in the dependent variable (Riggs et al. 1978). For these reasons, we turn to instrumental variables estimation in section five in an attempt to provide additional clarity.

Table 2 – Ordinary Least-Squares Estimates

Table 2 – Ordinary Least-Squares Estimates

Control specification:	(a)	(b)	(c)	(d)
Dependent variable:				
Log absolute magni per capita				
Estimate	0.038***	0.018*	0.036***	0.007
tandard Error	(0.011)	(0.010)	(0.011)	(0.011)
Log consumption capita				
Estimate	-0.007	-0.009*	-0.001	-0.001
Standard error	(0.005)	(0.005)	(0.004)	(0.004)
Share of workforc cultivation				
Estimate	0.009***	0.009***	0.006***	0.005**
Standard error	(0.002)	(0.002)	(0.002)	(0.002)
Gini index				
Estimate	0.003*	0.002	-0.001	-0.004**
Standard error	(0.002)	(0.002)	(0.002)	(0.002)
Intergenerational income mobility				
Estimate	0.050	-0.289*	0.207	-0.204
Standard error	(0.178)	(0.151)	(0.148)	(0.185)
Percent of adults ' college degree				
Estimate	0.00003*	0.00008*	-0.00007	-0.00004
Standard error	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Percent of villages w full electrification				
Estimate	-0.002	-0.004	0.004	-0.002
Standard error	(0.007)	(0.006)	(0.007)	(0.006)
Percent of villages w medical center				
Estimate	0.002	-0.005	0.015	0.005
Standard error	(0.005)	(0.007)	(0.010)	(0.009)
Percent of villages bus service				
Estimate	0.009	-0.004	0.015***	0.004
Standard error	(0.007)	(0.004)	(0.005)	(0.004)

Notes: Independent variable is number of with recorded famines (famine code of 2 or above). Control specifications: (a) no controls, (b) land-tenure control (proportion of villages with tenant-ownership land tenure system), (c) geographic controls (see section three for enumeration), (d) both land-tenure and geographic controls.

Source: Author calculations. These are more table notes. The style is Table Notes.

*** Significant at the 1 percent level or below ($p \leq 0.01$).

** Significant at the 5 percent level ($0.01 < p \leq 0.05$).

* Significant at the 10 percent level ($0.05 < p \leq 0.1$).

5 Weather Shocks as an Instrument for Famine Severity

As explained in section two, there are many possible reasons why recorded famine data may not be exogenous. In any case, it would be desirable to have a truly exogenous measure of famine, for which we turn to climate data in the form of rainfall shocks. As mentioned in section two and in section three, rainfall is of course plausibly connected to the occurrence of famines, especially when the policy of the colonial government was often to adopt a laissez-faire approach to famine relief (Bhatia 1968). For example, across all districts, mean rainfall averaged around 1.31m in years without famine and around 1.04m in districts which were at least somewhat affected by famine (code 1 or above). Figure 2 below shows that there is a strong correlation between rainfall activity and famines in colonial India. It should be clear from the scatterplots that there is, as expected, a negative relationship between the amount of rainfall a district receives and the general prevalence of famine, but more importantly, the total size of the rainfall shocks and the total occurrences of famine in that district.

Figure 2: Associations between rainfall and famine occurrence

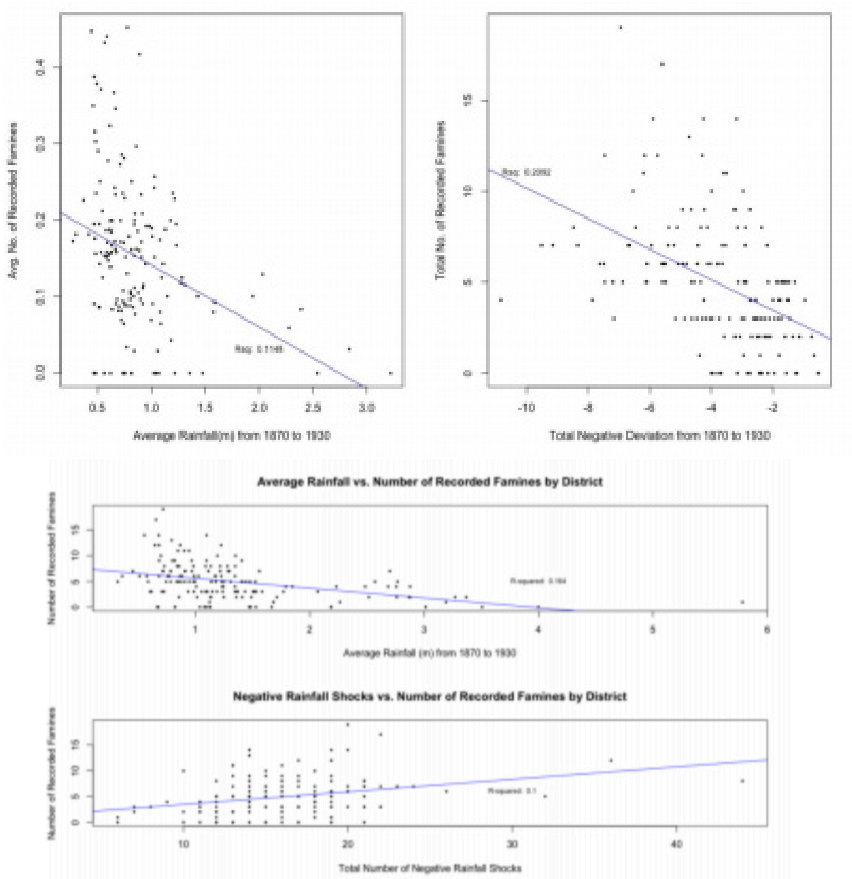


Fig. 2: The R^2 values indicated in each plot denote the coefficient of determination for the simple linear model predicting the variable on the y-axis from the variable on the x-axis (i.e, the R^2 corresponding to the blue line of best-fit, with no additional controls).

In order to use this to measure famine exogenously, we use two-stage least-squares, where we first estimate (2) (see section two and section three) and predict the number of famines from the number of negative rainfall shocks as represented by deviation from the mean in the bottom fifteen percent of all deviations, before estimating (1) using this estimate of famine instead of the recorded famine counts. Thus, we observe the effects caused by famine due to rainfall shocks, which can be interpreted causally since they are essentially random. First, however, in order to be sure that historical rainfall thus indirectly impacts outcomes in the present, we run (1) using the number of negative rainfall shocks directly. These reduced form estimates are presented on the following pages in Table III, before we present the results of the actual two-stage least-squares estimation in Table IV. All estimates are accompanied by 90% confidence intervals.

$$\widehat{famine}_i = \lambda + \theta * rainfall_i + X_i^T \eta + \nu_i \quad (2) \rightarrow$$

$$y_i = \alpha + \beta * \widehat{famine}_i + X_i^T \gamma + \varepsilon_i \quad (1)$$

$$y_i = \alpha + \beta * rainfall_i + X_i^T \gamma + \varepsilon_i \quad (2)$$

Table 3 – Reduced form estimates for IV

Control specification:	(a)	(b)	(c)	(d)
Dependent Variable:				
Log absolute magnitude per capita				
Estimate	0.177***	0.112**	0.056	-0.015
Standard Error	(0.054)	(0.051)	(0.049)	(0.050)
Log consumption per capita				
Estimate	-0.060***	-0.065***	-0.045**	-0.044**
Standard error	(0.018)	(0.020)	(0.017)	(0.018)
Share of workforce in cultivation				
Estimate	0.028***	0.027***	0.032***	0.030***
Standard error	(0.009)	(0.009)	(0.011)	(0.010)
Gini index				
Estimate	0.020***	0.017**	-0.012	-0.014
Standard error	(0.008)	(0.008)	(0.011)	(0.011)
Intergenerational income mobility EEs				
Estimate	1.660**	0.871	0.557	-0.145
Standard error	(0.734)	(0.658)	(0.604)	(0.576)
Percent of adults with college degree				
Estimate	-0.001**	-0.001**	-0.001***	-0.001***
Standard error	(0.0004)	(0.0004)	(0.0001)	(0.0001)
Percent of villages with full electrification				
Estimate	0.002	0.007	0.005	-0.046**
Standard error	(0.022)	(0.029)	(0.024)	(0.020)
Percent of villages with medical center				
Estimate	0.002	0.007	0.005	-0.046**
Standard error	(0.022)	(0.029)	(0.024)	(0.020)
Percent of villages with bus service				
Estimate	0.046	0.020	-0.001	0.002
Standard error	(0.031)	(0.015)	(0.013)	(0.015)

Notes: Independent variable is number of years in which deviation of rainfall from the historic mean is in the bottom fifteenth-percentile. Control specifications: (a) no controls, (b) land-tenure control (proportion of villages with tenant-ownership land tenure system), (c) geographic controls (see section three for enumeration), (d) both land-tenure and geographic controls.

Source: Author calculations. These are more table notes. The style is Table Notes.

*** Significant at the 1 percent level or below ($p \leq 0.01$).

** Significant at the 5 percent level ($0.01 < p \leq 0.05$).

* Significant at the 10 percent level ($0.05 < p \leq 0.1$).

From Table III, it would appear that negative rainfall shocks have similar effects on the outcome variables as do recorded famines in terms of the statistical significance of the coefficients on the independent variable, with the added benefit that we can confirm our very small and slightly negative effects of famines on the proportion of adults with a college education (where for each additional year of exceptionally low rainfall in a district, the number of adults with a college education in 2011 decreases by 0.1%). In addition, whereas the coefficients in Table II were conflicting, Table III provides evidence in favor of the view that additional famines increase inequality in a district in terms of the Gini index.

However, the magnitudes of the effects of famines or low-rainfall years are almost all larger than their counterparts in Table II to a rather puzzling extent. While we stated earlier in section three that famines and rainfall are not perfectly correlated, it might be that variation in historical rainfall shocks does a better job of explaining variation in outcomes in the present day. In order to get a better understanding of the relationship between the two, it would be wise to look at the coefficients presented in Table IV, which are the results of the two-stage least-squares estimation using low-rainfall years as an instrument for recorded famines.

Table IV follows the patterns established in Table II and Table III in regards to the statistical significance of the coefficients as well as their signs; famines have a significant positive impact on nighttime luminosity, a significant negative impact on rural consumption, and a positive impact on the percent of the labor force employed in agriculture, with similar results to Table II concerning the impact of famine on the proportion of adults with a college education. Most other specifications do not show a significant effect of famine on the respective outcome, with the exception of access to medical care, where unlike in Table II and Table III, each additional famine is associated with an additional 11.2 to 12.5 percent of villages in that district having some form of medical center or service readily accessible (according to the specifications with geographic controls, which we argue are more believable than the ones without). This, however, breaks down at the level of famines seen in some of our districts; a district having suffered nine or ten famines (approximately 30% of our sample) would be predicted to have more than 100% of its villages having access to medical centers, which is clearly nonsensical. This provides some more motivation to look for nonlinearity in the effects of famine in section six.

Table 4 –Instrumental Variables Estimates

Control specification:	(a)	(b)	(c)	(d)
Dependent Variable:				
Log absolute magnitude per capita				
Estimate	0.094**	0.066*	0.051	0.007
Stand ard Error	(0.040)	(0.040)	(0.073)	(0.084)
Log consumption per capita				
Estimate	-0.041***	-0.048***	-0.066	-0.083
Standard error	(0.015)	(0.017)	(0.041)	(0.058)
Share of workforce in cultivation				
Estimate	0.015***	0.015**	0.044**	0.052*
Standard error	(0.006)	(0.006)	(0.020)	(0.028)

Gini index				
Estimate	0.010*	0.010	-0.016	-0.024
Standard error	(0.005)	(0.006)	(0.012)	(0.017)
Intergenerational income mobility EEs				
Estimate	1.007*	0.553	0.142	-0.487
Standard error	(0.523)	(0.481)	(0.921)	(1.089)
Percent of adults with college degree				
Estimate	-0.0001	-0.0002	-0.002*	-0.002
Standard error	(0.002)	(0.001)	(0.001)	(0.001)
Percent of villages with full electrification				
Estimate	0.027*	0.010	0.005	-0.019
Standard error	(0.015)	(0.013)	(0.030)	(0.036)
Percent of villages with medical center				
Estimate	0.026	0.022	0.112**	0.125*
Standard error	(0.016)	(0.018)	(0.054)	(0.074)
Percent of villages with bus service	0.034**	0.017	0.028	0.012
Estimate	(0.014)	(0.012)	(0.024)	(0.025)
Standard error				

Notes: Independent variable is number of years with recorded famines (famine code of 2 or above), instrumented with number of low-rainfall years (rainfall deviation from historic mean in bottom fifteenth percentile). Control specifications: (a) no controls, (b) land-tenure control (proportion of villages with tenant-ownership land tenure system), (c) geographic controls (see section three for enumeration), (d) both land-tenure and geographic controls.

Source: Author calculations. These are more table notes. The style is Table Notes.

*** Significant at the 1 percent level or below ($p \leq 0.01$).

** Significant at the 5 percent level ($0.01 < p \leq 0.05$).

* Significant at the 10 percent level ($0.05 < p \leq 0.1$).

Unfortunately, unlike in Table III, it seems that we cannot conclude much regarding the effect of famines on intergenerational mobility, as the coefficients are contradictory and generally not statistically significant. For example, the coefficient on famine in the model without any controls is highly significant and positive, but the coefficient in the model with all controls is not significant and starkly negative. The same is true for the effect of famines on the Gini coefficient. One possibility is that the positive coefficients on famine for both of these dependent variables are due to outliers, as we have slightly less than two-hundred data points.

The magnitudes of the coefficients in Table IV are generally smaller than those presented in Table III, but still significantly larger than the ones in Table II. For example, in Table II the ordinary least-squares model suggests that each additional famine is associated with an additional 0.5 to 0.9 percent of the district's workforce being employed in cultivation in 2011, but in Table IV these numbers range from 1.5 to 4.3 percent for the same specifications, in some cases representing almost a tenfold increase in magnitude. This is likely due to the elimination of attenuation bias from the earlier least-squares regressions, since we use instruments assumed to be uncorrelated with any measurement error in the recording of famines (Durbin 1954). On the other hand, the Hausman test for endogeneity often fails to reject the null hypothesis that the recorded famine variable taken from Srivastava (1968) and Donaldson and Burgess (2012) is already exogenous, as discussed in the subsequent pages. We are unable to conduct Sargan-Hansen tests for overidentification since we only have one instrument, and the test requires at least two.

How reliable are our instrumental variables estimates? Table V on the following page offers mixed support. While the weak-instrument test always rejects the null-hypothesis of instrument weakness, for models with more controls, namely those with geographic controls, the first-stage F-statistics are relatively small, as generally a value of ten or more[7] is recommended to verify instrument strength (Staiger and Stock 1997). Indeed, in Table IV we show confidence intervals obtained by inverting the Anderson-Rubin test, which accounts for instrument strength in determining the statistical significance of the coefficients. These are wider in the models with more controls, although not usually wide enough to move coefficients from significance to insignificance.

However, additional complications arise when considering the Hausman tests for endogeneity. The p-values in Table V suggest that around half of the regression specifications in Table IV do not actually suffer from a lack of exogeneity, meaning that the ordinary least-squares results are just as valid for those specifications. A more serious issue is that for four out of nine outcome variables, the Hausman test rejects the null-hypothesis of exogeneity. Combined with the fact that the first stage F-statistics are concerningly low for the specifications with geographic controls, this means that not only are the ordinary least-squares results likely to be biased, but the instrumental variables estimates are also likely to be imprecise. This matters most for the results concerning rural consumption and percent of the workforce in agriculture, whereas the results for nighttime luminosity are not affected as the Hausman tests do not reject exogeneity for that outcome variable.

Table 5 – Instrumental Variables Diagnostics

Table 5 – Instrumental Variables Diagnostics

Control specification:	(a)	(b)	(c)	(d)
Dependent Variable:				
Log absolute magnitude per capita				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.085	0.151	0.864	0.968
Log consumption per capita				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.013	0.008	0.018	0.020
Share of workforce in cultivation				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.152	0.170	0.001	0.001
Gini index				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.204	0.233	0.162	0.123
Intergenerational income mobility				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.027	0.047	0.900	0.718
Percent of adults with college degree				
First-stage F-statistic	25.56	21.96	5.591	3.967

Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.772	0.824	0.032	0.038
Percent of villages with full electrification				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.224	0.439	0.679	0.480
Percent of villages with medical center				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.224	0.439	0.679	0.480
Percent of villages with bus service				
First-stage F-statistic	25.56	21.96	5.591	3.967
Weak-instrument test p-value	<0.0001	<0.0001	0.012	0.048
Hausman test p-value	0.022	0.029	0.682	0.862

Notes: The weak-instrument test p-value is obtained from comparison of the first-stage F-statistic with the chi-square distribution with degrees of freedom corresponding to the model (number of data points minus number of estimands). Independent variable is number of years in which deviation of rainfall from the historic mean is in the bottom fifteenth-percentile. Control specifications: (a) no controls, (b) land-tenure control (proportion of villages with tenant-ownership land tenure system), (c) geographic controls (see section three for enumeration), (d) both land-tenure and geographic controls[6].

Source: Author calculations.

While we might simply use the ordinary-least squares results to complement the instrumental variables results where the latter are lacking in terms of instrument strength, the differences in magnitude between the coefficients presented in Table II and in Table IV are too large to do this without abandoning consistency in interpretation. Ultimately, given that the Hausman tests show that instrumentation is at least somewhat necessary and the actual p-values for the weak-instrument test are still reasonably low (being less than 0.05 even in the worst case), we prefer to uphold the instrumental variables results, as imperfect as some of them may be. We

6 To be precise, this heuristic is technically only valid with the use of a single instrument, which is of course satisfied in our case anyway.

argue that it is better to have unbiased estimates from IV, even if they may be somewhat unreliable, than to risk biased results from OLS where exogeneity conditions are likely not satisfied.

Before discussing the meaning of our findings, we first present one more investigation into the possible nonlinear effects of famines on development in the next section

6 Potential Non-linear Effects to Famines

While we have shown that there exists a credible long-run effect of famine on certain outcomes, we would like to further investigate how districts recover from famines and whether this impacts their growth in the long-run, according to our discussion in section two. In this section, we do this specifically by using the fact that these famines occurred repeatedly in the same place, which provides a unique opportunity to study the potential cumulative effects of natural disasters. As discussed in section two, the existence of diminishing or increasing marginal effects to famine would imply differences in how districts suffer and recover from famines, such as varying recovery times. For example, if we were to find that the marginal effect of each additional famine becomes greater with additional famines, then the recovery time increases with more famines. On the other hand, if we were to find that subsequent famines cause less damage than earlier ones, this would imply that famines initially impact districts to a certain extent, after which subsequent famines have little to no effect.

Our goal here is to estimate the marginal effect as a flexible function instead of a constant. We do so in two ways. First, we take advantage of the fact that famine is measured in counts and treat the number of famines as a categorical variable, i.e. we replace the singular famine variable with a series of indicators, as shown in equation (4), re-printed below for convenience:

$$y_i = \alpha + \sum_{j=1}^{20} \beta_j famine_{ij} + X_i^T \eta + \varepsilon_i \quad (4)$$

(4)

This allows us to estimate different coefficients at each level of famine. Each indicator famineij is equal to one if district i experiences j famines, and zero if not. In order to avoid perfect collinearity, we omit j = 0 for the baseline. For example, we noted at the beginning of the paper that Pune in Maharashtra (historically, Bombay Presidency) suffered thirteen famines as defined in section three based on our data. Therefore, the value of famineij for Pune would be one for j = 13 and zero for all the other j. The range of j is based on our data, with twenty being the maximum observed in more than one district. Fortunately, there are enough observations at each level of famine to estimate each of the βj, although the variability is somewhat high. Unfortunately, there are not nearly enough observations to test the hypothesis that the coefficients are all the same (obviously, we do test the hypothesis that the coefficients are equal to zero, and generally reject it), so the associated Wald test would not have enough power.

Therefore, what the estimated coefficients in (4) reveal a priori is the total effect of famine at each level of famine vis-à-vis a baseline of no famines. So, for example, the value of βj for j = 3 represents the difference in outcome between a district having suffered exactly three famines vs a district having suffered none. If for j = 4 this value is higher, then that means the effect of suffering exactly four famines is more extreme. Taking the difference of the two values yields the estimated marginal effect of the fourth famine. By estimating βj and then first-differencing the values (i.e. computing (the βj+1 - βj for j = 0...19) we are able to obtain a series of estimates for the marginal effects for the second, third, fourth, etc. famine up through the twentieth. In other words, linear regression forces the marginal effect of each additional famine to be the same, which allows us to estimate a different marginal effect at each level of famine. Thus, if these values (those of βj+1 - βj for j = 0…19) are increasing or at least increasing in trend, then the effects of famine would appear to be more severe at higher levels of famine.

As in sections four and five, we estimate four specifications with varying sets of controls, so that the first specification with no controls is equivalent to a fully nonparametric spline regression. Figure 3 on the following pages provides plots of our estimated marginal effects on famine obtained from equation (4) as described above, i.e. plots of the various $\beta_{j+1} - \beta_j$. We have also included lines of best fit so that it is easier to understand whether or not the marginal effects are increasing, decreasing, or constant, as the individual values are rather noisy due to underlying noisiness in the values of the β_j as described above.

In addition, we estimate (1) semi-parametrically as discussed in section two. We allow the effect of famines to be flexible in the number of famines, but constrain it to be linear for the control variables. Practically, we run spline-smoothed regressions of y on famine, estimating (5) below:

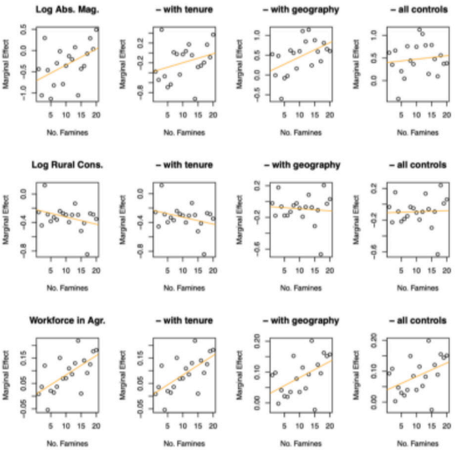
$$y_i = \alpha + h(famine_i) + X_i^T \eta + \varepsilon_i \quad (5)$$

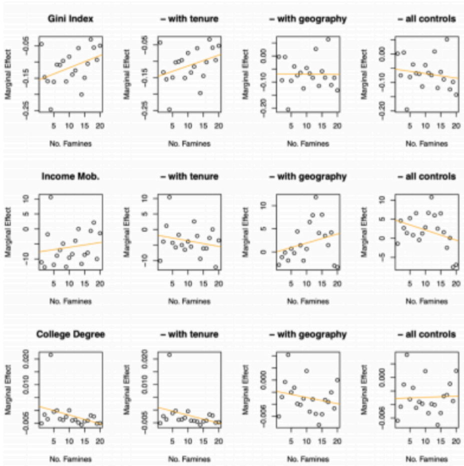
Figure 4 provides plots of the spline regressions and associated confidence intervals for the estimated curve. For brevity, we have only included in Figure 4 graphs for the specifications without controls, which are thus fully nonparametric estimates of the long-run impact of famine.

Figure 3 also includes lines of best-fit through the coefficients. For the models that had statistically significant coefficients on famine in Table IV, the fit is much better and the lines are positive or negative corresponding to the sign of the corresponding coefficient on famine, which suggests an increasing marginal effect of famine. For example, as Table IV suggested a significantly positive effect of the number of famines on luminosity, we observe in Figure 3 that the line of best fit is strongly positive, which suggests that the effect of each additional famine is increasing in the number of famines. Similarly, Table IV also suggested that there was a slightly negative impact of famines on the proportion of adults with a college degree within a district, and we observe that the lines of best fit for that specification in Figure 3 are indeed negative, which suggests that additional famines have stronger negative effects on the proportion of adults with a college degree. For most of the other models, however, the fit is too poor to be of use. In general, the various coefficients tend to be relatively scattered. In many cases the line of best-fit crosses zero, which suggests that the effect of famines can switch sign depending on the number of famines—which does not fit with the theory developed in Section Two. Such problems are particularly evident in the outcomes of income mobility, medical infrastructure, and bus service which, as noted above, did not have statistically significant coefficients on famine in Table II, III, or IV. For example, for income mobility, not only are the estimated marginal effects highly variable, but the lines of best fit also alternate between positive and negative slopes, according to the inclusion of controls. This set of estimations essentially tells us nothing. These problems are partly due to the nature of estimation as using twenty levels of famine means that each line is fitted with only twenty data points. However, these problems are mostly due to a lack of sufficient data to estimate separate coefficients for twenty separate levels of famine for less than two hundred districts.

Figure 3: Estimated marginal effects of famine counts (next page)

Fig. 3: Plotted points are the estimated marginal effect at each level of famine (see p.43 for the derivation) with the line of best fit shown in orange. Dependent variable labeled on the leftmost plot of each row (no controls), with hyphens indicating the addition of the control variables specified after the hyphen.


Log Abs. Mag.
– with tenure
– with geography
– all controls
Log Rural Cons.
– with tenure
– with geography
– all controls
Workforce in Agr.
– with tenure
– with geography
– all controls
Marginal Effect
No. Famines


Gini Index
– with tenure
– with geography
– all controls
Income Mob.
– with tenure
– with geography
– all controls
College Degree
– with tenure
– with geography
– all controls
Marginal Effect
No. Famines

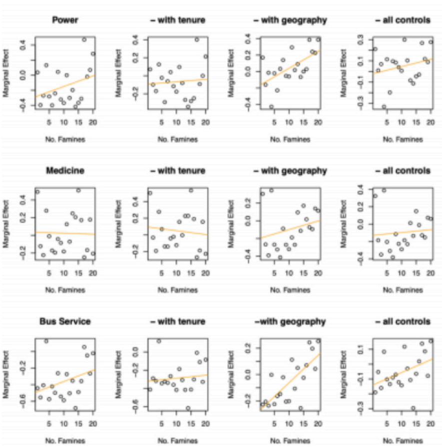

Power
– with tenure
– with geography
– all controls
Medicine
– with tenure
– with geography
– all controls
Bus Service
– with tenure
–with geography
– all controls
Marginal Effect
No. Famines

Figure 4: Non-parametric estimates of the impact of famine

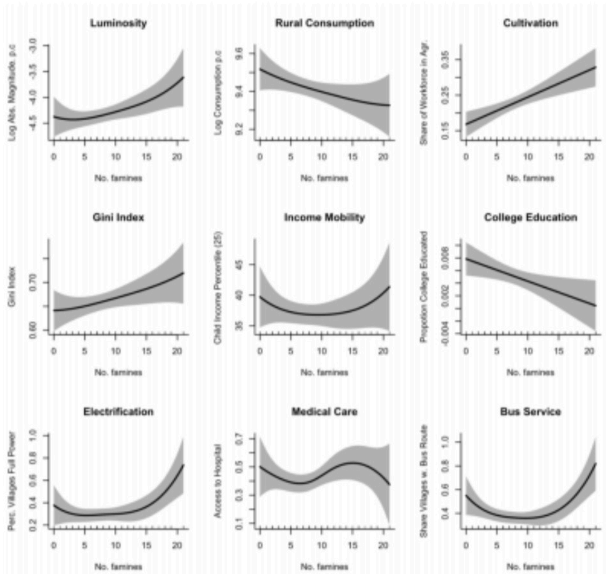


Fig 4: Functions are estimated using a series of cubic splines without controls, with shaded areas indicating 95% confidence bands.

While Figure 3 provides some evidence to suggest that the effect of famine is increasing in magnitude[7], Figure 4 does not provide sufficient evidence to back up this claim. First, the estimated functions for the outcomes we are interested in, namely those which had statistically significant outcomes on famine in Table II and Table IV, the estimated functions are roughly linear. For many others, the function may be non-linear but the confidence bands do not exclude the possibility of linearity. Secondly, even for the outcomes which do exhibit marked non-linearity in

7 I.e, to be clear where famine has a positive impact on the outcome, the impact is higher at higher levels of famine and additional famines have more positive impacts; where famine has a negative outcome, the impact is more negative at higher levels of famine and additional famines have even more negative impacts. In other words, the marginal effect of famines on outcomes for which it is already positive is increasing, and the marginal effect of famines on outcomes for which it is already negative is decreasing.

famine in both the fitted curve and the confidence bands, the outcomes themselves were not statistically significant in Table II and IV, so their usefulness is limited. Third, the use of semi-parametric techniques fails to resolve the issue in Figure 3 of coefficients on each level of famine tending to have both positive and negative signs; similarly, here, for the bus service, medical infrastructure, electrification, and mobility outcomes, the estimated function is non-monotonic. As discussed in the subsequent section, the particular pattern of a concave curve strangely suggests that low levels of famine may have harmful effects in the long-run, but higher levels have beneficial long-run effects in terms of district development, which we attempt to explain in the subsequent section. In any case, we cannot infer from these results that the effect of famine is not linear.

The major source of our imprecision is that when the effects of famine as detailed in Table II, III, and IV tend to be rather small to begin with, attempting to distinguish how the effects change with the level of famine is even more difficult. Another issue is possibly that our construction of the famine counts from the original dataset in Srivastava (1968) and Donaldson and Burgess (2012) masks information regarding the severity of famine, by treating famine codes of 2,3, and 4, which represent different levels of famine intensity, singularly as famine years. This interpretation is unaffected by the fact that we are comparing the coefficients to a baseline of districts that never experienced a single famine. In general, the biggest problem with the technique used for Figure 3, however, is that the estimated effects are very noisy due to only having 179 district-level observations.

Therefore, while we have some evidence to suggest that the effect of famines is nonlinear and that the marginal effect is possibly not constant, we do not have enough evidence to confirm this hypothesis, as more data is needed to be able to estimate the models in this section with sufficient power. In the following section, we consequently focus the bulk of our discussion of the results on those obtained in sections four and five.

7 Discussion

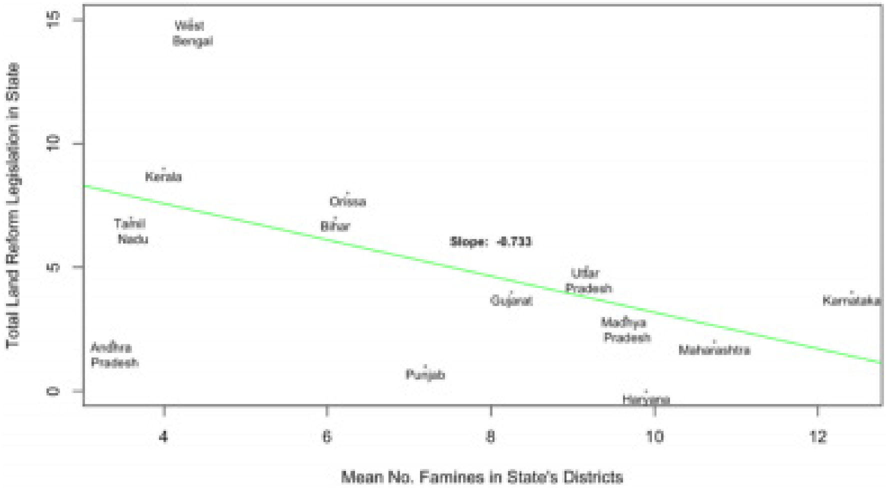
Are there long-run impacts to historical famines? Our data suggests so. Tables II, IV, and VII clearly show that the number of historical famines has a statistically significant, although sometimes small, impact on the average level of economic development as proxied by nighttime luminosity, the share of the population employed in cultivation, consumption, inequality, and even the provision of medical services in contemporary Indian districts. On the other hand, there appear to be no discernible effects on intergenerational income mobility or basic infrastructure such as electrification. The effects are quite small in comparison to the impact of other colonial-era policies such as land-tenure systems, and are generally overshadowed by other geographical factors such as climate (i.e., latitude and temperature). Nevertheless, they are still interesting to observe given that the famines in question occurred nearly a hundred years prior to the measurement of the outcomes. We believe that they reveal lasting and significant consequences of British food policy in colonial India; for example, Table IV suggests that a district having suffered ten famines, which is not atypical in our data, may have developed as much as ninety-four percent more log absolute magnitude per capita, around forty percent less consumption per capita in rural areas 150% percent more of the workforce employed in cultivation, and a Gini coefficient nearly ten percent greater than a district which suffered no famines. Aside from the question of whether or not the famines were directly caused by British policy, as the use of climate shocks demonstrates the presence of a random component that could not have been associated with British policy, the results suggest that at the very least, British nineteenth-century laissez-faire attitudes to disaster management may have had long-lasting consequences for India. Moreover, these estimates are causal in that since the use of rainfall shocks as instruments provides a means of estimation which is "as good as random", we can confidently state that these effects are truly the result of having undergone the observed famines and not simply there by association.

Should we trust the instrumental estimates even though sometimes the instrument is not as strong as we would like (as noted in Table V), or should we prefer the ordinary least-squares estimates in Table III even though they do not allow for a causal interpretation, because sometimes the Hausman tests (also shown in Table V) for the instrumental specifications fail to reject the null hypothesis of exogeneity, and therefore suggest that the instruments are not needed? We argue for the former. First of all, as the rainfall measures are truly as-good-as-random, being climate-shocks, the instrumentation of the recorded famine data with the demeaned rainfall data provides reasonably causal estimation. Even if the recorded famine measure is itself reasonably exogenous as suggested by the Hausman tests, we argue that it is better to be sure, as using instruments for a variable that is already exogenous will not introduce additional bias into the results, and may even help reduce attenuation bias from any possible measurement error (the Hausman test, after all, cannot completely eliminate this possibility, it can only suggest how likely or unlikely it is). In this sense, the instrumental estimates give us "peace of mind" by allowing us to be far more confident in our assessment of the presence or absence of the long-run impact of famines. Even the drawback with the weak-instrument problem for the specifications with controls is not so severe; even though the first-stage F-statistics are less than ten, they are still large enough to reject the null hypothesis of instrument weakness as shown by the p-values for this test in Table V. Finally, we argue that it is better to be consistent rather than picking and choosing which set of estimates we want to accept for a given dependent variable and model because the differences in magnitude between the IV and OLS coefficients are too large to do otherwise.

A more interesting question raised by the reported coefficients in Table II, Table IV, and Table VII is that of their sign. Why do more historically famine-afflicted districts seem to have more economic development as suggested by the use of our proxy for direct observation of GDP or income per capita, yet worse outcomes in terms of rural consumption and inequality? One possible explanation is that this is largely due to redistributive preferences associated with or possibly even caused by the famines, which is a hypothesis supported elsewhere in the literature, for example in Gualtieri et al. (2019) regarding earthquakes in Italy. We note that districts suffering more famines in the colonial era are more "rural" today in that they tend to have a greater proportion of their labor force working in cultivation. This cannot be a case of mere association where more rural or agricultural districts are more susceptible to famine, as our instrumental estimates in Table IV suggest otherwise. Rather, we explore the possibility that post-independence land reform in India was greater in more agricultural districts. Much of the literature on land-tenure suggests that redistributing land from large landowners to smaller farmers has casually positive effects on productivity and therefore economic development (Iyer and Banerjee 2005, Varghese 2019). If the historical famines are causally associated with districts having more unequal land tenure at independence, then this would explain their positive yet small impact on economic development by inducing, to a comparatively small extent, more land reform in those districts. On the other hand, if they are causally associated with districts remaining more agricultural at independence, and a district's "agriculturalness" is only indirectly associated with land reform (since they have more agricultural land, they benefit more from the reform), this would indicate that famines have a small and positive impact on economic development through a process that is slightly less causal in nature.

Although we are unable to observe land-tenure and agricultural occupations immediately at independence, we are able to supplement our data with additional state-level observations of land reform efforts in Indian states from 1957-1992 (compiled in Besley and Burgess (2010)) and aggregate the district-level observations of famines in our dataset by state[8]. If our hypothesis above is correct, then we should see a positive association between the number of historical famines in a state's district (keeping in mind that provinces-states were almost completely reorganized after independence) and the amount of land-reform legislation passed by that state after independence (although this data is quite coarse, being on the state level). However, the plot below suggests completely the opposite, as each additional famine across the state's districts appears to be associated with nearly 0.73 fewer land-reform acts. Even after removing the outlier of West Bengal, which underwent far more numerous land reforms during a long period of governance under the Communist Party of India, the relationship is still apparent, with every two additional famines being associated with almost one fewer piece of land-reform legislation post -independence.

Figure 5: Data points are labeled with the name of the associated state.



8 To be clear, the value of famine for each state is technically the average number of famines in the historical districts that are now part of the state, since subnational boundaries were drastically reorganized along linguistic lines after independence.

Figure 5b: without West Bengal

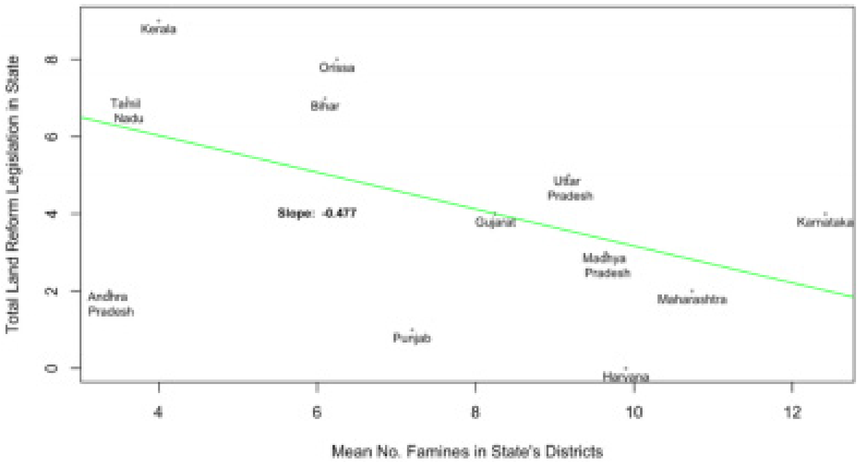


Fig 5: Data points are labeled with the name of the associated state. Line of best-fit redrawn with West-Bengal removed.

Therefore, the evidence rejects the proposition that historical famines are positively associated with later land-reforms. This is quite puzzling because it is difficult to rationalize how famine occurrence could lead to positive economic development while hurting outcomes such as inequality, consumption, and public goods provision.

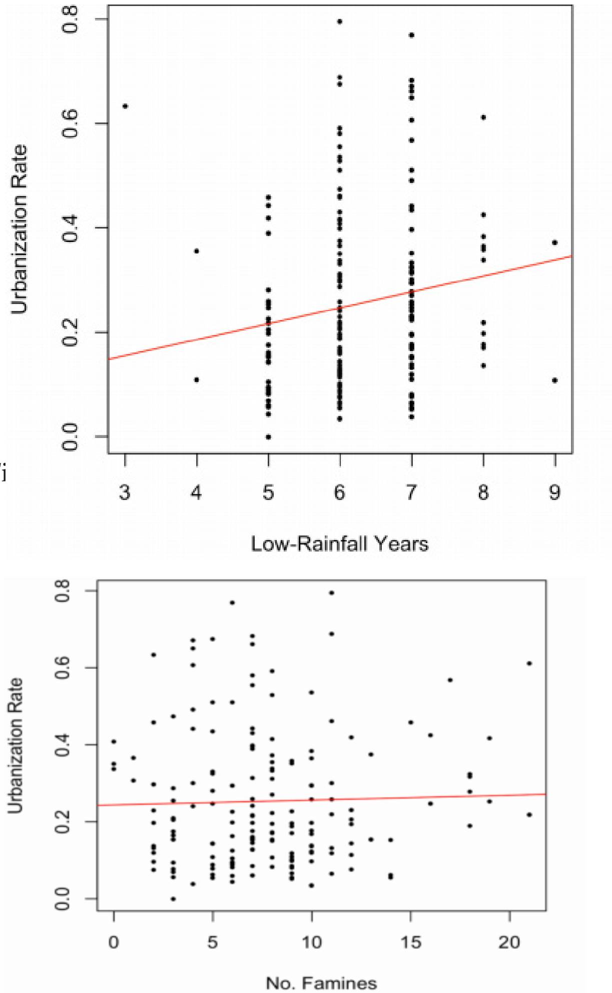
One potential explanation is that famines lead to higher urban development while hurting rural development, i.e. a key impact of famine occurrence is a worsening of an urban-rural divide in economic development. Such a consequence is definitely plausible; reports of village depopulation and of survivors seeking refuge in nearby urban centers abound in historical records. In addition, what little famine relief distributed by the British was often transported by rail, and urban centers were better served by railways (Carlyle 1900, Bose and Jalal 2004). This would explain how higher famine occurrence is linked with higher night-time luminosity, which would itself be positively associated with urbanization yet is also linked to lower rural consumption, higher inequality (which may be the result of a stronger rural-urban divide), and a higher proportion of the workforce employed in the agricultural sector. If famines somehow lead to more people living in urban areas while simultaneously leaving more of the remaining population employed in (likely closer to a level of subsistence) agriculture, then they would also exacerbate inequality and worsen rural economic outcomes. If the urbanization effect dominates, this would also explain the slight increase in night-time luminosity.

In addition, this might explain why certain outcomes in section six appear to be U-shaped functions of famine when estimated non-parametrically; perhaps for low levels of famine occurrences, the harmful effect of the famine dominates the beneficial effect due to increased urbanization, but at higher levels, the effect on urbanization dominates. In essence, it would seem that famines always hurt these outcomes in the long-run, but at high levels of famine, the positive, indirect effect via the effect on subsequent urbanization rates dominates the negative effects of the famines themselves. This makes more sense given the nature of the outcomes themselves; one would expect electrification, bus service and income mobility to be higher in urban areas, even if these outcomes themselves are lowered by famine occurrence.

Importantly, famines driving urbanization may also be able to explain the odd U-shaped patterns in certain development outcomes as noted in section six (income mobility and bus service). If famines are associated negatively with these outcomes but urbanization is associated positively with them and then if famines are also responsible for increased urbanization rates, then it is possible that the former effect dominates at low levels of famine, while the latter effect dominates at higher levels.

This hypothesis is somewhat supported by the plots in Figure 6, which show that urbanization (defined as the proportion of a district's population that is urban as defined in the Indian census) is weakly associated with famine occurrence when using rainfall shocks, positively associated with nighttime luminosity and inequality, and negatively associated with rural consumption and agricultural employment (exactly as hypothesized above, as shown in the plots in Figure 7). However, instrumental estimates of urbanization as a result of famine detailed in Table VI only weakly support the idea that famine occurrence causally impacts urbanization, as only the specification without any controls is statistically significant.

Figure 6: Urbanization rates vs. famine occurrence



Fi

Fig 6: X-axis is the number of years with deviation of rainfall from the district's historic mean in the bottom fifteenth percentile,while the y-axis is the percent of that district's population that was classified as urban in the 2011 Indian census.

Figure 7: Urbanization Rate vs. Development Outcomes

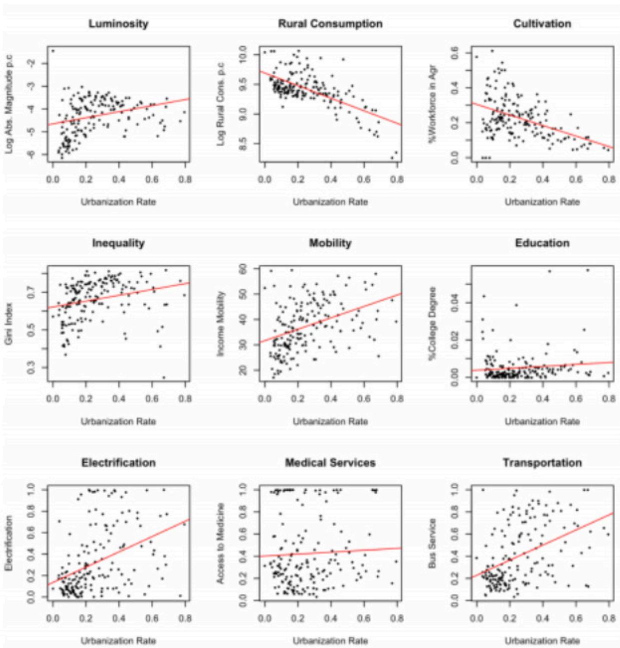


Fig 7: X-axis in each figure is the percent of the district's population that was classified as urban in the 2011 Indian census.

Table VI –Urbanization Vs. Famine Occurrence

Control specification:	(a)	(b)	(c)	(d)
Dependent Variable:				
Urbanization Rate				
Estimate	0.019**	0.015	0.017	0.013
Standard Error	(0.009)	(0.010)	(0.021)	(0.026)
Log consumption per capita				
Estimate	-0.041***	-0.048***	-0.066	-0.083
Standard error	(0.015)	(0.017)	(0.041)	(0.058)

Notes: Independent variable is the number of famines is instrumented with the number of low-rainfall years, i.e years with rainfall deviation in the bottom fifteenth percentile. Urbanization rate is the percent of a district's population that is urban as defined in the 2011 Indian census. Control specifications: (a) no controls, (b) land-tenure control (proportion of villages with tenant-ownership land tenure system), (c) geographic controls (see section three for enumeration), (d) both land-tenure and geographic controls.
Source: Author calculations.
*** Significant at the 1 percent level or below ($p \leq 0.01$).
** Significant at the 5 percent level ($0.01 < p \leq 0.05$).
* Significant at the 10 percent level ($0.05 < p \leq 0.1$).

Nevertheless, this represents a far more likely explanation for our results than land reform, especially since if famines being associated with land-reform at independence was the real mechanism behind our results, we would also expect that famine occurrence would then be associated with better rural outcomes, since the literature on land-reform, as noted previously, suggests that land-reform as linked with improved rural development. Therefore, not only is differential urban vs. rural development as a result of famine occurrence better supported by our data, but it is more plausible. While we do not have enough data to investigate exactly why famine occurrence seems to worsen urban-rural divides in economic development, such a question would certainly be a key area of future study.
As for our investigation into the repeated nature of the famines in India and the possibility of increasing or

diminishing effects of additional famines, we do not find strong evidence for the existence of such effects. Figure 3 weakly suggests that the marginal effect of each famine is increasing in a few cases, but there is not enough evidence to conclude that we have found a cumulative effect. Most of the marginal effects still appear to be constant, so that the overall trend is linear in the number of famines, and as noted in section six, it is difficult to be more precise regarding the nature of the marginal effects of famine occurrence when the effects themselves are small. We also emphasize that there is generally insufficient data to reliably perform the necessary estimations, as shown by the noisiness of our estimates in section six. Thus, in terms of the model we developed in section two, most of the evidence thus points to a very rapid recovery time, so that districts appear to recover from the damage caused by famines very quickly, before the next famine tends to occur. Although famines are not necessarily short-term disasters on the scale of tornadoes, earthquakes, or hurricanes, this is still generally consistent since famines cause little damage to existing infrastructure. Finally, while famines may exact large tolls on human capital, factors such as internal migration mitigate such damages in the medium to long term.

8 Conclusion

In this paper, we have shown that famines occurring in British India have a statistically significant long-run impact on present-day outcomes, using both ordinary least-squares and instrumenting for famine with climate shocks in the form of deviated rainfall. In particular, the occurrence of famine seems to exacerbate a rural-urban divide in economic development, as famines appear to cause a small increase in overall economic development but lower consumption and welfare in rural areas, worsening wealth inequality. This is supported by the finding that famines appear to lead to slightly higher rates of urbanization while simultaneously leading to a higher proportion of a district's labor force remaining employed in the agricultural sector.

Even though our ordinary-least squares measures are generally acceptable, we point to the similar instrumental variable estimates as stronger evidence of the causal impact of the famines. With regards to the repetition of famines, our evidence weakly suggests, but does not definitively show, that the absolute marginal effect of famines is increasing in the number of famines. Ultimately, our results demonstrate that negative climate chocks combined with certain disaster management policies, such as British colonial laissez-faire approaches to famine in India, may have significant yet counter-intuitive impacts on economic outcomes in the long-run.

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Evaluating the Impact of Chinese and World Bank Foreign Aid Projects on Preferences for Democracy in Tanzania

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Abstract

Recognizing that democracy is important for the efficient use of foreign aid, this paper examines whether the presence of foreign aid projects impact the democratic preferences for local residents in Tanzania. Considering China's increased presence as an international donor, we compare the democratic preferences for Tanzanians surrounding Chinese and World Bank aid projects. We match a geo-referenced data set with the subnational allocation of Chinese and World Bank development projects over the years 2000–2014 to 2,636 respondents from four Afrobarometer survey waves carried out in Tanzania. We thereby employ a spatial-temporal strategy which allows us to examine the extent to which development projects impact the democratic preferences for local Tanzanians surrounding project sites. We find no causal relationship between development projects and democratic preferences. Some results, however, imply that Tanzanians surrounding Chinese aid projects are associated with lower levels of democratic preferences compared to Tanzanians who reside near World Bank project sites.

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Table of Contents

1 Introduction

When asked what the most important thing to happen during the 20th century was, famed Indian economist and philosopher Amartya Sen replied without difficulty: the rise of democracy (Sen 1999). Despite a range of profound historical events which occurred during the 20th century, such as two world wars, the rise and fall of Fascism and Nazism, or the collapse of the Soviet Union, Amartya Sen still replied that the rise of democracy was the most important event during the 20th century.

If the 20th century can be characterized by the rise of democracy, then this begs the question if the 21st century will be characterized by its decline. Already authors have begun shedding doubts on the future of democratic development (see e.g. Dryzek 1997; Rosenthal 1998; Diamond 2015; Plattner 2015). For their 25th anniversary the Journal of Democracy published an article with the title: *Is Democracy in Decline?* Though the authors' conclusions ultimately differ in regard to the extent of which democracy is in decline, they highlight the growing somber mood amongst democrats for future democratic development. The grounds for this statement stem from the struggles in democracy-building in post-invasion Iraq, poor institutional development, shifting geo-politics and the strengthening of autocratic Russia and China (Plattner 2015).

The rise of autocratic China as an emerging global economic power has not only cast doubt on the effectiveness of democracy, but on the future of foreign development assistance. Brazys et al. (2017 p. 228), for instance, claims that: "The rise of China as a development partner has been one of the most important phenomena in the international development field over the past decade". Compared to the more traditional donors in for instance the World Bank, China's approach to foreign aid relies more on non-conditionality and non-interference. This approach has been welcomed by recipient countries, including many African countries, as they believe that conditions to aid enforced by more traditional donors have been far too constraining (Zhao 2014). However, this has also been met by a growing concern from authors who believe that Chinese aid practices may be undermining efforts in promoting good governance and accountability for African countries (Wang and Ozanne 2000; Collier 2007; Pehnelt 2007).

These concerns bring the rise of China as a new global development partner directly into the discussion of the relationship between development outcomes and good governance in Africa. Indeed, Wa Mutharika, former Malawian economist and politician, recognizes that for Africa to shake off its current political and governance issues, it has to embrace new directions based on regionalism, good governance, and democracy, and that these must emanate from within civil society (Ahluwalia and Zegeye 2001). Civil society, as Wa Mutharika highlights, has often been said to be a crucial factor in establishing and maintaining political democracy (Bratton 1994).

While most papers have focused on democracy promotion largely as an endogenous issue (Remmer 1995), Brown (2005) argues that international actors and donors have an important role to play in either promoting or hindering democracy. Furthermore, Svensson (1999) finds that foreign aid has had a positive impact on economic growth in countries which have had an institutionalized check on governmental power, i.e. more democratic countries. It is therefore of interest to research whether international donors and foreign aid itself may impact the democratic preferences of the recipient populations as more democratic countries may then in turn be better equipped to handle this aid.

This paper therefore seeks to address whether foreign aid may impact the preferences for democratic values. Recognizing that democracy promotion must stem from within civil society, we more specifically wish to address whether foreign aid projects impact the local preferences for democratic values surrounding development project sites. Considering China's emergence as a powerful player in the international donor community, and their differing foreign aid practices, we wish to address whether they impact the local preferences for democratic values differently than that of a more traditional donor in the World Bank. Specifically, the following questions will be addressed: a) if the implementation of Chinese aid projects affect the preferences for democratic values surrounding development project locations, b) if there is a systematic difference between preferences for democratic values surrounding Chinese project sites and World Bank project sites, and c) if so, what may explain these differences.

In order to determine the relationship between development projects and democratic preferences, we geographically match a georeferenced dataset from Afrobarometer with 2,636 respondents through four survey waves (rounds 3-6) in Tanzania with new data for the subnational allocation of Chinese and World Bank development projects in Tanzania over the period 2000-2014, provided by AidData. We compare the preferences for democratic values of individuals residing near a location where a project is being implemented at the time of the interview to that of individuals residing near a location where a planned project has yet to begin at the time of the interview. In doing so, we get a difference-in-difference type estimate that controls for time-invariant characteristics that may determine the choice of project locations. Similarly, we do this for World Bank development projects and compare those results to that of Chinese development projects to determine if the identity of the donor affects the local preferences for democratic values differently.

We choose to limit our study to Tanzania for two reasons. First, China and the World Bank have been similarly involved in Tanzania as development partners in both time and scope (Brazys et al. 2017). Thus, we can avoid biases in regard to length and scope of development practices which might follow "lead" donorship (Steinwand 2015). Second, Tanzania has made commitments regarding democracy-building to its people and the international community and consider ongoing rapid

democratic decline as a
concern (USAID 2018). Therefore, the relationship between development aid and the preference for democratic values is directly important for the Tanzanian government if they wish to continue their outspoken focus on democracy-building.

Our paper most similarly resembles that of Brazys, et al. (2017) and Isaksson and Kotsadam (2018), which both seek to disentangle whether Chinese development projects fuel local corruption and how Chinese projects differ from World Bank projects in that regard. Their articles differ in that Brazys et al. (2017) conduct their study by focusing on one country while Isaksson and Kotsadam (2018) include 29 African countries. Nonetheless, they both employ spatial strategies similar to ours. To our knowledge however, ours is the first article of its kind seeking to examine the relationship between development projects and preferences for local democratic values. Our paper thus contributes to the emerging quantitative literature concerning Chinese aid allocation and its effects on development outcomes at the micro level.

Our paper is organized as follows. The following section provides a background of global foreign aid practices and a background on China's, the World Bank's, and Tanzania's stance on democracy. Section 3 provides a literature overview of previous research concerning the aid effectiveness debate and how foreign aid may impact democracy. In section 4 we present our conceptual framework and hypotheses. Section 5 provides our data and empirical methods used to determine the relationship between aid and local preferences for democratic values. Section 6 presents our empirical results. Section 7 discusses the implications of these results and mentions some potential areas for further research. We conclude in section 8.

2 Background

This section aims to provide a background on the development of global foreign aid practices to better contextualize foreign aid and democracy promotion. We furthermore provide a brief background on how China, the World Bank, and Tanzania view democracy in order to illustrate their contrasting views on the matter. We provide first a definition of foreign aid.

2.1 *Foreign aid*

The Official Development Committee (DAC) of the Organization for Economic Cooperation and Development (OECD) has defined Official Aid (OA) as flows, of financial and technical nature, to developing countries with the intent of furthering economic development and welfare. These flows can take the form of grants, loans or credit, but cannot include flows for military purposes (OECD 2013).

Development flows are segmented into two categories; Official Development Assistance (ODA) and Other Official flows (OOF). The official definition of ODA was developed by the DAC in 1972 as official financing intended to further economic development and welfare. These flows can either be provided bilaterally, from government agencies directly to developing countries on the DAC List of ODA Recipients, or through multilateral institutions such as the World Bank. Further, the financial terms of ODA must be concessional and include a "soft loan", consisting of a donation component of at least 25 percent, with a 10 percent discount rate. Like OA, ODA can consist of grants, loans or credits, but can never support military purposes. Development aid that does not fulfill the requirements for ODA are classified as OOF (OECD N.d.).

2.2 *Historical development of foreign aid practices*

The basis for the development aid apparatus, as we today know it, was established following World War II. The devastation caused by the war gave rise to several organizations whose original missions were to assist those in need. During this time, aid was predominantly aimed at relief and reconstruction. Since then, these organizations have become institutions paramount to the foreign aid community. Among these institutions are Oxfam, the Development Assistance Committee (DAC), and the World Bank (Hjertholm and White 2000).

As the intensity of the Cold War increased during the 1950s, two thirds of total multilateral aid was provided by the US under the Mutual Security Act. Following this, aid took on the role as a political tool to contain the spread of communism and the expansion of the Soviet Union (Hjertholm and White 2000), as US policymakers feared that developing countries would develop in a non-capitalist manner (Wood 1986). The effectiveness of this agenda became widely debated and generated a lot of critique due to the attempt at leveraging political support through aid. Furthermore, the US became concerned that they were carrying an unproportioned amount of responsibility for an outcome that would be beneficial for countries all around the world. Following this, other countries' bilateral aid programs grew during the 1960s. This, in part, led to the founding of the DAC, whose mission it was to oversee and evaluate aid performance (Hjertholm and White 2000).

The great accomplishments of the aid system in the 1950s and 1960s contributed to a surge in multilateral aid during the 1970s (Wood 1986; Hjertholm and White 2000). The decade before, 80 percent of total aid was provided by the US, the UK, and France, but now these countries accounted for 50 percent of total aid flows as other countries rapidly increased their aid expenditure (Dudley and Montmarquette 1976). However, an issue with the aid system started to become more apparent. World Bank president Robert McNamara argued in 1973 that foreign aid efforts were not reaching the poor equitably, neither among developing countries nor within them (Wood 1986). This inequality would provide some developing countries with up to 100 times as many dollars of aid per capita compared to what some of the poorest developing countries were receiving. Following this observation, the donor community underwent a transition to make the most poverty-ridden countries the focal point of development aid efforts (Hjertholm and White 2000).

The international donor community underwent two major changes following the fall of the Soviet Union. First, Eastern Europe and former Soviet Union countries switched from the role of donor to recipient. Second, the aid system established new constraints in regard to the allocation of aid. During the Cold War, this was done on the basis of whether a regime was positively inclined towards the West or not. In a new bout of democracy promotion, however, donors started to distribute aid on the basis of good governance, rewarding democratization (Hjertholm and White 2000).

With the new emphasis on good governance, international donors reprioritized their aid programs causing a surge in democracy promotion. When the threat of the Soviet Union and its associated communism dissolved, Western countries took a greater interest into the domestic policies of the countries they were aligned with. Prominent was the issue of weak governance, which is why several donors developed policies demanding that bilateral and multilateral aid should incorporate political liberalization as a basis for its aid allocation (Brown 2005). This time, known as the "Third Wave" of democratization in developing countries, also brought forward concerns regarding how corruption affected economic development. Though most donors have found that democracy is the self-evident instrument to attain good governance and anti-corruption, this is not necessarily always the case (Marquette 2001). As an economic institution, the World Bank considers itself apolitical, and will therefore not acknowledge a political ideology as superior in combating corruption. Like other donors, the World Bank considers corruption evidence of poor institutions and a weak judiciary system, among other things, but it claims that autocratic and democratic regimes are equally able to implement anti-corruption strategies (Marquette 2001).

2.3 *View of democracy in China*

China has a mixed view of democracy which stems from a troublesome history in regard to both attempts and failures in implementing democratic institutions. At the start of the 19th Century, China attempted to install a republican government under Dr. Sun Yat-Sen. However, this attempt quickly crumbled and ultimately led to the formation of the People's Republic under Mao Zedong. Though the formation of the People's Republic made a "class-based" claim to democracy, underlying anti-democratic and illiberal sentiments fueled class struggles which culminated in the Cultural Revolution, and in more modern times, the Tiananmen Square protests of 1989 (Zhao 2001).

Moreover, there is the question of whether Chinese ideologies, such as Confucianism, are compatible with democracy. Liang Shu-ming (1990 p. 48), for instance, states, *"it is not that China has not entered democracy, it is rather that China cannot enter democracy,"* believing that Chinese values alone can provide the basis for a good society and that there is no room for democracy in Chinese culture. Mou Tsung-San (1992) furthermore doubts that the cornerstones of democracy, such as liberty, equality and human rights, can be integrated into Confucianism, which places an emphasis on duty, loyalty and family values (Li 1997).

Today, China is a one-party authoritarian state, regularly oppressing the media through strict monitoring, firewalls, shutting down publications or websites, and jailing journalists (Xu and Albert 2017). Furthermore, China has begun sending thousands of Muslim Uighurs to reeducation camps in an effort to eradicate "weeds" and "tumors" that are infected with "ideological illnesses" according to local officials (Hammond et al. 2018). Despite China's restrictions on freedom of speech and thought, survey polls consistently show support for Chinese governance and the ideal of political meritocracy, indicating that there is a public approval of the Chinese government (Bell 2018). This raises concerns to not only governance within China, but how this may influence governance in other countries, who may deem China's actions effective in countering political and social tensions.

2.4 The Worlds Bank's view of democracy

As mentioned previously, the fall of the Soviet Union in the 1990s resulted in a change in how the donor community approached foreign aid. Instead of channeling aid to geo-politically important countries, emphasis was put on allocating aid on the basis of good governance and political conditionality. Donors began offering help in the removal of authoritarian governments and promoting democracy through election assistance, support for civil society, judicial reform, training of the media, and combating corruption. For most donors, good governance equals democratic governance, viewing efforts such as improved participation, multi-party elections, accountability, and the strengthening of the rule of law as democratic attempts to improve good governance (Marquette 2001).

Not all donors share this view, however. The World Bank considers itself an apolitical economic institution, and therefore insists that democracy may not be the sole answer to promote good governance and to combat corruption. The Bank's allocation of aid also reflects this as they continuously distribute aid to democratic and authoritarian countries alike (Marquette 2001). Article III, section 5 (b) of the World Bank's Articles of Agreement states that: *"The Bank shall make arrangements to ensure that the proceeds of any loan are used only for the purposes for which the loan was granted, with due attention to considerations of economy and efficiency and without regard to political or other non-economic influences or considerations."* (World Bank 2012).

Despite the World Bank's official apolitical stance, Marquette

(2001) argues that the Bank does seem to endorse liberal democracy through the use of language (e.g. accountability, participation and transparency) and through which projects the World Bank ultimately chooses to fund. Marquette (2001) further states that it is difficult to see where the World Bank differs from other donors, in terms of democratization efforts, other than through excluding the word “democracy” from their official policy statements.

Furthermore, the World Bank itself is a democratic institution, comprising 189 member countries, or shareholders, who are represented by a Board of Governors. The president of the World Bank is appointed by the Board of Executive Directors for a five-year, renewable term (World Bank 2019). In other words, the World Bank adheres to democratic ideals of participation, separation of power and limited terms of office via its governance structure.

2.5 Government and democracy in Tanzania

Tanzania (then known as Tanganyika) gained independence from Great Britain in 1961. Julius Nyere became the country’s first independent prime minister, and the following year was elected president. Since 1965, Tanzania has been a one-party state, with the Tanganyika African National Union (TANU) being the only party in mainland Tanzania and Afro-Shirazi Party (ASP) being the only one in Zanzibar (Ngasongwa 1992). In 1977 the two parties merged to become the Chama Cha Mapinduzi (CCM) party, the country’s sole legal political party until 1992. The government during the 1960s embraced tighter state control and a socialist model of governance (Oxford Business Group 2019). For this reason, Tanzania had, and continues to have, a close relationship to the People’s Republic of China, having already established diplomatic relations in 1961 (Brazys et al. 2017).

Tanzania is today a multi-party state led by President John Magufuli who took office in 2015. Following Tanzania’s move from a one-party state in the early 1990s, the country has had regular multi-party elections. However, the opposition in Tanzania remains weak and the ruling party has remained in power for half a century. Since the last elections in 2015, the government has been increasingly cracking down on critics from the opposition, civil society, and the media. For instance, one of the first acts the president took upon gaining office was to ban actions of oppositional parties. Similar actions, such as restricting the free media and jailing members of the opposition, speak towards a gradual move away from a multi-party system and towards a one-party state. For these reasons, Tanzania has dropped dramatically on the Freedom House score from an aggregate score of 58/100 in 2017, to an aggregate score of 45/100 in 2019 (100 being most free) giving them a freedom status of "partly free" (Freedom House 2019).

Despite this, the country still ranks above neighbors in accountability, civil rights, and transparency. However, democratic decline poses a threat to this, and the government has therefore made commitments both to its people and the international community to focus on improving democratic governance (USAID 2018).

3 Previous Literature

This section will serve first to provide an overview of the literature on the effectiveness of foreign aid on economic growth and second to provide an overview of the literature that exists on how foreign aid may impact (or hinder) democracy. Ultimately, this section should provide for the reader an overview of the literature that exists so that the reader may better understand how our paper contributes to this literature, both in terms of the effectiveness of foreign aid on economic growth and on democracy promotion and how they are intertwined.

3.1 Foreign aid and economic growth

Historically, there have been three different stances on the effectiveness of foreign aid. The first stance argues that foreign aid has had a positive impact on economic growth (see e.g. Papanek 1973; Levy 1988; Sachs et al. 2004; Rajan and Subramanian 2005). The second stance argues that foreign aid has in fact been detrimental to development outcomes such as economic growth, democracy and corruption (see e.g. Griffin and Enos 1970; Bauer 1972; Weisskopf 1972; Friedman 1995; Easterly and Easterly 2006). The third stance argues somewhat more cautiously that foreign aid may be beneficial, but only under certain circumstances (see e.g. Burnside and Dollar 2000; Clemens et al. 2011).

Lately, a new wave of foreign aid research has emerged that posits that the reason the literature on the effectiveness of foreign aid has been so contended is that the impact of foreign aid is insufficiently large to measurably affect aggregate economic outcomes (Dreher and Lohman 2015). The argument that foreign aid may indeed have a visible positive effect on economic outcomes on regional levels, but not on national levels, has been termed the micro-macro paradox which illustrates the disparity between macro-level ineffectiveness and micro-level effectiveness of aid apparent in empirical studies (Mosley 1987; Dreher and Lohman 2015).

Until recently, it has been difficult to adequately measure the impact of foreign aid on development outcomes at the micro-level. This has largely been the case due to a lack of data detailing project-specific information regarding foreign aid and the lack of transparency of donor countries' foreign aid practices (Dreher and Lohman 2015). These issues have been somewhat addressed due to the rise in data availability following AidData and selected recipient countries' increased efforts in geo-coding aid projects and of the existence of comprehensive data material (Strange et al. 2017) that allows for, quantitative analysis of Chinese aid flows which had previously been impossible due to a lack of data (Isaksson and Kotsadam 2018).

The rise in data availability has resulted in a surge of articles seeking to address the impact of foreign aid on certain developmental outcomes at the micro-level (see e.g. Dreher and Lohman 2015; Berlin et al. 2017; Brazys et al. 2017; Isaksson and Kotsadam 2018). These articles provide support for the micro-macro paradox in that they find results at the micro-level which would have been difficult to provide at the macro-level. For instance, Isaksson and Kotsadam (2018) find that corruption is more widespread surrounding Chinese project sites compared to World Bank project sites. Dreher and Lohman (2015) test whether aid affects development at the micro level using night time light growth as a proxy for development and find significant correlations between aid and growth at the micro-level. The emphasis on the effectiveness of foreign aid at the micro-level has been important research as Dreher and Lohman (2015, p 421) claim that *"The lack of systematic empirical evidence on the effectiveness of aid below the country level is an important gap in the literature."*.

3.2 Foreign aid and democracy promotion

As the literature on the effectiveness of aid on economic growth and other developmental outcomes is relatively inconclusive, so too is the literature on the effect of foreign aid on democracy. Critics of foreign aid's impact on democracy promotion have found that aid is associated with a decrease in institutional quality and democratization (Bräutigam and Knack 2004; Djankov, Montalvo and Reynal-Querol 2008) or has had only a minor effect either way on democratization (Knack 2004). Other studies have found more positive effects of foreign aid on democratization, for instance that aid is associated with higher levels of democracy, in particular after the Cold War (see e.g. Goldsmith 2001; Dunning 2004).

The disappointing results that international democratization efforts have yielded can be attributed to a host of factors, but two reasons stand out in the literature. First, there are empirical challenges attributed to measuring foreign aid's impact on democracy. Wright (2009), for instance, states that many empirical studies that have aimed to establish the relationship between foreign aid and democracy employ a cross-sectional approach which averages out important variation such as changes in levels of democracy (typically, the Freedom House scores are used as the dependent variable). Secondly, Brown (2005) recognizes that democratization processes are largely endogenous and that there exist significant structural obstacles which hinder democratization within countries. Nevertheless, Brown (2005) goes on to state that international donors play an important role in either promoting, or preventing, democratization in African countries through the use of political conditionality to aid which might raise the cost of continued authoritarian practices. Brown thus professes that the impact donors may have on democratization in developing countries is largely exogenous.

Furthermore, foreign aid itself may not only be beneficial in promoting democracy, through the use of conditionality to aid, but democracy itself may be beneficial in how aid is ultimately used. For instance, Svensson (1999) finds that foreign aid has had a positive impact on economic growth in countries that had an institutionalized check on governmental power, in other words, more democratic countries. He argues that aid flowing to more authoritarian countries may be more commonly misused to satisfy the government's own non-productive goals. As such, this study provides a link between these two academic fields: development economics and political science. While on one hand, foreign aid may help to promote democracy, on the other hand, more democratic countries may then be better equipped to use this aid effectively.

Our paper thus contributes to this important gap in the literature, as mentioned by Dreher and Lohman (2015), in that we seek to establish the relationship between foreign aid and democracy at the micro-level, while keeping in mind that democracy itself may be essential in ensuring that foreign aid is efficiently utilized.

4 Conceptual Framework and Hypothesis

In this section we develop our conceptual framework which illustrates via which mechanisms we propose development projects may influence individuals' preferences for democratic values and how the source of the donor may impact local democratic preferences differently. We build our conceptual framework on the recent literature on the effect of foreign aid on micro-level economic outcomes, most notably corruption, and on two competing theories of aid.

4.1 Conceptual Channels

We propose that foreign aid projects may influence local preferences for democratic values via two main channels: a) close encounters with project workers leading to a change in norms, and b) experiences with corruption following increased economic activity surrounding project sites.[1]

With respect to the former, we propose that local Tanzanians' preferences for democracy will become influenced by coming into contact with Chinese and World Bank project workers through the transmission of norms and values that the project workers bring with them upon engaging in local communities. Upon engaging with foreign development experts and project workers, locals' embedded social norms and values may be challenged.

1 We define democratic values as values inherently in support of the notion of democracy. These values include, but are not limited to: liberty (including the freedom of belief in whatever you want and to be able to express your own opinions and ideas in public), justice (in that no group or person should be favored over another), equality (in that everyone should be treated equally, regardless of background) and popular sovereignty (in that the government receives its power from the people) (Learning To Give N.d.).

Chinese values of democracy differ from that of the World Bank as put forth in the background. Ceteris paribus, we should expect then to see Tanzanians living in close proximity to Chinese development projects, exhibiting values of democracy more resembling that of Chinese individuals. Similarly, we should expect to see local Tanzanians residing in close proximity to World Bank projects exhibiting values of democracy similar to that of World Bank values.

With respect to the latter, we argue similarly to Isaksson and Kotsadam (2018) that increased development flows may increase the economic activity surrounding the project sites increasing the level of available resources. As a result, corrupt activity may flourish around project sites, as they attract corrupt actors seeking to capitalize on the newly available resources (Karl 2007). The effect may however be the opposite. Donors who actively seek to monitor and engage proactively against corrupt activities may be able to curtail corruption despite the increased opportunity for corruption to flourish (Isaksson and Kotsadam 2018).

Individuals, officials, and local elites residing near project sites where corrupt activities and actors are present may then feel the need to engage in corrupt activities themselves (Brazys et al. 2017). If donors choose to neglect the corrupt activities their local partners are engaging in, this may lead to higher levels of corruption surrounding project sites. Corrupt behavior may therefore become normalized and embedded in the individuals' behaviors. If donors actively seek to combat corruption proactively and monitor local partners to prevent corrupt behavior, this may increase the perceived costs of engaging in corrupt activities, which could lead to lower levels of corruption surrounding project sites (Isaksson and Kotsadam 2018).

We argue here that corruption negatively influences democratic preferences based on the current literature on political corruption. Warren (2004 p. 328) states that "Corruption, it is increasingly noted, breaks the link between collective decision making and people's powers to influence collective decisions through speaking and voting, the very link that defines democracy." Corruption, Warren (2004) claims, leads to a loss of confidence and trust that public decisions are freely available and justifiable. The people may then become cynical of public speech and come to expect deception of public officials, whether or not they are corrupt. Individuals will, as a result lose faith in public goods and will instead choose to pursue narrower domains of self-interest which they can control. Morris and Klesner (2010 p. 1278) furthermore state that: "Analysis of political corruption, particularly in countries where corruption is endemic, suggests a vicious circle wherein corruption breeds a climate of distrust that in turn feeds corruption."

We should then expect to observe individuals who become increasingly subjected to corruption to exhibit lower levels of democratic preferences as the people's trust in politicians and political institutions diminish. We should therefore expect to observe individuals who are subjected to lower levels of corruption, or no corruption, to exhibit higher levels of democratic preferences compared to individuals who are subjected to increasing corruption.

4.2 Donor heterogeneity hypothesis

Local Tanzanians preferences for democratic values may thus be influenced differently through the two above proposed channels, and we argue that in which way democratic preferences are influenced is donor-dependent. By this we mean that depending on who the donor is, local democratic preferences may be influenced differently. We base this argument on two competing theories of aid: "donor control" and "donor capture" (Milner et al. 2016).

The "donor control" and "donor capture" theories rest on underlying assumptions with respect to the public and the donors. These theories claim that it is important who has more influence in this relationship. With respect to the "donor capture" model, more influence is emphasized on the recipient of aid. In this model, aid is allocated to recipients with little to no conditionality attached as to how aid should be used or allocated. An explanation for this is that aid could merely be used for strategic political purposes and thus aid will be provided to geo-strategically important countries (De Mesquita and Smith 2007, 2009). This type of aid is often more fungible by nature and, as such, recipient countries are more likely to be able to use this aid as they please. In corrupt or clientelist environments this aid is more likely to be misused for private gain (Milner et al. 2016).

The "donor control" theory assumes that the donors have much more influence over how aid is ultimately used and allocated. Donors in this scenario care more about outcomes of aid such as development, reform, and democracy promotion than geo-politically strategic goals (Milner et al. 2016). Therefore, donors impose conditions on and shape aid, so it exhibits a less fungible nature. Donors may monitor the allocation of aid and even resort to withhold, or threaten to withhold, aid should they not see desired outcomes (Milner et al. 2016). Thus, the public is seen to gain the most from this relationship, as politicians may struggle to divert the revenue streams to themselves or their allies, and, therefore, more aid flows to public goods provision benefitting society (Mavrotas and Ouattara 2006).

4.2.1 Donor capture and control theory applied to China and The World Bank

With regard to the two donors in this study, there are both empirical and theoretical arguments for Chinese development projects exhibiting "donor capture" tendencies and World Bank projects exhibiting "donor control" tendencies.

In the case of China, their foreign aid practices differ significantly to that of the DAC donors with aid focusing on infrastructure development and loans provided to countries without conditionality attached (Wang and Elliot 2014). While this has been appreciated by recipient countries who feel that loans provided with conditionality have been unnecessarily constraining (Zhao 2014), this has also been met by international critique as several authors have noted that Chinese aid may be easier to exploit by politicians due to China's non-conditionality to aid, non-interference approach, and lack of monitoring and sanctioning of corrupt behavior (Tull 2006; Bräutigam 2010; Dreher et al. 2016). Furthermore, authors have expressed their concern for China's unconditional aid practices and non-interference principles as they could potentially undermine efforts in promoting good governance and accountability for African countries (Wang and Ozanne 2000; Collier 2007; Pehnelt 2007).

Tanzania is an important development partner to China due to Tanzania's vast resource endowment, and its strategic location that functions as a gateway to the rest of Africa via the Indian Ocean. China has therefore been heavily involved in Tanzania for over 40 years, having directed over two billion dollars for a large number of development projects (Brazys et al. 2017). This engagement in Tanzania has caused the country's population to view Chinese engagement as mostly positive (Mwombela 2015). Mwombela (2015) even finds that China is perceived by Tanzanians as having more influence on Tanzania than the USA, UK, India, South Africa, UN or the World Bank.

In the case of the World Bank, Charron (2011) states that multiple multilateral donors have, since 1997, shifted their focus of aid to promote good governance practices and to reduce corruption. Indeed, the World Bank now allocates aid based on their "Country Policy and Institutional Assessment" (CPIA) scores that consider a host of dimensions such as corruption, transparency, and accountability (World Bank 2019). Furthermore, the World Bank has been, since 1995, engaged in a "fight against corruption" in Tanzania (World Bank 1998). This has entailed constructing national integrity systems that directly focus on stemming corruption and, importantly, have included efforts to alter prevailing corruption norms (Leeuw et al. 1999). Compared to China, these efforts speak towards differing foreign aid practices, namely conditionality attached to aid and active interference principles.

Existing perceptions of the World Bank in Tanzania are mixed. The relationship between the World Bank and Tanzania was limited at first, due to Tanzania's socialist leanings, which did not adhere to the World Bank's preferred development approach (Brazys et al. 2017). This somewhat fragile relationship ultimately led to a struggle following a World Bank/IMF structural reprogram project in 1979-80 which in turn led to Tanzanian "capitulation" in 1985 (Holtom 2005). Relationships between the World Bank and Tanzania are today more sustainable, but this does suggest that there may be some underlying tensions which could influence local perceptions of the World Bank negatively. Breen and Gillanders (2015), for instance, found that Africans who had experienced corruption in the past held less positive views of the World Bank.

In light of the reasoning put forth, we argue that Tanzanians' preferences for democracy will be influenced differently depending on which donor is engaged in their local community. Chinese values of democracy differ greatly compared to the World Bank's stance on democracy (see Background). Therefore, Tanzanians' preferences for democracy will be influenced through our first proposed channel, through the transmission of norms, negatively if they reside close to Chinese project sites, and positively if they reside close to World Bank project sites. The transmission of norms will furthermore be facilitated by the positive view that Tanzanians hold of China and might be hampered by the mixed view that Tanzanians hold of the World Bank.

China's non-conditionality approach and non-interference principles towards foreign aid, and the World Bank's conditionality approach to foreign aid and outspoken emphasis to combat corruption, should lead to, on average, higher levels of corruption surrounding Chinese project location compared to World Bank project locations. Through our second channel, this will then influence democratic preferences negatively for local Tanzanians residing close to Chinese project sites, and positively for local Tanzanians residing close to World Bank project sites. This argument is strengthened from recent research by Isaksson and Kotsadam (2018) and Brazys et al. (2017) who find that there is more widespread corruption surrounding Chinese development projects compared to World Bank project sites.

Our hypotheses therefore are:

Hypothesis 1: *Respondents near a Chinese project site which has been implemented will exhibit lower levels of democratic preferences than respondents residing within a Chinese project site which has not yet been implemented.*

Hypothesis 2: *Respondents residing near a World Bank project site which has been implemented will exhibit higher levels of democratic preferences compared to respondents residing near a project site that has not yet been implemented.*

Following hypotheses 1 and 2 our final hypothesis reads:

Hypothesis 3: *Respondents residing near a Chinese project site will exhibit lower levels of democratic preferences compared to respondents residing near a World Bank project site.*

5 Data and Methodology

5.1 Data

In order to establish the relationship between development flows and local preferences for democratic values, we make use of the Afrobarometer Survey: an individual level survey regularly conducted throughout Africa which geo-locates its respondents in clusters. Similar to e.g. Milner et al. (2016), Brazys et al. (2017) and Isaksson and Kotsadam (2018), we then match the surveys to geo-referenced project-level data of Chinese and World Bank development projects over the period 2000-2014, provided by AidData.

By adopting a cross-sectional approach, our study will concentrate on Tanzania. From a methodological point of view, this allows us to bypass the wide range of country specific variables that could affect project allocation. This could, for example, depend upon the political climate in the country or the density of natural disasters. As previously discussed, the involvement of China and the World Bank in Tanzania extend over a similar timeline, omitting potential biases owing to one actor operating for a longer time in the region.

Obtaining project-specific data on Chinese development projects comes with some challenges. Compared to more conventional donors, the Chinese foreign aid practices are less transparent, challenging the traditional donor norms and principles provided by the DAC (De Haan 2011; Kim and Lightfoot 2011). Following this, it has been difficult to evaluate Chinese flows, as the literature has been unable to differentiate between financial flows that are intended as aid and those who are of a more commercial nature (Dreher et al. 2018). We therefore make use of AidData's Geocoded Global Chinese Official Finance, version 1.1.1. which is the first dataset ever to assign geographic coordinates to Chinese development projects, including both aid and non-concessional official financing. The dataset was published in September 2018 and has overcome the issue of non-transparency through AidData's Tracking Underreported Financial Flows (TUFF) methodology. Described further in Strange et al. (2014), this methodology triangulates open source data to create a cohesive collection of official finance data for donors with nontransparent aid policies.

There is some level of risk associated with using open source information as a proxy for officially sourced data. However, the dataset is based upon more than 15,000 different sources and information for each project is, on average, confirmed by three separate sources. Furthermore, we only make use of the information regarding when and where a project was realized, similar to Isaksson and Kotsadam (2018). Consequently, information at risk of being less dependable, like deflators used or the volume of project commitments, will be unlikely to affect our estimations.

As a result of the insufficient reporting on Chinese official flows, TUFF coders assign all Chinese projects with flow-class categories; ODA-like, OOF-like and Vague Official Finance. According to Dreher et al. (2018), Chinese ODA-like flows are mainly associated with foreign policy objectives and beneficiary needs. OOF-like flows are on the other hand mainly driven by economic interests. Following this, we limit our focus to those Chinese development projects which have been classified as ODA-like, as these are the closest in nature to those of traditional donors.

To accommodate for varying levels of precision in location coordinates, as some development projects are implemented on an aggregate level rather than in a smaller specific area, eight precision categories have been developed ranging from exact point locations to country coordinates, which are assigned to projects with unknown locations. Considering the purpose of our paper in determining the democratic effects of Chinese development projects on a local level, we limit our scope to project locations which either correspond to a specific place (precision code 1) or are up to 25 km away from a specific location (precision code 2). We exclude all projects which are coded to locations on a second order administrative division and higher (precision codes 3-8). By doing so, we exclude projects which do not have physical projects sites in the area but might have a widespread effect which could affect our sample. However, we here draw from Berlin et al. (2017) in assuming that this effect is consistently spread throughout our sample.

Imposing these limitations on our dataset reduces our sample from the original 6,190 project locations across Africa (out of which 313 are in Tanzania) providing a sample of 158 Chinese development project locations in Tanzania during the period 2000-2014 which are of suitable levels of precision to be included in our study.

We further use AidData's World Bank Geocoded Research Release Version 1.4.2, released in March 2017, which encompasses all projects approved by the World Bank IBRD and IDA lending lines between 1995-2014. This encompasses 61,243 geocoded locations amounting to $630 billion in commitments. Restricting this data to Tanzania and the period 2000-2014 provides us with 1,035 project locations, out of which 273 are at a sufficient level of precision.

We obtain our outcome and control variables from the Afrobarometer Survey, which is the most prominent research network surveying matters of economy, democracy, governance as well as other national issues in Africa. Their individual level survey applies a random, stratified, clustered and nationally representative strategy targeting eight households per primary sampling unit, of citizens 18-years or older. Furthermore, this dataset follows a double-blind methodology which allows for geo-referencing respondent clusters.[2] We can therefore match Chinese and World Bank projects to Afrobarometer clusters based on spatial proximity. Our analysis draws on four Afrobarometer waves (3-6) conducted in Tanzania comprising 7,298 observations, out of which 2,636 provide coordinates that are at a suitable level of precision for our analysis.

2 See Strandow et al. 2011 for further explanation of the methodology.

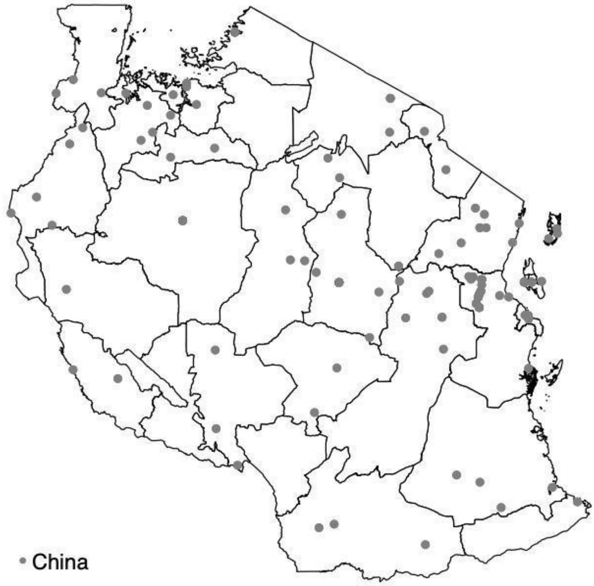

Figure 1: Chinese and World Bank aid projects in Tanzania. [3]

3 Source: authors' own rendering of data from AidData.

5.2 *Estimation strategy*

Our study uses a spatial-temporal strategy, similar to that used by Isaksson and Kotsadam (2018), Knutsen et al. (2017) and Kotsadam and Tolonen (2015), in order to account for potential identification problems, as discussed below. The spatial identification approach furthermore allows for the evaluation of the effect of foreign aid on micro-level outcomes making it a reasonable methodological approach for this study.

As development aid projects are not located randomly throughout a country (see e.g. Dreher et al. 2016; Brazys et al. 2017; Isaksson and Kotsadam 2018), we must assume that particular aspects of communities and locations are more likely to attract aid projects than others, creating an identification problem. Democracy promoting donors might, for example, prefer locations with certain governance characteristics and levels of institutional quality. The systematic distribution of development aid projects could therefore make it difficult to estimate a causal relationship between foreign aid and preferences for democracy. To handle this problem, we make use of binary variables distinguishing between individuals who live within a certain radius of a currently, or previously, active Chinese aid project and those who live within the same distance to a future project which had not yet been implemented at the time of the interview. Through this, we can differentiate between individuals who live in locations which are attractive to project locators and individuals who live in areas which do not display the characteristics that attract aid projects.

In regard to the size of the radius, we utilize different radii. We first make use of a 50km radius (see table 2) but utilize later other radii as robustness checks. We have no strict a priori reasoning for the size of our radii and choose 50km as our primary radius following Isaksson and Kotsadam (2018). To get a more nuanced picture, we find it necessary to utilize different radii and therefore provide in our sensitivity analysis radii of 25km and 75km.

Following this, we measure the distance from each cluster to surrounding aid projects and if at least one currently, or previously, active Chinese aid project is found within our chosen radius, it is captured by the binary variable "active." If a cluster is within the radius of a future, but not yet implemented, aid project it will correspondingly be captured by the binary variable "inactive." Our linear probability model will use the following regression:

$$Y_{ivt} = \beta_1 * active + \beta_2 * inactive + g_t + \lambda X_i + \varepsilon_{ivt}$$

where Y is the democracy outcome measure for an individual i, in a cluster v, for year t which is regressed on our project variables $active$, a dummy for living in the proximity of at least one active, or finished aid project and $inactive$, a dummy for living in the proximity of at least one future project, which has not been implemented yet. g_t is year fixed effects, X_i, a vector of individual level control variables where we control for age, age squared, gender, urban residence, level of education, unemployment and income, and the error term ε_{ivt}.

The clustered nature of our data could give rise to spatial autocorrelation issues, causing our error term to no longer fulfil the assumption of being independently and identically distributed. To account for this in order to achieve correct inference, we make use of geographically clustered errors on the ward level to control for correlation within clusters.

Following the potentially systematic distribution of development aid, the coefficient for "active" (β_1) is in itself not a sufficient estimator of the causal effect of aid on democratic preferences and we are thereby not able to consider it in isolation. In order to do so, we would be required to assume a non-correlative relationship between project location decisions and the characteristics of project locations. This, as discussed further in Isaksson and Kotsadam (2018), is highly improbable, which elicits the use of it in combination with the coefficient for "inactive" (β_2). By introducing this coefficient, our regression accounts for the time invariant location characteristics that attracts aid projects. In doing so, we facilitate the comparison of locations which all display the characteristics required to qualify as a project location. This allows us to estimate the difference between the locations where development projects have been implemented and the locations where implementation has not yet begun. As follows, we estimate a difference-in-difference type estimate ($\beta_1 - \beta_2$) with a treatment group (active) and a control group (inactive). However, this is not to be confused with a true difference-in-difference estimate, which examines the change in the treatment and control group over time. Our estimate solely examines the difference between the treatment and control group at a specific point in time. In order for this estimate to be viable, we need to rely on an underlying assumption that the locations near active and inactive projects have the same unobserved characteristics. Our primary test is whether we can reject the hypothesis that there is no difference between the coefficient for treatment group and control group. We evaluate hypothesis one and two using the following test:

$$H_0 : \beta_1 - \beta_2 = 0$$
$$H_1 : \beta_1 - \beta_2 \neq 0$$

In other words, we test if Chinese projects have an effect on the level of democracy preference in an area, given certain location characteristics and baseline controls. By discarding our null hypothesis, we would thereby be able to conclude that there is a significant difference in democracy preferences between locations where Chinese projects are being, or have been, implemented compared to locations where projects are yet to begin.

In order to test our third hypothesis, we also conduct a test comparing the difference in coefficients for Chinese aid projects and World Banks projects. We thereby evaluate hypothesis three using the following test:

$$H_0 : (\beta_1^C - \beta_2^C) - (\beta_1^{WB} - \beta_2^{WB}) = 0$$

$$H_1 : (\beta_1^C - \beta_2^C) - (\beta_1^{WB} - \beta_2^{WB}) \neq 0$$

5.3 Dependent variables

Following Keulder and Wiese (2005), we argue that a preference for democracy can take two shapes: a) a normative commitment to democracy which requires citizens to show a clear preference for democracy and reject all other non-democratic means of governance, and b) instrumental support conditioned on economic and material performance of the government. Our primary outcome variables are, therefore, a range of different proxies for a preference for democracy obtained from rounds 3-6 of the Afrobarometer Survey, which seeks to determine both a normative unconditional support for democracy and an instrumental conditional support for democracy. Our primary indicator of a normative commitment to democracy is based on Question 30 in round 6 of the survey (Q32 in round 5, Q30 in round 4 and Q37 in round 3):

"Which of these three statements is closest to your own opinion?"

Statement 1: Democracy is preferable to any other kind of government.
Statement 2: In some circumstances, a non-democratic government can be preferable.
Statement 3: For someone like me, it doesn't matter what kind of government we have.

We create a binary indicator that equals "1" if the respondent chose statement 1, and "0" if the respondent chose either statement 2 or 3. We include other outcome variables that showcase a normative commitment to democracy, but which illustrate instead what we choose to refer to as "elements of democracy." We are therefore interested not only in determining a complete preference for democracy, but also discerning whether development flows influence certain characteristics of democracy. For instance, we choose Question 36 in round 6 (Q38 in round 5, Q35 in round 4 and exempt from round 3) of the survey to capture Tanzanians' view on whether the media should report on negative events, reasoning that free media is a pillar of democracy:

"Which of the following statements is closest to your view? Choose statement 1 or Statement 2."

Statement 1: The news media should constantly investigate and report on government mistakes and corruption.
Statement 2: Too much reporting on negative events, like government mistakes and corruption, only harms the country.

Similar to how we created a binary indicator for Question 30, we do this for all of our outcome variables. For instance, in the case of Question 36 we create the indicator "1" if the respondent agrees either very strongly or simply agrees with statement 1 and "0" if the respondent agrees either very strongly or simply agrees with statement 2. Furthermore, we code "agree with neither" as "0" and the responses "don't know," "refused to answer," and "missing" as missing values for all variables.

In terms of an instrumental preference for democracy, our primary outcome variable is Question 41 of the survey (Q43 in round 4 and 5, and Q47 in round 3):

Overall, how satisfied are you with the way democracy works in Tanzania?

Here we create a binary indicator equaling "1" if the respondent indicated any level of satisfaction and "0" if the respondent indicated any level of dissatisfaction.

We utilize four other dependent variables: opinion on whether leaders should be chosen through open elections, the belief that multiple political parties are needed, disapproval of one-party rule, and the perceived extent of democracy in Tanzania. For all outcome variables except support of democracy, we also provide ordinal variables ranging from 0-2 where "2" indicates a strong preference for the democratic option, "1" indicates a preference for this option, and "0" indicates indifference or opposing opinions. See appendix Table A1 for a detailed list of our dependent variables.

We compose an index of our seven dependent variables in order to measure the effect of project aid on an aggregate democracy measure. To this end, we use a Principal Components Analysis (PCA), which is a method of dimensionality reduction that can be used to reduce the number of variables in a dataset while keeping as much information as possible. We use PCA to convert our dependent variables into principal components: linearly uncorrelated factors explaining the variance within our data. By default, the number of components are the same as the number of variables, and the first component is always assigned the largest possible variance in the data set. Our first principal component only provides an explanatory value of 22.61%, which does not explain enough of the total variance to be a suitable index. Following the Kaiser Rule (Kaiser 1974), we retain the components with an Eigenvalue above one ($\lambda > 1$), which implies that the component explains more of the variance in our data than would a single variable . This leaves us with a set of three principal components explaining 57.04% of the total variance (22.61%, 20.07%, and 14.36%, respectively) on which we base our index.

Following Krishnan (2010), we develop a Non-Standardized Index (NSI) based on the component scores assigned to each individual by the principal components. The component scores are then multiplied with the corresponding component factor as seen below. In this way, each score is assigned a proportionate weight.

$$NSI = Factor_n * Component\ score_n$$

To facilitate easier interpretation, as the NSI includes both positive and negative values, we develop a Standardized Index (SI) ranging between 0 and 1 using the formula below:

$$SI = \frac{NSI - Min(NSI)}{Max(NSI) - Min(NSI)}$$

To evaluate the suitability of our PCA, we conduct the Kaiser-Meyer-Olkin (KMO) test for sampling adequacy and retrieve the factor 0.5405. KMO values beneath 0.8 indicate that the sampling is not adequate and that the sum of partial correlation in our data is large in relation to the sum of correlations. Following this, we include our index as a dependent variable in our estimations but will not rely on it in our results.

5.4 Control Variables

Individual characteristics are controlled for by a number of baseline variables throughout our regressions in order to reduce the within-group variance, broadly following the structure of Brazys et al. (2017). However, we refrain from including variables that could be associated with political party affiliation as this could interfere with the accuracy of our estimations.

All regressions control for the age, age squared, gender, employment status and education of the individual. Furthermore, we control for whether the respondent resides in an urban or rural area, as well as if—and how often—their household has gone without cash income over the past year. Further, we control for year fixed effects utilizing binary variables representing each round of the Afrobarometer waves.

5.5 Methodological limitations

In conducting a cross-sectional study with multiple time periods, we assume that the relationship between aid projects and democracy preferences is constant over time, except for the time variance corrected by our year fixed effects. This assumption is rather strong as institutional and societal norms change over time, and democracy preferences are not exempt from this. The time period in which norms change is, however, ambiguous but is generally considered incremental (North 1993).[4] Considering the short time frame of our paper,

4 In the referenced article the author Douglas North does not use the word "norm" as we use it here. North speaks of institutions as consisting of both informal and formal constraints which govern human behavior. We take norms as being one of these

we deem this issue to have modest implications for our overall results.

Further issues with cross-sectional data include the potential of individually fixed effects affecting the results, which our method cannot preclude. For example, in longitudinal studies using panel data, individuals are observed over time, which allows for consideration of these individual effects. The structure of the Afrobarometer survey does not allow for this, as each wave focuses on different geographical areas. On the other hand, in not using longitude data we run a smaller risk of problems with attrition, as a loss of follow-up is non-existent when only conducting the interviews once. This does not , however, avert biases caused by non-responses. We thereby run the risk of examining a sample that is not representative of the population, seeing that the response rate for round 6 is 74.6% and 85.5% for round 5 (no information for the previous rounds exist).

The geographical reach and subsequent noise of our project variables could be a further source of dispute. As mentioned previously, our reasoning in choosing radius does not rely on any compelling statistical claims. In using spatial data, it is feasible that noise or irrelevant information could be prevalent to different extents depending on our choice of radii. Thereby, we conduct robustness checks using different radii ranging between 15 and 100 km (see appendix Table A4.A and B).

6 Results

6.1 Descriptive Statistics

informal constraints that North mentions.

Table 1: Descriptive statistics for baseline sample

Variable	Mean	Std. Dev.	Min	Max	Obs
Dependent variables					
Normative commitment to democracy					
Complete preference for democracy					
Support democracy	0.82	0.39	0	1	2,003
Elements of democracy					
Elected leaders	0.84	0.37	0	1	2,602
Several political parties	0.64	0.48	0	1	2,590
Reject one-party rule	0.69	0.46	0	1	2,595
Media checks government	0.74	0.44	0	1	2,181
Instrumental support for democracy					
Extent democracy	0.80	0.40	0	1	2,022
Satisfaction democracy	0.78	0.41	0	1	2,075
Project variables					
Active 25 km	0.38	0.49	0	1	2,636
Inactive 25 km	0.06	0.24	0	1	2,636
Active 50 km	0.54	0.50	0	1	2,636
Inactive 50 km	0.10	0.30	0	1	2,636
WB Active 25 km	0.52	0.50	0	1	2,636
WB Inactive 25 km	0.14	0.34	0	1	2,636
WB Active 50 km	0.76	0.43	0	1	2,636
WB Inactive 50 km	0.21	0.41	0	1	2,636
Control variables					
Age	38.37	14.21	18	99	2,636
Age2	1673.70	1297.49	324	9801	2,636
Female	0.50	0.50	0	1	2,636
Urban	0.28	0.45	0	1	2,636
Unemployed	0.47	0.50	0	1	2,636
Education:	2.98	0.96	1	5	2,636
1 No Formal Schooling					260
2 Some Schooling					337
3 Primary School					1,364
4 Secondary School					549
5 Post-Secondary School					126
Income:	1.94	1.17	0	4	2,636
0 Without Income: Never					450
1 Without Income: Once or Twice					425
2 Without Income: Several Times					699
3 Without Income: Many Times					969
4 Without Income: Always					99

In Table 1 we find descriptive statistics for our baseline regression.[5] We find that the mean of our binary dependent variables range between 0.69 ($SD = 0.48$) and 0.84 (

5 A more in-depth description of our variables can be found in Appendix I.

$SD = 0.37$), implying that the democratic values are rather high. However, these values are accompanied with high standard deviations, a pattern prevalent among most of our variables. We find a pattern among our project variables, which indicates that an individual is much more likely to live near an active aid project, whether that be a Chinese or a World Bank project, than near a future project.

The sample as a whole consists of equally many men and women ($M = 0.50, SD = 0.50$), with a relatively high age on average ($M = 38.37, SD = 14.21$): a result of the survey only considering individuals at the age of 18 or older. The unemployment rate in our sample could be of interest as it is fairly high ($M = 0.47, SD = 0.50$) but so is also the corresponding standard deviation.

6.2 Chinese and World Bank aid and local preferences for democracy

Table 2 showcases our results for our baseline OLS regressions regarding preferences for democracy surrounding (<50km) Chinese and World Bank aid projects. As mentioned previously, we utilize several proxies for democratic preferences. Columns one to five showcase what we refer to as a normative commitment to democracy, which requires individuals to not only prefer democracy over other governments but to also reject non-democratic governments or elements thereof. Columns six and seven showcase an instrumental support for democracy conditioned on the performance, or perceived performance, of the regime. Finally, column eight displays our index, which is an aggregate democracy measure. We include year fixed effects and baseline control variables in all regressions.

Table 2.A: Chinese aid and local preferences for democracy

Table 2.A: Chinese aid and local preferences for democracy

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks govern.	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Active 50 km	-0.044*	-0.017	-0.029	-0.055**	0.021	-0.042*	-0.035	-0.017
	(0.025)	(0.019)	(0.026)	(0.025)	(0.026)	(0.023)	(0.021)	(0.012)
Inactive 50 km	-0.013	-0.006	-0.023	0.047	-0.009	-0.004	-0.002	-0.015
	(0.045)	(0.030)	(0.042)	(0.044)	(0.056)	(0.038)	(0.044)	(0.031)
DiD type estimate	-0.031	-0.010	-0.006	-0.102	0.031	-0.039	-0.033	-0.002
F-test: active-inactive=0	0.308	0.072	0.014	3.795	0.218	0.633	0.390	0.004
p-value, F-test	0.579	0.789	0.904	0.052	0.641	0.427	0.533	0.947
R-squared	0.047	0.012	0.058	0.119	0.031	0.032	0.028	0.031
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,602	2,590	2,595	2,181	2,022	2,075	1,741

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimates are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Table 2.B: World Bank aid and local preferences for democracy

Table 2.B: World Bank aid and local preferences for democracy

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks govern.	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Active 50 km	-0.062**	0.008	-0.001	-0.036	-0.005	-0.012	-0.044*	-0.022
	(0.027)	(0.018)	(0.027)	(0.024)	(0.028)	(0.025)	(0.025)	(0.014)
Inactive 50 km	-0.003	-0.000	-0.029	0.057	0.003	0.014	-0.033	-0.018
	(0.042)	(0.032)	(0.045)	(0.039)	(0.046)	(0.031)	(0.041)	(0.028)
DiD type estimate	-0.058	0.008	0.029	-0.093	-0.008	-0.026	-0.012	-0.004
F-test: active-inactive=0	1.107	0.041	0.236	3.662	0.019	0.336	0.048	0.012
p-value, F-test	0.294	0.839	0.628	0.057	0.891	0.562	0.827	0.913
R-squared	0.048	0.011	0.057	0.117	0.031	0.030	0.029	0.031
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,602	2,590	2,595	2,181	2,022	2,075	1,741

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimates are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

By looking at the coefficients on "active," we can see that on nearly all regressions for Chinese and World Bank projects (except columns 5 and 2 respectively) we observe the preferences for democracy, elements of democracy, instrumental support for democracy and index decline for Tanzanians residing within 50km of an active project site compared to Tanzanians who do not.

Table 2.A regressions one, four and six tell us that Tanzanians

who reside within 50km of an active Chinese project site are 4.4 percentage points less likely to state that a democracy is preferable over any other form of government ($p<0.1$), 5.5 percentage points more likely to approve of only one political party standing for election and holding office ($p<0.05$), and 4.2 percentage points more likely to not consider Tanzania a full democracy or a democracy with major problems ($p<0.1$), respectively, compared to Tanzanians who do not reside near an active Chinese project site.

In Table 2.B, regressions one and seven tell us that Tanzanians who reside near an active World Bank project site are 6.2 percentage points less likely to state that a democracy is preferable over any other form of government ($p<0.05$) and 4.4 percentage points less likely to be satisfied with the democracy in Tanzania ($p<0.1$), respectively, compared to Tanzanians who do not reside near an active World Bank project site.

The coefficients on "inactive" show statistically insignificant results and no clear pattern in regard to pre-existing levels of democratic preferences. This does not rule out the possible endogeneity problem, however. There could still be a strong possibility that Chinese and World Bank project locations are located on the basis of other factors relevant for democratic preferences.

As mentioned earlier, we cannot assume that there is zero correlation between the location of Chinese aid projects and pre-existing levels of democratic preferences. We address this possible endogeneity problem regarding the placement of aid projects by comparing the coefficients on "active" and "inactive." The associated difference-in-difference type estimates $(\beta_1 - \beta_2)$ indicate lower levels of democratic preferences for all regressions except in regression five in Table 2.A and regression two and three in Table 2.B. However, the associated F-tests and p-values yield insignificant results, preventing us from drawing any concluding remarks as we cannot reject the null hypothesis that they are significantly different from zero. Only regression four in both tables stands out throughout our regressions. By observing regression four we can conclude that Tanzanians residing near active Chinese and World Bank project sites are 10.2 and 9.3 percentage points, respectively, more likely to approve of only one political party standing for election and holding office compared to Tanzanians who reside near inactive project site locations ($p<0.1$ for both).

Table 3: Comparison of beta coefficients for China and the World Bank

Table 3: Comparison of beta coefficients for China and the World Bank

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks govern.	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
China								
Active 50 km	-0.044*	-0.017	-0.029	-0.055**	0.021	-0.042*	-0.035*	-0.017
	(0.025)	(0.019)	(0.026)	(0.025)	(0.026)	(0.023)	(0.021)	(0.012)
Inactive 50 km	-0.013	-0.006	-0.023	0.047	-0.009	-0.004	-0.002	-0.015
	(0.045)	(0.030)	(0.042)	(0.043)	(0.056)	(0.037)	(0.044)	(0.031)
World Bank								
Active 50 km	-0.062**	0.008	-0.001	-0.036	-0.005	-0.012	-0.044*	-0.022
	(0.027)	(0.018)	(0.027)	(0.024)	(0.028)	(0.025)	(0.024)	(0.014)
Inactive 50 km	-0.003	-0.000	-0.029	0.057	0.003	0.014	-0.033	-0.018
	(0.042)	(0.032)	(0.045)	(0.039)	(0.046)	(0.031)	(0.041)	(0.028)
Beta comparison	0.027	-0.018	-0.035	-0.008	0.039	-0.013	-0.021	0.002
B: p-value	0.683	0.712	0.569	0.886	0.649	0.821	0.750	0.972
R-squared								
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,602	2,590	2,595	2,181	2,022	2,075	1,741

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

In Table 3 we test hypothesis three to see whether World Bank project sites positively influence the democratic preferences surrounding their project sites to a greater extent compared to Chinese projects. The comparison of the beta coefficients does not yield a clear pattern whether or not preferences for democracy are greater surrounding Chinese or World Bank project sites. The p-values furthermore indicate that we cannot reject our null hypothesis that there is no significant difference in terms of Chinese and World Bank projects' impact on local preferences for democracy. Therefore, we find no support for hypothesis three.

To sum up so far, while we do observe lower levels of local democratic preferences for Tanzanians surrounding active Chinese project sites compared to Tanzanians who do not, we observe a similar trend for Tanzanians residing near World Bank project sites. However, the difference-in-difference type estimates are too inconclusive to suggest that Chinese and World Bank project sites actually fuel lower levels of democratic preferences or that there is a significant difference between them. Next, we explore our suggested theoretical channels and then perform a sensitivity analysis with associated robustness checks to determine the stability of our findings.

6.3 Exploring theoretical channels

We proposed two theoretical channels via which the presence of development projects might influence the preference for democratic values. The first channel proposed is that by coming into contact with project workers, a change in norms might occur through the transmission of norms. Our baseline regressions and dependent variables all to some extent already capture whether foreign aid might impact societal norms, especially those we referred to as a normative commitment to democracy. As mentioned, these results were inconclusive, but some coefficient estimates indicate that there is a level of decrease in the preference for democracy surrounding both Chinese and World Bank project sites.

In order to further explore our second channel — whether corruption might influence preferences for democracy negatively — we run OLS regressions on our dependent variables and a dummy variable labeled "bribe," which indicates whether or not Tanzanians have had to pay a bribe in the past in order to obtain a permit. The results can be found in Table 4.

Table 4: Permit bribes on democratic preferences

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks government	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Panel A: All projects								
Bribe permit	-0.101**	-0.047	-0.077*	0.057	-0.026	-0.120***	-0.153***	-0.068**
	(0.039)	(0.039)	(0.042)	(0.040)	(0.038)	(0.046)	(0.048)	(0.026)
R-squared	0.077	0.020	0.094	0.178	0.036	0.061	0.057	0.066
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	814	1,015	1,017	1,012	776	818	827	689
Panel B: Chinese projects								
Bribe permit	-0.114**	-0.063	-0.099*	0.060	-0.015	-0.093*	-0.182***	-0.066**
	(0.060)	(0.045)	(0.051)	(0.043)	(0.042)	(0.055)	(0.057)	(0.031)
R-squared	0.091	0.037	0.107	0.144	0.059	0.063	0.085	0.076
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	498	609	612	608	502	500	505	438
Panel C: World Bank projects								
Bribe permit	-0.096**	-0.043	-0.050	0.073	-0.013	-0.119**	-0.143***	-0.064**
	(0.043)	(0.042)	(0.044)	(0.045)	(0.040)	(0.050)	(0.052)	(0.030)
R-squared	0.077	0.025	0.090	0.170	0.053	0.077	0.051	0.072
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	659	819	821	816	631	665	671	560

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered

standard errors at the ward level.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$
Table 4 indicates that Tanzanians who have had to pay a bribe in the past exhibit lower levels of democratic preferences, in particular lower levels of instrumental support for democracy. Regressions one, three, six and seven in panel A tell us that Tanzanians who have had to pay a bribe in order to obtain a permit are 10.1 percentage points less likely to consider democracy as the most preferable way to govern a country ($p<0.05$), 7.7 percentage points more likely to agree with the statement that multiple parties are unnecessary ($p<0.1$), 12 percentage points more likely to not consider Tanzania a full democracy or a democracy with major problems ($p<0.01$) and 15.3 percentage points less likely to be satisfied with democracy in Tanzania ($p<0.01$), respectively, compared to Tanzanians who have not had to pay a bribe in the past. The index also indicates a decline in democratic preferences for individuals who have had to pay a bribe in the past ($p<0.05$). The comparison between the democratic preferences for Tanzanians who have had to pay a bribe surrounding Chinese and World Bank projects yield similar results.

Considering that Tanzanians who have experienced corruption in the past exhibit lower levels of democratic preferences, it begs the question why we did not observe more significant results in democratic preferences surrounding Chinese project sites. The underlying assumption which was made here was that due to China's non-conditionality approach and lax attitude to combat corruption in contrast with the World Bank's active stance to combat corruption, we should observe higher levels of corruption surrounding Chinese project sites compared to World Bank project sites. This assumption also stemmed from the study by Isaksson and Kotsadam (2018) who found results indicating that local corruption is more widespread surrounding Chinese project sites compared to World Bank project sites, stable across a range of robustness checks.

We therefore run OLS regressions similar to that of Isaksson and Kotsadam (2018), in order to determine whether Chinese aid fuels local corruption surrounding project sites in our sample (see appendix Table A5). While the results do indicate that Chinese aid projects are associated with higher levels of corruption, the difference-in-difference type estimate is not significant, and we can therefore not conclude as Isaksson and Kotsadam (2018) concluded that Chinese aid projects fuel local corruption. In fact, the results point to World Bank projects being associated with higher levels of corruption. The reason for our varying results likely stems from our varying empirical approaches. Isaksson and Kotsadam (2018) employ a cross-country analysis with data from 29 African countries and therefore have a significantly larger sample size that results in potentially higher external validity.

6.4 Sensitivity Analysis

The results of our robustness checks can be found in Table A3.A and B in appendix. We find limited support that Tanzanians surrounding Chinese projects exhibit lower levels of democratic preferences compared to Tanzanians surrounding World Bank projects.

First, we include different cut-off distances of 25km and 75km. In the case of Chinese project sites (Table A3.A appendix), the coefficients on “active” indicate a decrease in democratic preferences for individuals surrounding Chinese project sites, both with a radius of 25km and 75km (several of the coefficients being significant). In the case of World Bank project sites (Table A3.B appendix), we do not observe an equivalent pattern. With a 25km radius we obtain no significant results indicating either lower or higher levels of democratic preferences. Using a radius of 75 km surrounding World Bank project sites, we can observe a decline in support for democracy ($p<0.05$) and a decline in the perceived extent of democracy ($p<0.1$).

Next, we include ordinal variables for all possible dependent variables as mentioned in section 4. We include ordinal dependent variables because they have more information regarding the extent of a preference for democracy. The trade-off being that they are harder to interpret. These results are however inconclusive across the board, for both Chinese and World Bank project sites, and we refrain therefore from making any conclusive statements regarding the outcomes.

The difference-in-difference type estimates for Chinese projects are for the most part negative throughout all regressions in table 3.A in appendix. However, only one regression (column 16) is statistically significant ($p<0.1$). The difference-in-difference type in column 16 estimate indicates that Tanzanians within 75km of an active project site are 8.4 percentage points more likely to believe that Tanzania is not a democracy or a democracy with major problems compared to Tanzanians within 75km of an inactive project site. We can observe two significant results in the case of World Bank project sites in table A3.B in appendix. The difference-in-difference type estimates in column four and twelve indicate that Tanzanians within 75km of an active project site are 11.7 percentage points more likely to believe that leaders should be elected through regular, open and honest elections ($p<0.01$) and for Tanzanians within 25km of an active project site are 15.9 percentage points more likely to believe that the media should constantly investigate on government mistakes ($p<0.05$), respectively, compared to Tanzanians who reside within an inactive project site with the corresponding cut-off distances.

To conclude, the results from our robustness checks indicate to some extent that we should expect lower levels of democratic preferences surrounding active Chinese project sites compared to active World Bank project sites. Some results even point to World Bank project sites positively influencing democratic preferences. However, we still cannot definitely conclude whether Chinese and World Bank project sites influence local democratic preferences or if either does so to a greater extent compared to the other.

7 Disscusion

7.1 *Analysis of results*

We are not able to determine a causal relationship between development projects and preferences for democracy. While certain coefficients indicate lower levels of democratic preferences surrounding project sites, in particular Chinese project sites, the corresponding difference-in-difference type estimates provide no systematic evidence that development projects impact democratic preferences. This section will discuss the implications of these findings and connect them to the existing body of literature concerning the impact of development flows on development outcomes at the micro-level and on the literature of democracy promotion.

Consistent with the findings of Brown (2005), that donors have an important role to play in either promoting or preventing democracy, some point estimates in our study imply that the presence of foreign aid projects in Tanzania affects the democratic preferences of inhabitants to a certain extent. While Brown (2005) maintains that donors may have an exogenous impact on democracy by the use of political conditionality to aid, and that democracy promotion largely remains in the hands of recipient countries, this study however relates the endogenous factor of democracy promotion within the country to international donors. By influencing the democratic preferences for individuals surrounding project sites, it could be considered that donors may both exogenously, and endogenously, impact democracy promotion within a country.

Considering Svensson's (1999) findings that foreign aid impacts economic growth in more democratic countries, this has implications for Tanzania's future use of foreign aid. As Tanzania's government has increasingly begun exhibiting authoritarian tendencies, such as oppressing the opposition, negative consequences could follow in terms of the effective use of foreign aid. Our results further indicate that Tanzanians surrounding both Chinese and World Bank project sites are less likely to reject one party rule, suggesting that the government may have civil support for these actions. Bearing in mind that our results stem from surveys conducted before the latest political developments, it does however shed light on the fact that there seems to exist tendencies amongst Tanzanians to be willing to consider one-party rule. These tendencies could potentially be remnants from Tanzania's one-party state past. These results are in line with what we might expect surrounding Chinese project sites, considering that China is itself a one-party state and that through the transmission of norms, this might extend to Tanzanians who reside near their project sites. We, however, find it surprising that similar results can be observed near World Bank project sites.

China's increased presence as an international donor and close historical ties to Tanzania give weight to our findings. As China continues to develop their foreign aid practices and expand their reach, it is important to consider the implications that this might entail for recipient populations' views of democracy. While many developing countries remain dependent on foreign aid, they should be aware of the unintended consequences that come with aid, considering for instance Isaksson and Kotsadam's (2018) study that finds that corruption is more widespread surrounding Chinese project sites compared to World Bank project sites. However, in light of our study, we cannot conclude that changes in democratic preferences is one of those consequences.

Considering China's involvement in Tanzania, a question worth asking is: to what extent has China's involvement to this day impacted Tanzanians democratic preferences? Since our study only considers aid projects between the years 2000-2014, and surveys carried out during these years, it could be that China has impacted democratic preferences to a larger extent than can be observed here. Data limitations make it a difficult task to evaluate Chinese aid flows over a longer time span but would undoubtedly yield interesting results. This would be worth considering both in terms of long-term democratic promotion and economic growth.

In terms of World Bank project sites, our findings indicate that they do not influence democratic preferences to the same extent as Chinese projects. In light of the World Bank's apolitical stance to foreign aid, it suggests that the World Bank manages to maintain an impartial position in the domestic politics of Tanzania, at least in terms of democratic preferences. The practical implications of this is that while recipient countries may believe that conditions to World Bank aid is constraining as it is, it does not seem to be followed by unintended consequences in regard to changes in democratic preferences.

Following our results, we cannot conclude that our proposed conceptual channels have any explanatory power. However, we can observe that individuals who have had to pay a bribe in the past are associated with lower democratic preferences. Considering that we also observe increased levels of corruption surrounding Chinese, and in particular, World Bank project sites, we find it interesting that we do not observe lower levels of democratic preferences surrounding project sites, especially those of the World Bank. A possible explanation for this could be the World Bank's efforts to combat corruption norms, which might cancel out the effect that corruption has on the democratic preferences for individuals in that particular location. Another explanation for this could be that there may be other channels, or confounding variables, at play which we cannot observe that cancels out the effect corruption experiences have on democratic preferences.

7.2 Contribution

Our paper first of all contributes to the expanding quantitative literature on the impact of Chinese development flows and development outcomes at the micro-level. To our knowledge, ours is the first of its kind to seek to explain the relationship between development flows and the democratic preferences for individuals surrounding development project sites. Recognizing that democracy promotion must stem from within civil society (Ahluwalia and Zegeye 2001), our paper contributes by increasing the understanding of how democratic preferences within civil society may be influenced through the engagement with international donors.

Secondly, this study has implications for long-term economic growth as Svensson (1999) finds that foreign aid positively impacts economic growth in more democratic countries. By examining if and how development actors and foreign aid impacts the preferences for democracy surrounding project sites, governments in recipient countries may gain new insights in what to expect from donors and how their engagement influences democratic preferences for their population. By recognizing that democracy is important for the effective use of development aid, we bridge the gap between the two academic fields of political science and development economics.

7.3 Further research

A limitation to our study is the issue of external validity. By conducting a study focusing on one country, the generalizability is limited. Considering Tanzania's commitments regarding democracy-building, it would be a mistake to assume that democratic preferences in countries with other viewpoints in regard to governance will be affected by aid in the same way. In light of this and our ambiguous results in respect to the causal relationship between local democratic preferences and development aid, future research could carry out a study similar to ours, but which encompasses several countries. This would result in not only a significantly larger sample size, which could yield more conclusive results, but would furthermore increase the external validity of the results.

Furthermore, while we have in this study provided some support for our second proposed channel, that corruption experiences may impact the democratic preferences for individuals, this is arguably worthy of a study in its own right. A study such as this, might be able to shed better light on the determinisms of democratic preferences within civil society and corruption and how they are intertwined.

Finally, we have in this study limited ourselves to study only the effect of ODA and "ODA-like" projects on democratic preferences which could lead to a source of inaccuracy considering that China and the World Bank typically implement projects within different sectors.[6] Furthermore, we limit us to World Bank and Chinese aid, a future study could thereby seek to examine the relationship between democratic preferences and development projects within different projects sectors or between different donors.

8 Conclusion

Considering the latest troubling developments for the future of democracy, and the rise of autocratic China as not only a new global economic power, but as a global development partner, this study investigates whether the presence of Chinese development projects impacts the preferences for democratic values for local Tanzanians differently from how a more traditional donor in the World Bank does.

We present two conceptual channels via which development projects may influence the preferences for democratic values in recipient countries – by means of norm transmission through donor engagement in local communities and through corruption experiences following increased economic activity surrounding project sites. Considering China's non-conditionality approach to foreign aid and lax attitude towards combating corruption, the World Bank's active stance to combat corruption and their differing views of democracy, we hypothesize that Tanzanians' preference for democracy will be negatively influenced through these two channels surrounding Chinese project sites and positively surrounding World Bank project sites.

We are not able to identify a causal relationship between development projects and the preferences for democratic values. Some coefficient estimates however indicate that Tanzanians residing in close proximity to Chinese

6 See appendix table A6.A and B for a detailed list of Chinese and World Bank funded projects.

project sites are associated with lower levels of democratic preferences. The results further indicate that the World Bank does not seem to influence preferences for democracy to a large extent in any direction. This speaks to the World Bank in maintaining an impartial stance in the domestic politics of Tanzania.

Despite finding that corruption experiences impact democratic preferences negatively, the inconclusive nature of our results limit us to draw any conclusions regarding the explanatory power of these channels. Further research is needed in order to shed light on the possible determinisms and mechanisms of development projects' impact on democratic preferences.

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10 Appendices

Appendix I: Variable list

Table A1: Variable list

Variable	Description	Max
Support democracy	Dummy for preferring democracy to any other kind of government	Afrobarometer
Elected leaders	Dummy for preferring open and honest elections of the country's leaders to other methods	Afrobarometer
Several political parties	Dummy for thinking many political parties are needed for the people to have real choices in who governs them	Afrobarometer
Reject one-party rule	Dummy for disapproving of only one political party being allowed to stand for election	Afrobarometer
Media checks government	Dummy for thinking that media should investigate and report on government mistakes and corruption	Afrobarometer
Extent democracy	Dummy for thinking Tanzania is a functional democracy	Afrobarometer

Satisfaction democracy	Dummy for being satisfied with how the democracy works in Tanzania	Afrobarometer
Elected leaders ordinal	Ordinal for preferring open and honest elections of the country's leaders to other methods. 2: Agree very strongly. 1: Agree	Afrobarometer
Several political parties ordinal	Ordinal for thinking many political parties are needed for the people to have real choices in who governs them. 2: Agree very strongly. 1: Agree	Afrobarometer
Reject one-party rule ordinal	Ordinal for disapproving of only one political party being allowed to stand for election. 2: Strongly disapprove. 1: Disapprove.	Afrobarometer
Media checks government ordinal	Ordinal for thinking that media should investigate and report on government mistakes and corruption. 2: Agree very strongly. 1: Agree	Afrobarometer
Extent democracy ordinal	Ordinal for thinking Tanzania is a functional democracy. 2: A full democracy. 1: A democracy, but with minor problems	Afrobarometer
Satisfaction democracy ordinal	Ordinal for being satisfied with how the democracy works in Tanzania. 2: Very satisfied. 1: Fairly satisfied	Afrobarometer
Bribe permit	Dummy for having had to pay a bribe, give a gift or do a favor for a government official in order to obtain a document or permit.	Afrobarometer
Active 25 km	Dummy for living within 25 km of a currently active, or finished, Chinese aid project	AidData
Inactive 25 km	Dummy for living within 25 km of a future, not yet implemented, Chinese aid project	AidData
Active 50 km	Dummy for living within 50 km of a currently active, or finished, Chinese aid project	AidData
Inactive 50 km	Dummy for living within 50 km of a future, not yet implemented, Chinese aid project	AidData
WB Active 25 km	Dummy for living within 25 km of a currently active, or finished, World Bank aid project	AidData
WB Inactive 25 km	Dummy for living within 25 km of a future, not yet implemented, World Bank aid project	AidData
WB Active 50 km	Dummy for living within 50 km of a currently active, or finished, World Bank aid project	AidData
WB Inactive 50 km	Dummy for living within 50 km of a future, not yet implemented, World Bank aid project	AidData
Age	Age of participant	Afrobarometer
Age2	Age of participant to the power of 2	Afrobarometer
Female	Dummy for being female	Afrobarometer
Urban	Dummy for living in an urban area.	Afrobarometer
Unemployed	Dummy for being unemployed.	Afrobarometer
Education	Discrete variable for level of education. 1: No Formal Schooling. 2: Some Schooling. 3: Primary School. 4: Secondary School. 5: Post-Secondary School.	Afrobarometer
Income	Discrete variable for amount of times being without cash income. 1: Once or Twice. 2: Several Times. 3: Many Times. 4: Always.	Afrobarometer

Appendix II: Full Regression Results

In the following tables we present the full regressions for the tables in our results.

Table A2.1.A: Chinese aid and local preferences for democracy

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks government	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Active 50 km	-0.044*	-0.017	-0.029	-0.055**	0.021	-0.042*	-0.035	-0.017
	(0.025)	(0.019)	(0.026)	(0.025)	(0.026)	(0.023)	(0.021)	(0.012)
Inactive 50 km	-0.013	-0.006	-0.023	0.047	-0.009	-0.004	-0.002	-0.015
	(0.045)	(0.030)	(0.042)	(0.043)	(0.056)	(0.038)	(0.044)	(0.031)
age	0.004	0.001	0.002	0.001	0.000	-0.005*	-0.007**	-0.003*
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
age2	-0.000	-0.000	-0.000	-0.000	-0.000	0.000**	0.000***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
female	-0.046***	0.014	-0.080***	-0.133***	-0.044**	0.034**	0.024	0.033***
	(0.016)	(0.015)	(0.017)	(0.018)	(0.017)	(0.017)	(0.017)	(0.009)
urban	-0.004	-0.003	0.071***	0.047*	0.010	-0.033	-0.027	-0.027**
	(0.025)	(0.018)	(0.025)	(0.024)	(0.026)	(0.025)	(0.023)	(0.013)
unemployed	-0.010	-0.012	-0.011	-0.009	0.000	-0.028	-0.052**	-0.015
	(0.0200)	(0.017)	(0.024)	(0.020)	(0.021)	(0.023)	(0.024)	(0.013)
1.education	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2.education	-0.020	0.010	-0.044	-0.041	-0.010	0.041	0.038	0.038
	(0.044)	(0.031)	(0.039)	(0.043)	(0.041)	(0.045)	(0.044)	(0.027)
3.education	0.027	0.015	0.001	0.033	-0.017	0.026	0.041	0.024
	(0.038)	(0.028)	(0.036)	(0.036)	(0.037)	(0.040)	(0.035)	(0.022)
4.education	0.076*	0.034	0.080**	0.130***	0.028	-0.016	0.009	0.0053

Table A2.1.A: Chinese aid and local preferences for democracy

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks government	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Active 50 km	-0.044*	-0.017	-0.029	-0.055**	0.021	-0.042*	-0.035	-0.017
	(0.025)	(0.019)	(0.026)	(0.025)	(0.026)	(0.023)	(0.021)	(0.012)
Inactive 50 km	-0.013	-0.006	-0.023	0.047	-0.009	-0.004	-0.002	-0.015
	(0.045)	(0.030)	(0.042)	(0.043)	(0.056)	(0.038)	(0.044)	(0.031)
age	0.004	0.001	0.002	0.001	0.000	-0.005*	-0.007**	-0.003*
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
age2	-0.000	-0.000	-0.000	-0.000	-0.000	0.000**	0.000***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
female	-0.046***	0.014	-0.080***	-0.133***	-0.044**	0.034**	0.024	0.033***
	(0.016)	(0.015)	(0.017)	(0.018)	(0.017)	(0.017)	(0.017)	(0.009)
urban	-0.004	-0.003	0.071***	0.047*	0.010	-0.033	-0.027	-0.027**
	(0.025)	(0.018)	(0.025)	(0.024)	(0.026)	(0.025)	(0.023)	(0.013)
unemployed	-0.010	-0.012	-0.011	-0.009	0.000	-0.028	-0.052**	-0.015
	(0.0200)	(0.017)	(0.024)	(0.020)	(0.021)	(0.023)	(0.024)	(0.013)
1.education	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2.education	-0.020	0.010	-0.044	-0.041	-0.010	0.041	0.038	0.038
	(0.044)	(0.031)	(0.039)	(0.043)	(0.041)	(0.045)	(0.044)	(0.027)
3.education	0.027	0.015	0.001	0.033	-0.017	0.026	0.041	0.024
	(0.038)	(0.028)	(0.036)	(0.036)	(0.037)	(0.040)	(0.035)	(0.022)
4.education	0.076*	0.034	0.080**	0.130***	0.028	-0.016	0.009	0.0053
F-test								
R2	0.047	0.012	0.058	0.119	0.031	0.032	0.028	0.031
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,602	2,590	2,595	2,181	2,022	2,075	1,741

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Table A2.1.B: World Bank aid and local preferences for democracy

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks government	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Active 50 km	-0.062**	0.008	-0.001	-0.036	-0.005	-0.012	-0.044*	-0.022
	(0.027)	(0.018)	(0.027)	(0.024)	(0.028)	(0.025)	(0.025)	(0.014)
Inactive 50 km	-0.003	-0.000	-0.029	0.057	0.003	0.014	-0.033	-0.018
	(0.042)	(0.032)	(0.045)	(0.039)	(0.046)	(0.031)	(0.041)	(0.028)
age	0.004	0.001	0.002	0.001	0.000	-0.005*	-0.007**	-0.003*
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
age2	-0.000	-0.000	-0.000	-0.000	-0.000	0.000**	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
female	-0.044***	0.014	-0.080***	-0.132***	-0.044**	0.034**	0.026	0.033***
	(0.016)	(0.015)	(0.017)	(0.018)	(0.017)	(0.017)	(0.017)	(0.009)
urban	-0.002	-0.005	0.070***	0.048*	0.011	-0.035	-0.025	-0.026**
	(0.026)	(0.018)	(0.026)	(0.025)	(0.026)	(0.025)	(0.023)	(0.013)
unemployed	-0.013	-0.014	-0.014	-0.014	0.003	-0.032	-0.054**	-0.017
	(0.020)	(0.017)	(0.024)	(0.020)	(0.021)	(0.023)	(0.024)	(0.013)
1.education	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2.education	-0.021	0.010	-0.044	-0.043	-0.010	0.042	0.037	0.038
	(0.044)	(0.031)	(0.039)	(0.044)	(0.041)	(0.046)	(0.044)	(0.027)
3.education	0.026	0.015	0.000	0.031	-0.016	0.026	0.039	0.024
	(0.038)	(0.028)	(0.036)	(0.036)	(0.036)	(0.040)	(0.035)	(0.022)
4.education	0.0733*	0.031	0.076**	0.125***	0.031	-0.021	0.006	0.004

	(0.041)	(0.030)	(0.038)	(0.038)	(0.038)	(0.048)	(0.040)	(0.026)
5.education	0.137***	0.065	0.100*	0.166***	0.043	0.012	0.009	0.027
	(0.046)	(0.041)	(0.051)	(0.046)	(0.052)	(0.057)	(0.054)	(0.030)
0.income	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
1.income	-0.065**	0.021	0.049	0.010	0.028	-0.012	0.004	0.001
	(0.030)	(0.025)	(0.035)	(0.032)	(0.037)	(0.029)	(0.030)	(0.016)
2.income	-0.047*	0.029	0.033	0.017	0.035	-0.007	0.006	0.002
	(0.027)	(0.023)	(0.030)	(0.029)	(0.035)	(0.026)	(0.027)	(0.015)
3.income	-0.087***	-0.003	-0.025	-0.021	0.025	-0.044*	-0.078***	-0.026
	(0.029)	(0.024)	(0.031)	(0.029)	(0.033)	(0.026)	(0.029)	(0.016)
4.income	-0.082	-0.059	-0.087	0.031	0.011	-0.085	-0.101**	-0.060*
	(0.050)	(0.045)	(0.057)	(0.051)	(0.062)	(0.057)	(0.051)	(0.033)
y2005	0.085*	0.098***	-0.071	-0.333***	0	0.125***	0.133***	0
	(0.048)	(0.032)	(0.048)	(0.041)	(.)	(0.039)	(0.045)	(.)
y2008	-0.041	0.034	0.090*	-0.199***	0.145***	0.007	0.027	0.034
	(0.041)	(0.036)	(0.051)	(0.041)	(0.035)	(0.032)	(0.043)	(0.025)
y2012	0.113***	0.035	0.158***	0.045	0.137***	-0.070**	0.032	-0.001
	(0.026)	(0.022)	(0.029)	(0.028)	(0.028)	(0.030)	(0.029)	(0.014)
y2014	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
_cons	0.768***	0.743***	0.603***	0.816***	0.697***	0.908***	0.932***	0.788***
	(0.080)	(0.072)	(0.096)	(0.084)	(0.082)	(0.087)	(0.076)	(0.047)
DiD type estimate	-0.058	0.008	0.029	-0.093	-0.008	-0.026	-0.012	-0.004
F-test	1.107	0.041	0.236	3.662	0.019	0.336	0.048	0.012
p-value	0.294	0.839	0.628	0.057	0.891	0.562	0.827	0.913
, F-test								
R2	0.048	0.011	0.057	0.117	0.031	0.030	0.029	0.031
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,602	2,590	2,595	2,181	2,022	2,075	1,741

Standard errors in parentheses

Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Table A2.2: Permit bribes on democratic preferences

	(1) Support democracy	(2) Elected leaders	(3) Several political parties	(4) Reject one-party rule	(5) Media checks government	(6) Extent democracy	(7) Satisfaction democracy	(8) Index
Bribe permit	-0.101**	-0.047	-0.0771*	0.057	-0.026	-0.120***	-0.153***	-0.068**
	(0.039)	(0.039)	(0.042)	(0.040)	(0.038)	(0.046)	(0.048)	(0.026)
age	0.005	-0.000	-0.004	0.002	0.011*	0.002	-0.008	-0.001
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)
age2	-0.000	-0.000	0.000	-0.000	-0.000*	-0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
female	-0.000	0.023	-0.081***	-0.138***	-0.004	0.005	0.056**	0.035**
	(0.024)	(0.021)	(0.027)	(0.030)	(0.027)	(0.029)	(0.028)	(0.014)
urban	-0.045	-0.043	0.030	0.011	-0.001	-0.034	-0.033	-0.033
	(0.032)	(0.031)	(0.033)	(0.037)	(0.039)	(0.041)	(0.036)	(0.022)
unemployed	0.011	-0.011	-0.0022	-0.039	-0.030	-0.041	-0.057	-0.029
	(0.028)	(0.026)	(0.035)	(0.028)	(0.034)	(0.033)	(0.035)	(0.019)
1.education	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2.education	0.077	-0.049	-0.104	-0.052	-0.066	0.042	0.107	0.030
	(0.076)	(0.050)	(0.064)	(0.070)	(0.065)	(0.075)	(0.073)	(0.046)
3.education	0.115*	-0.028	-0.022	0.046	-0.028	0.032	0.094	0.036
	(0.063)	(0.040)	(0.060)	(0.058)	(0.060)	(0.063)	(0.062)	(0.039)
4.education	0.158**	-0.028	0.027	0.112*	-0.001	-0.024	0.068	-0.000
	(0.067)	(0.047)	(0.063)	(0.066)	(0.064)	(0.073)	(0.071)	(0.043)
5.education	0.206***	0.033	0.028	0.156**	0.089	0.050	0.040	0.045
	(0.077	(0.05	(0.07	(0.07	(0.075)	(0.083	(0.085	(0.05

	)	8)	8)	8)		)	)	0)
0.income	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
1.income	-0.098**	0.004	0.041	0.019	0.073	-0.011	0.011	0.016
	(0.040)	(0.031)	(0.047)	(0.045)	(0.050)	(0.042)	(0.040)	(0.021)
2.income	-0.082**	-0.021	0.025	0.033	0.102**	0.035	-0.014	0.029
	(0.038)	(0.034)	(0.041)	(0.040)	(0.047)	(0.039)	(0.042)	(0.024)
3.income	-0.019	-0.034	-0.035	-0.052	0.028	-0.016	-0.114**	-0.009
	(0.038)	(0.031)	(0.047)	(0.044)	(0.049)	(0.042)	(0.044)	(0.023)
4.income	-0.083	-0.106	-0.125	0.0044	-0.058	-0.051	-0.161*	-0.079
	(0.070)	(0.073)	(0.096)	(0.089)	(0.088)	(0.086)	(0.085)	(0.066)
y2005	-0.009	0.022	-0.231***	-0.325***	0	0.135***	0.111**	0
	(0.045)	(0.036)	(0.051)	(0.047)	(.)	(0.051)	(0.055)	(.)
y2008	-0.150***	-0.023	-0.014	-0.176***	0.118***	0.055	0.0032	0.017
	(0.041)	(0.037)	(0.044)	(0.042)	(0.042)	(0.049)	(0.049)	(0.025)
y2012	0.035	-0.038	0.066	0.042	0.058	-0.060	0.051	-0.023
	(0.035)	(0.036)	(0.040)	(0.037)	(0.042)	(0.054)	(0.050)	(0.024)
y2014	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
_cons	0.703***	0.948***	0.886***	0.812***	0.527***	0.761***	0.879***	0.757***
	(0.122)	(0.121)	(0.144)	(0.151)	(0.147)	(0.162)	(0.137)	(0.087)
R-squared	0.077	0.020	0.094	0.178	0.036	0.061	0.057	0.066
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	814	1,015	1,017	1,012	776	818	827	689

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Appendix III: Sensitivity Analysis

.A: Robustness checks for Chinese aid and local democratic preferences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Support democracy		Elected leaders		Elect. leaders ordinal	Several political parties		Sev. pol. parties ordinal	Reject one-party rule		Reject one-party rule ordinal	Media checks government		Media checks gover. ordinal	Extent democracy		Extent demo. ordinal	Satisfaction democracy		Satisfaction demo. ordinal
	0.001		-0.006			-0.033			-0.056**			0.014			-0.057**			-0.052**		
	(0.026)		(0.019)			(0.026)			(0.026)			(0.024)			(0.024)			(0.022)		
	-0.030		-0.012			-0.045			-0.040			-0.029			0.007			-0.011		
	(0.049)		(0.033)			(0.057)			(0.050)			(0.056)			(0.046)			(0.052)		
					-0.025			-0.047			-0.063			0.043			-0.056			-0.029
					(0.042)			(0.050)			(0.047)			(0.052)			(0.042)			(0.037)
Inactive 50 km					-0.004			-0.087			0.038			-0.0189			-0.048			0.004
					(0.071)			(0.086)			(0.076)			(0.111)			(0.081)			(0.084)
Active 75 km		-0.060**		-0.029			-0.050*			-0.061**			0.042			-0.073***			-0.039*	
		(0.025)		(0.020)			(0.027)			(0.027)			(0.029)			(0.022)			(0.022)	
Inactive 75 km		-0.002		-0.013			0.005			0.015			-0.025			0.012			0.021	
		(0.042)		(0.030)			(0.042)			(0.041)			(0.053)			(0.035)			(0.041)	
DiD type estimate	0.031	-0.058	0.007	-0.016	-0.020	0.012	-0.054	0.039	-0.016	-0.076	-0.100	0.043	0.066	0.061	-0.064	-0.084	-0.008	-0.041	-0.060	-0.033
F-test: .	0.257	1.087	0.026	0.156	0.053	0.038	1.056	0.156	0.072	2.194	1.121	0.427	0.984	0.218	1.352	3.442	0.006	0.457	1.514	0.109
p-value, F-test	0.613	0.298	0.872	0.693	0.819	0.847	0.305	0.693	0.788	0.140	0.291	0.514	0.322	0.641	0.246	0.065	0.937	0.500	0.220	0.742
R2	0.044	0.049	0.011	0.013	0.038	0.058	0.059	0.075	0.120	0.119	0.113	0.031	0.032	0.031	0.034	0.036	0.059	0.030	0.028	0.057
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,003	2,602	2,602	2,602	2,590	2,590	2,590	2,595	2,595	2,595	2,181	2,181	2,181	2,022	2,022	2,022	2,075	2,075	2,075

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered

standard errors at the ward level. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Table A3.B: Robustness checks for World Bank aid and local democratic preferences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Support democracy		Elected leaders		Elect. leaders ordinal	Several political parties		Several pol. parties ordinal	Reject one-party rule		Reject one-party rule ordinal	Media checks government		Media checks gover. ordinal	Extent democracy		Extent demo. ordinal	Satisfaction on democracy		Satisfa-ction demo. ordinal
Active 25 km	-0.011		-0.019			-0.023			-0.038			0.030			-0.022			-0.025		
	(0.026)		(0.018)			(0.025)			(0.025)			(0.025)			(0.023)			(0.022)		
Inactive 25 km	-0.087**		-0.021			-0.014			-0.023			-0.129**			-0.004			-0.022		
	(0.040)		(0.030)			(0.041)			(0.039)			(0.050)			(0.034)			(0.041)		
Active 50 km					0.018			0.013			-0.068			-0.010			-0.040			-0.052
					(0.047)			(0.059)			(0.051)			(0.056)			(0.050)			(0.041)
Inactive 50 km					0.041			-0.065			0.090			0.007			0.037			-0.009
					(0.065)			(0.091)			(0.071)			(0.092)			(0.075)			(0.076)
Active 75 km		-0.065**		0.016			-0.009			-0.010			0.004			-0.050*			-0.039	
		(0.033)		(0.022)			(0.033)			(0.028)			(0.037)			(0.028)			(0.031)	
Inactive 75 km		-0.001		-0.101***			0.033			0.037			-0.035			0.012			-0.069	
		(0.054)		(0.029)			(0.061)			(0.041)			(0.058)			(0.042)			(0.053)	
DiD type estimate	0.076	-0.064	0.002	0.117	-0.023	-0.009	-0.042	0.078	-0.014	-0.047	-0.157	0.159	0.038	-0.017	-0.018	-0.062	-0.077	-0.004	0.029	-0.043
F-test:	1.928	0.754	0.003	7.717	0.062	0.029	0.297	0.398	0.075	0.667	2.685	6.266	0.218	0.019	0.153	1.080	0.574	0.005	0.165	0.208
p-value, F-test	0.166	0.386	0.954	0.006	0.804	0.865	0.586	0.529	0.784	0.415	0.102	0.013	0.641	0.891	0.696	0.300	0.449	0.944	0.685	0.648
R2	0.048	0.047	0.012	0.013	0.039	0.057	0.057	0.074	0.118	0.116	0.113	0.034	0.031	0.031	0.030	0.031	0.058	0.028	0.029	0.058
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,003	2,003	2,602	2,602	2,602	2,590	2,590	2,590	2,595	2,595	2,595	2,181	2,181	2,181	2,022	2,022	2,022	2,075	2,075	2,075

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban residence, unemployment, income and education level. All regressions control for year fixed effects and clustered standard errors at the ward level. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Appendix IV: Robustness check 15-100 km

Table A4.A: Robustness check radius China

	(1) Support democracy	(2) Support democracy	(3) Support democracy	(4) Support democracy	(5) Support democracy
Active 15 km	-0.042				
	(0.038)				
Inactive 15 km	0.010				
	(0.071)				
Active 25 km		0.001			
		(0.028)			
Inactive 25 km		-0.030			
		(0.049)			
Active 50 km			-0.044*		
			(0.025)		
Inactive 50 km			-0.013		
			(0.045)		
Active 75 km				-0.060**	
				(0.025)	
Inactive 75 km				-0.002	
				(0.042)	
Active 100 km					-0.077***
					(0.025)
Inactive 100 km					0.034
					(0.041)
DiD type estimate	-0.052	0.031	-0.031	-0.058	-0.110
F-test: active-inactive=0	0.353	0.257	0.308	1.087	4.337
p-value, F-test	0.553	0.613	0.579	0.298	0.038
R-squared	0.046	0.044	0.047	0.049	0.051
Year FE	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES
Observations	2,003	2,003	2,003	2,003	2,003

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban and unemployment. All regressions control for year fixed effects and clustered standard errors. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Table A4.B: Robustness check radius World Bank

	(1) Support democracy	(2) Support democracy	(3) Support democracy	(4) Support democracy	(5) Support democracy
Active 15 km	-0.001				
	(0.027)				
Inactive 15 km	-0.111***				
	(0.043)				
Active 25 km		-0.011			
		(0.026)			
Inactive 25 km		-0.087**			
		(0.040)			
Active 50 km			-0.062**		
			(0.027)		
Inactive 50 km			-0.003		
			(0.042)		
Active 75 km				-0.065**	
				(0.033)	
Inactive 75 km				-0.001	
				(0.054)	
Active 100 km					-0.095***
					(0.036)
Inactive 100 km					0.049
					(0.080)
DiD type estimate	0.110	0.076	-0.058	-0.064	-0.144
F-test: active-inactive=0	3.784	1.928	1.107	0.754	2.063
p-value, F-test	0.053	0.166	0.294	0.386	0.152
R-squared	0.048	0.048	0.048	0.047	0.047
Year FE	YES	YES	YES	YES	YES
Baseline controls	YES	YES	YES	YES	YES
Observations	2,003	2,003	2,003	2,003	2,003

Standard errors in parentheses. Baseline controls include age, age-squared, female, urban and unemployment. All regressions control for year fixed effects and clustered standard errors. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Appendix V: Bribe and aid projects

Table A5: Development aid projects and corruption

	(1) Bribe China	(2) Bribe World Bank
Active 50 km	0.059*	
	(0.031)	
Inactive 50 km	0.023	
	(0.032)	
Active 50 km		0.056**
		(0.025)
Inactive 50 km		-0.037
		(0.034)
DiD type estimate	0.036	0.093
F-test: active-inactive=0	0.555	3.385
p-value, F-test	0.457	0.067
R-squared	0.061	0.059
Year FE	YES	YES
Baseline controls	YES	YES
Observations	1,026	1,026

Standard errors in parentheses
Baseline controls include age, age-squared, female, urban and unemployment. All regressions control for year fixed effects and clustered standard errors. DiD type estimations are based on the coefficients of active and inactive, which also are the basis for the associated F-test and the following p-value.
* p<0.1, ** p<0.05, *** p<0.01

Appendix VI: Project Sectors

Table A6.A: Project sectors Chinese aid projects

Codes		Freq.
110	Education	7
120	Health	34
140	Water Supply and Sanitation	36
150	Government and Civil Society	2
160	Other Social infrastructure and services	4
210	Transport and Storage	2
220	Communications	64
310	Agriculture, Forestry and Fishing	2
320	Industry, Mining, Construction	1
420	Women in Development	2
430	Other Multisector	1
530	Non-food commodity assistance	1
600	Action Relating to Debt	1
700	Emergency Response	1

Table A6.B: Project sectors World Bank aid projects

Codes		Freq.
151|310	Agriculture, forestry, fishing|Government and civil society, general	12
151|311	Agriculture|Government and civil society, general	13
230	Energy generation and supply	42
151|312|160	Forestry|Other social infrastructure and services|Government and civil society, general	4
312|311|310|160|140	Forestry|Water supply and sanitation|Agriculture|Other social infrastructure and services|Agriculture, forestry, fishing	1
151	Government and civil society, general	19
151|210	Government and civil society, general|Transport and storage	37
322|230	Mineral resources and mining|Energy generation and supply	5
151|230|322|160	Mineral resources and mining|Other social infrastructure and services|Government and civil society, general|Energy generation and supply	10
160	Other social infrastructure and services	10
151|160	Other social infrastructure and services|Government and civil society, general	2
151|114|240|113	Post-secondary education|Banking and financial services|Government and civil society, general|Secondary education	1
151|114	Post-secondary education|Government and civil society, general	2
151|114|113|112	Post-secondary education|Secondary education|Government and civil society, general|Basic education	1
210	Transport and storage	41
151|210	Transport and storage|Government and civil society, general	9
151|410|140	Water supply and sanitation|General environmental protection|Government and civil society, general	4
151|140	Water supply and sanitation|Government and civil society, general	7
151|230|140	Water supply and sanitation|Government and civil society, general|Energy generation and supply	6
151|410|140|210	Water supply and sanitation|Government and civil society, general|General environmental protection|Transport and storage	20
151|140|210	Water supply and sanitation|Government and civil society, general|Transport and storage	5
311|160|140	Water supply and sanitation|Other social infrastructure and services|Agriculture	3
151|114|311|160|140	Water supply and sanitation|Post-secondary education|Agriculture|Government and civil society, general|Other social infrastructure and services	7
151|210|140	Water supply and sanitation|Transport and storage|Government and civil society, general	12

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