Financing for Sustainable Development: Emerging Issues and Actors

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Preamble

1. Introduction

The Sustainable Development Goals (SDGs) succeeded the Millennium Development Goals in 2015 as guiding political framework that was meant to coordinate and focus global efforts in development cooperation. What is more, the SDGs set out to integrate the formerly separate agenda on environmental sustainability into the framework of human development. While their predecessor consisted of only 8 goals, the SDGs are a list of 17 interlinked goals that are again subdivided into 169 concrete targets. From their inception, the SDGs were criticized for doing too little by trying to do too much (The Economist 2015). Whether this prediction will turn out correct is still to be seen. In any case, the lengthiness of the SDGs is a testimony of the added levels of complexity that development cooperation is faced with today. While development finance emerged in the 1950s under a relatively simple narrative of Western industrialized countries aiding the development of the Global South, it has undergone many changes since – both geopolitically and conceptually.

The motivation for this thesis is to analyze which changes development finance is currently undergoing and how this affects the way the global community has to approach the financing of sustainable development.

In particular, this thesis focuses on three important changes that have added complexity to development finance under the SDGs. These are:

1. **Diversity of the donor landscape**
2. **Multitude of the policy objectives**
3. **Riskiness of the operating environment**

While this list is by no means exclusive, it offers insight on how donors and recipients of today's development finance have to deal with changes at multiple levels simultaneously.

The **diversity of the donor landscape** has increased due to the emergence of new bilateral and multilateral donors. Since the emergence of foreign aid in the 1950s, it had been dominated by a club of rich Western democratic countries (Morgenthau 1962, Lumsdaine 1993). These are organized in the

OECD Development Assistance Committee (DAC) and have – at least on paper – strived to coordinate their efforts as documented, for instance, in the Paris Declaration of Aid Effectiveness from 2005. However, in the last two decades, donors outside the OECD DAC have increasingly gained importance (Walz & Ramachandran 2010, Dreher et al. 2011). Most prominently, China has emerged as a major bilateral donor that operates outside the OECD DAC and outside the Western democratic norms associated with it (Dreher et al. 2022). The emergence of these new actors has turned the development arena into "sites of contested cooperation" (Chaturvedi et al. 2021). While more competition among the supply-side of development financing bears the potential to raise efficiency, the multitude of actors with competing interests and policy objectives risks efficiency losses from duplication and incoherence of the efforts.

The **multitude of the policy objectives** has increased due to both the attempt to integrate environmental sustainability into the agenda of human development and the recognition that development is multidimensional. The long list of the SDGs makes it apparent that the narratives and policy priorities in development cooperation have not only changed, but widened over time. While absolute income was for a long time the main indicator for poverty, aspects such as the distribution of income or the access to basic services (e.g., energy or water and sanitation) are increasingly seen as integral. In addition, environmental pressures from climate change, environmental degradation and the loss of biodiversity are inseparably linked to the fight against poverty. This multitude of political objectives runs the risk of creating contradictions between individual targets. Consequently, policy-makers are forced to assess the benefit of interventions based on multiple scales in order to identify synergies and trade-offs between separate policy objectives.

Finally, the **riskiness of the operating environment** has grown due to the increasing intersection of poverty with other risk factors. It is estimated that by 2030, 80 percent of people living below the poverty line will reside in fragile states (OECD 2018). According to this estimate, the Democratic Republic of Congo and Nigeria will together account for 40 percent of the global poor (Kim 2019). What is more, climate change will increase both the intensity and frequency of environmental risks associated with natural disasters and resource scarcities. This added risk will mostly be borne by low-income countries. Thus, development finance is increasingly faced with risky environments where success does not only depend on the internal logic of an intervention, but also the operating environment. Current estimates show that development projects in fragile states are 8 percent less likely to be successful than comparable projects in non-fragile states (Caselli & Presbitero 2020). Consequently, policy-makers need to find new aid strategies specific to risky environments as recommended by the Commission on State Fragility, Growth and Development (2018). These must be resilient to potential negative shocks from the operating environment.

These changes that development finance is currently facing cannot be dealt with conclusively in the context of a thesis. Instead, each chapter of this thesis provides an in-depth analysis of a research question that arises from one of the described changes. The first chapter investigates reasons related to the increasing multitude of the donor landscape, the second chapter illustrates the difficulty of satisfying multiple policy objectives simultaneously, and finally, the third chapter sheds light on the consequences of the increasing riskiness of the operating environment.

The remainder of the Preamble is structured as follows. First, I summarize the articles of the thesis in section 2 and outline how they relate to the conceptual framework introduced above. Next, I describe the methodological approach in section 3, and finally, I conclude in section 4.

2. Summary of chapters

The first chapter **Democracy and Aid Donorship**, co-authored with Andreas Fuchs, addresses the emerging diversity of the donor landscape. In particular, this chapter examines the question in how far democratic countries differ from autocratic countries when deciding to start an aid initiative or to institutionalize aid giving. Historically, the donor landscape had been dominated by Western rich democracies organized in the OECD DAC. Based on this observation, aid giving has long been associated with democratic institutions. This can be rationalized, for instance, by the assumption that a higher level of domestic redistribution observed in democracies translates into preferences for global redistribution.

However, the emergence of new actors such as China, that neither possess democratic institutions nor belong to the club of DAC donors are challenging this notion. What is more, not all emerging donors are of the high income bracket. For instance, the emerging donors China, Brazil and South Africa share a recent history as recipients of aid (Asmus et al. 2020). How is it possible that these countries chose to share a part of their government budget in the form of foreign aid rather than investing it domestically?

The chapter proposes a simple, yet novel explanation: Whether or not democratic institutions make it more likely to start an aid initiative depends on the income level. Democratic institutions make it more likely that preferences of the citizens translate into actual policies. While citizens of richer countries display some preference for global poverty alleviation, citizens of poorer countries prefer the government budget to be spend on the development at home. In contrast, political elites have many incentives for aid giving out of self-interest (Kuziemko & Werker 2006, Dreher et al. 2009, Bearce & Tirone 2010, Fleck & Kilby 2010, Bermeo 2011, Kersting & Kilby 2014, Dietrich et al. 2018, Dreher et al. 2019). This explains why political leaders of poor autocratic states might still opt into aid giving against the interest of their population.

To test this theory empirically, we collect a new dataset on global aid donorship covering 114 countries for the years 1945-2015. It contains information on the year of a country's first outgoing aid project, the name and year of its current institution responsible for aid provision, the name and year of establishment of its first institution responsible for aid provision, and the name and year of its first aid legislation.

For identification, we rely on regional waves of democratization as exogenous source of variation in the democracy measure. The empirical results are in line with our hypothesis. While democratic institutions make it more likely that richer countries start an aid initiative, for poorer countries the effect is reversed. This finding is relevant given the rising influence of non-democratic donors outside the OECD DAC. Our findings imply that the motives of autocratic donors lean heavier on elite interests than it has been the case for democratic donors. This has implications for the type of aid and the way of coordination that can reasonably be expected from them. The paper has been published in the *American Economic Journal: Economic Policy* (Budjan & Fuchs 2021).

The second chapter **Move on up – Electrification and Internal Migration in Nigeria** examines an example of public investments where direct and indirect effects create a conflict between multiple policy objectives. In particular, it analyzes how rural infrastructure development creates unintended consequences for internal migration. Given the increasing need to assess development programs holistically against a number of policy objectives this study seeks to understand the complex interaction between direct and indirect policy outcomes. Development economics as a discipline has made major progress in deriving credible estimates for the direct intended effects of interventions – mainly due to the increased use of randomized trials. However, scholars are now increasingly aware that in order to understand the effect of an intervention fully, it is important to assess also its indirect, long-run or general equilibrium effects. This is where conflict between different policy objectives often arises.

In this chapter, I use the example of the expansion of the electric transmission grid in Nigeria to understand the effects of rural electrification on population dynamics. Investments in rural infrastructure are an important component of a development strategy that seeks to harmonize economic activity across space. This is important in light of the large productivity gap between rural and urban centers in many developing countries. While an increase of local productivity is one of the primary objectives of rural infrastructure construction, policy-makers often hope for indirect effects of reduced rural depopulation and urban migration pressure by offering valid options at home. Theoretically, investments in rural electricity infrastructure should lead to both a direct effect on productivity and an indirect effect on reduced net out-migration. However, in most developing countries migration preferences might exceed observed migration due to existing credit constrains. Consequently, raising productivity and raising income might result in an increased net out-migration when credit constrains are lifted.

To test this question empirically, I use a geo-referenced household panel from Nigeria covering the years 2009 – 2016 and information in the recent expansion of the electric transmission grid. In order to account for endogenous allocation of the grid, I use methods developed in the transport infrastructure literature. In particular, I rely on peripheral households that were not directly targeted by the policy and instrument for actual grid construction by a hypothetical grid path that isolates supply side factors of grid provision.

I find that grid construction indeed leads to an increase in out-migration both at individual level and at district level. The effect is driven by younger male household members and is at least partly associated with work-related migration. While grid expansion increases employment of the average household head, younger members of the household seem to remain in a situation of underemployment. This suggests that under the ex ante presence of credit constrains, rural electrification investments might cause net population loss for remote areas. This findings is important, given that policy-makers typically assume and aim for the opposite effect. For policy-makers these findings have the following implications: In the short run, rural electrification might increase migration pressure and policy-makers need to prepare for a smooth integration of migrants at their destinations. In the long run, there is a risk that rural areas get locked into a path of irreversible depopulation, low levels of human capital and consequently low growth which policy-makers need to counteract appropriately.

The third chapter **Broken Promises – Evaluating an Incomplete Cash Transfer**, co-authored with Utz Pape and Laura Ralston, examines the consequences of the increasing riskiness of the operating environment of development programs. Due to the growing intersection of poverty with other risk factors, there is an ever larger risk of operational disruptions or program failures. Despite the high prevalence of program failures the academic literature has so far neglected this issue. However, it would be a strong assumption that program failure or cancellation has no real and lasting effects. It is, therefore, necessary for policy-makers to understand the risks associated with operational problems.

The chapter takes advantage of a failed cash transfer program that was originally designed as a randomized trial. The intervention consisted of a lump sum transfer equivalent to 1,000 USD targeted at the youth in South Sudan with 60 percent of the grants reserved for female participants. The only condition on receiving the grant was the participation in a one-week business skills training and the opening of a formal bank account with one of the partnering bank branches. The condition was put to ensure that the grants were used productively, particularly in order to promote (self-)

employment. The grant disbursements via the partnering bank branches, however, had to be put to a halt when violent conflict re-erupted across the program states in mid 2016. While some of the participants had already received the cash grant via their bank branch, others had not. This fact and the existence of a control group make it to a unique case to study the effect of a program cancellation empirically.

In the analysis, I distinguish between three different effects: (1) the net effect on all selected beneficiaries based on the intention-to-treat, (2) the effect of the two separate *ex post* treatment groups of those that received “training and grant” and those that received “training, no grant” based on local average treatment effects, and (3) the effect of the two *ex post* treatment groups divided by gender based again on local average treatment effects. For the local average treatment effects of the two *ex post* treatments, I use an instrumental variable consisting of exogenous variation in original treatment group assignment interacted with geographic distance to the closest bank branch.

The results of the intention-to-treat estimates show no net effect on most outcomes. However, when considering the two *ex post* treatment groups separately, I find that participants that failed to access the grant show reduced levels of consumption and trust. Moreover, female participants that failed to access the grant show increased levels of risk aversion.

These findings have important implications for development practice given that an increasing share of projects will be located in volatile environments. With increasing risk of interruption or cancellation, development programs need to consider the consequences of such instances at the planning stage and minimize potential adverse outcomes. A previous version of this paper has been released as working paper in the *World Bank Policy Working Paper Series* (Müller et al. 2019).

3. Methodological approach

While each chapter offers new insights for economic theory, the main focus of this thesis is empirical. In the recent past, the economics literature has undergone an "empirical turn" that shifted the emphasis towards sound identification of causal effects (documented in Hamermesh 2013), which has lead to a larger influence of economic research outside its discipline (Angrist et al. 2020). This trend has been particularly pronounced in the field of *Development Economics* where the share of empirical publications compared to pure theoretical work has grown from 60 percent in the 1980s to more than 95 percent in 2015 (Angrist et al. 2017).

All three chapters of this dissertation apply *quasi-experimental methods* to address the very distinct identification challenges of their respective research question. Quasi-experimental methods rely on some form of *plausibly* exogenous variation of the main explanatory variable to derive results. By doing so, quasi-experimental method seek to come as close as possible to true randomization in settings where true randomization is not possible. While randomized controlled trials as sources of true randomization are increasingly regarded as the "gold standard" in the economics literature (de Souza Leão & Eyal 2019),[1] the necessity for quasi-experimental methods remains.

Each chapter of this dissertation relies on the same quasi-experimental method for identification – an *instrumental variable approach*. This method exploits exogenous variation in a variable that is correlated with the explanatory variable and is not correlated with the dependent variable via any other channel than its effect on the explanatory variable. While all chapters make use of the same method, the type of data and the identification challenge it is applied to differ greatly.

Chapter 1 uses a long global country panel that covers the period from 1950 to 2015. It seeks to identify the causal effect of democratic institutions on the decision to become a donor of foreign aid conditional in the income level. By observing countries that have just undergone democratization, one risks simultaneity bias from unobserved third variables. In an ideal world,

[1] For instance, this is documented by the 2019 Nobel Prize in Economics for Abhijiit Banerjee, Esther Duflo and Michael Kremer for their pioneer work on randomized controlled trials.

researchers would like to assign democratic institutions randomly to a pool of countries to estimate the causal effects. This is obviously not possible. Instead, we rely on regional waves of democracy as instrumental variable for the democratization process of a single country as proposed by Acemoglu & Robinson (2019).

Chapter 2 uses a geo-referenced household panel in Nigeria, covering three waves between 2009 and 2016. It seeks to identify the effect of an exogenous supply shock of electricity on internal migration. The infrastructure literature struggles with the identification of causal effects, since infrastructure is determined by strong demand side factors. Despite first attempts of true randomization that use randomized subsidies for electricity connections to under-grid households (Lee et al. 2020), the effects are likely to differ from large scale infrastructure construction. I, therefore, propose to use a quasi-experimental design to identify the effect of electricity grid expansion. In particular, the apoproach of chapter 2 draws on a method developed in the economic literature on the effects of transport infrastructure. First, it relies on a natural experiment that exploits effects of new large scale electricity infrastructure on peripheral households not directly targeted by the intervention (Faber 2014). Second, it employs an algorithm to isolate supply-side factors of grid construction to generate a hypothetical least cost path and uses this as instrumental variable for the true path of the grid.

Chapter 3 uses data from a field experiment in South Sudan that was originally planned as randomized trial. It seeks to identify the causal effect of a failure to deliver a lump-sum cash grant on intended beneficiaries. The intervention had to be cancelled mid-way due to re-erupting violence in the project region which meant that only a share of the intended participants received the grant money. Since participants had to initiate grant disbursement via a formal bank account, assignment to *ex post* treatment groups was not at random. Instead of relying on treatment-on-the-treated effects, this study proposes to apply an interacted instrument based on exogenous assignment to the treatment group and distance to the closest branch of the project partner bank. Interacted instruments consisting of a common exogenous shock and a relative exposure variable have gained popularity in the quasi-experimental literature and are valid under some mild assumption (see Christian & Barrett 2017, for a current discussion of the method). This study is innovative by applying the same logic to data from a randomized trial where existence of a control group allows for treatment group assignment to act as exogenous shock and distance to the bank as exposure share.

4. Conclusion

This dissertation explores recent changes that global development finance is faced with in times of the Sustainable Development Goals. In particular, it highlights three changes where complexity for both donors and recipients has increased.

First, this dissertation discusses the emergence of non-democratic donors as important actors in bilateral aid. As chapter 1 documents, non-democratic donors seem less likely to be motivated by altruistic preferences of their population, but rather motivated by the interests of the political elite – even against the interest of their population. This notion is crucial in understanding what the global community can and cannot expect from these donors.

In addition, chapter 2 analyzes the unintended effects of rural infrastructure development on internal migration. While household incomes rise on average, not all sub-populations benefit equally. Given *ex ante* credit constrains, this can lead to a net increase in out-migration. These migration effects are important, given that depopulation can lock regions into vicious cycles of low development. The chapter illustrates how development interventions need to be assessed holistically with respect to multiple policy objectives.

Finally, the dissertation highlights the need to consider the increasing riskiness of the operating environment of development interventions. In particular, this results in an increased risk for cancellation or failure. Chapter 2 illustrates how this can ultimately harm intended beneficiaries both economically and psychologically.

While these challenges might seem overwhelming, the pace at which scholars generate new and better data and develop empirical approaches to understand policy effects has also accelerated. What is more, scholars and policymakers alike are increasingly aware of the need to yield scientific knowledge for effective policy-making. This process is crucial to guide the financing of sustainable development today.

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Chapters

I. Democracy and Aid Donorship

Bibliographic Information

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Abstract

Almost half of the world's states provide bilateral development assistance. While previous research takes the set of donor countries as exogenous, this article introduces a new dataset on aid giving covering all countries in the world, both rich and poor, and explores the determinants of aid donorship. It argues and shows empirically that democratic institutions support the setup of an aid program in richer countries but undermine its establishment in poorer countries. The findings hold in instrumental-variable regressions and the pattern is similar for the amount of aid.

JEL: F35, F55, H77, H87, O19, O57

Keywords: foreign aid, Official Development Assistance, aid donorship, aid institutions, new donors, democracy

I.1 Introduction

The Kingdom of Morocco is a lower-middle-income country. It ranks only 123rd of 188 on the 2017 Human Development Index (HDI) published by the Human Development Report Office (2018). Still, the Kingdom has provided development aid to other countries since 1986 through the *Agence Marocaine de Coopération Internationale*. Almost all African countries, whether poorer or richer than the donor itself, are recipients of Moroccan aid. Much more recently in 2013, Mongolia ranked 92nd on the HDI has established its own outward aid institution. The *International Cooperation Fund of Mongolia* is part of the Mongolian government's strategy to "strengthen the country's role and contributions internationally as a means of diplomatic soft power policy."[1] As funders of development cooperation, Morocco and Mongolia are by no means exceptions among developing countries. In today's world, 88 countries are active as aid donors, of which 44 countries are classified as low- or middle-income economies according to World Bank classifications.

It is puzzling why some governments decide to already engage in aid donorship at early stages of economic development, while others do not. On the one hand, low- and middle-income countries face strong opportunity costs when spending resources on outgoing development aid rather than investing them directly in the development of their own countries. On the other hand, governments might be interested in reaping the benefits of aid deliveries, which have been documented by an extensive literature. For example, aid can promote geostrategic interests (Bearce & Tirone 2010, Fleck & Kilby 2010), contribute to regime changes in recipient countries that align with donor interests (Bermeo 2011, Kersting & Kilby 2014), buy political support in international organizations (Kuziemko & Werker 2006, Dreher et al. 2009), boost exports (Martínez-Zarzoso et al. 2009), and improve the donor's image in recipient countries (Dietrich et al. 2018). Aid can also serve political leaders' personal goals by channelling aid in accordance with their own electoral interests (Jablonski 2014, Dreher et al. 2019).

In this paper, we offer an explanation of how these two opposing interests enter into the decision to start aid giving. More precisely, we analyze the role of political institutions for aid initiation and how it is contingent on countries' level of development. In particular, we hypothesize that democratic institutions make it less likely that poorer countries, and more likely that richer countries, become aid donors. To enable an exploration of the role that democratic institutions play for aid initiation, we construct a database

[1] See website of Mongolia's Ministry of Foreign Affairs at http://www.mfa.gov.mn/?p=29286 (accessed September 11, 2017).

on aid donorship for all countries in the world since 1945.[2] Our regression results confirm that democracy has a positive effect on aid giving in richer countries and a negative effect in poorer countries.

There are two puzzle pieces to derive our hypothesis that democratic institutions make it less likely that poorer countries, and more likely that richer countries, become aid donors. First, we argue that the income elasticity of demand for international development varies at different income levels. Global development exhibits the characteristics of a luxury good (Dudley 1979), which is only supplied when more basic needs are fulfilled. With rising levels of per-capita income, the donor population may also demand more regional and global public goods. In particular, richer individuals are more likely to demand that their governments use aid to protect air, water, land, biodiversity, and the climate; prevent the transboundary spread of infectious diseases; combat the illicit trafficking of drugs, weapons, and wildlife; prevent the spread of terrorism and violent conflict; and address large-scale human population movements across borders (Chauvet 2003, Sandler & Arce 2007, Hicks et al. 2008, Bermeo 2017, Dreher, Fuchs & Langlotz 2019). Preferences for the provision of aid should thus rise disproportionally with increased income. Consequently, richer individuals should be more likely to accept (or even push for) the provision of development assistance to the developing world. Conversely, there should be less support for the usage of tax money for development aid if there is still a considerable degree of poverty in the potential donor country. This aligns with empirical evidence that individual income is positively associated with support for development aid giving (Chong & Gradstein 2008, Paxton & Knack 2012, Cheng & Smyth 2016).

Second, the degree to which citizens' preferences translate into actual policy making should be larger in democracies than in authoritarian regimes. In contrast to autocracies, aid policies in democratic systems require the approval of parliaments. Legislators have incentives to respond to the preferences of their constituents in their votes on aid (Milner & Tingley 2010). This implies that the lack of support for aid in poorer countries will decrease a democracy's likelihood to start aid giving. Conversely, any public opposition to aid giving should be less consequential in equally poor authoritarian regimes since the leadership there relies on a small elite rather than a large electorate. The leaders of these regimes face fewer constraints that would hinder governments from reaping the benefits of a development aid program to

[2] *Development* aid, which is the focus of our paper, is a post-Second World War phenomenon. However, foreign aid more broadly understood has deep historical roots. As Markovitz et al. (2019) highlight, European and non-European powers, such as Rome and China, frequently used foreign aid to restore, maintain or disrupt the geopolitical status quo throughout different historical episodes prior to the 20th century.

themselves and their cronies. On the contrary, we expect democracies to be more likely to initiate aid giving at high levels of income when public opinion is favorable towards the provision of global public goods. Since democracy is conducive to the development of a vivid civil society, rich democracies should become more prone to aid giving than rich autocracies (Lumsdaine & Schopf 2007).

The results presented in our paper challenge existing theories of the link between democratic institutions and aid donorship. Most prominently, Bueno de Mesquita & Smith (2007, 2009) theorize that democracies are more likely to engage in development cooperation. This result emerges as rational leaders of democratic countries support higher levels of public good provision, including policy concessions bought from other countries through development aid, in order to remain in power. Coincidentally, implications of this theory are in line with the conventional wisdom in the development literature that aid is a phenomenon driven by Western-style liberal democracies. Lumsdaine (1993), for example, explains the emergence of development aid as a reflection of domestic redistributive norms of Western welfare states, while Noël & Thérien (1995) emphasize the link to specific institutional characteristics of social democracies. However, a first glance at our new dataset raises doubts whether democracies are indeed more likely to become aid donors. For instance, China's aid activities date back to the 1950s (Dreher & Fuchs 2015). Arab countries, such as Kuwait or the United Arab Emirates, became aid donors in the 1960s and 1970s (Neumayer 2003, Werker et al. 2009). We argue and show instead that the effect of democracy on aid initiation is a function of a country's income level. More precisely, both the first aid delivery and the setup of aid institutions are more likely to occur in democratic countries at a time of high per-capita income when support of aid likely outweighs opposition to aid.

Our empirical approach addresses endogeneity concerns. Both the initiation of an aid program and measures of political regime type are institutional variables that might be simultaneously affected by country-specific and time-varying omitted variables. We address this with a variant of the instrumental-variable approach introduced by Acemoglu et al. (2019). Departing from the observation that democratization often emerges in the form of regional waves, our instrument is the lagged average level of democracy within a peer group of countries in the same world region that share a similar political history. The instrument is powerful, and we explain in detail below why we consider it unlikely that the exclusion restriction is violated. We also discuss below remaining concerns related to our identification strategy.

Our main results are robust to alternative treatment of missing values, changes in temporal aggregation, an alternative definition of our dependent variable, several extensions of the set of explanatory variables, and the exclusion of EU accession countries as potential outliers. Extending our analysis to aid volumes, we also find that, compared to authoritarian donor countries, democratic donor countries provide smaller amounts of aid when they are poorer. This suggests that our proposed mechanism is not only applicable to the initial decision to provide aid but also affects the extent of aid.

Rather than taking the sample of donor countries as exogenous, this paper is the first study to empirically explore the determinants of aid donorship with a dataset covering all countries in the world. So far, data availability has dictated which of the world's countries could be included in empirical studies of aid.[3] As a result, an overwhelming number of studies analyze donor countries organized in the OECD's Development Assistance Committee (DAC), which is a club of rich democracies.[4] Thus, existing studies that aim to shed light on aid motives run the risk of sample selection biases. Yet, a better understanding of governments' motives to start aid giving is crucial since previous research has shown that donor motives affect the effectiveness of aid (Kilby & Dreher 2010, Dreher et al. 2013).

This article proceeds as follows. Section I.2 introduces the new database on aid donors and provides a first descriptive overview on the proliferation of aid donorship across the globe. Section I.3 explains the empirical approach, including the instrumental-variables strategy, and also introduces the other datasets used in our study. In Section I.4, we present our results and discuss the robustness of our findings. We close this paper with our conclusions in Section I.5.

I.2 The New Aid Donors Database

The conventional data sources on development aid, such as OECD-DAC and AidData, report commitments and disbursements of Official Development Assistance (ODA) and Other Official Flows (OOF), but their cross-donor coverage is low and depends on the availability of data on financial values. Therefore, the absence of data for a particular country must not be interpreted as an absence of aid activities.[5] For those donors that are captured,

[3] Survey studies that provide an overview on the aid literature include Doucouliagos & Paldam (2011), Milner & Tingley (2013), Fuchs et al. (2014), and Doucouliagos (2019).

[4] Studies that extend the scope of their research to non-DAC countries cover only one or a small number of these donors (Dreher et al. 2011, Fuchs & Vadlamannati 2013, Semrau & Thiele 2017, Strange et al. 2017, Asmus et al. 2020).

[5] For example, India has provided aid since 1959 but the OECD only has reported Delhi's aid volume since 2011 (see http://www.oecd.org/dac/stats/non-dac-reporting.htm, accessed May 31, 2018) and AidData reports Indian aid projects systematically after 2007

these databases do not necessarily provide information on the entire history of their aid giving. What is even more critical for the purposes of our research question, is that aid data are not missing at random. Data availability is biased toward rich and democratic countries (Nielson et al. 2017).[6]

To fill this information gap, we build a comprehensive database on aid donorship since the end of the Second World War (Budjan & Fuchs 2020). It contains information on 114 countries from 1945-2015 on the year of a country's first outgoing aid project, the name and year of its current institution responsible for aid provision, the name and year of establishment of its first institution responsible for aid provision, and the name and year of its first aid legislation. Data were collected between March 2016 and August 2017. We constructed a questionnaire in the English language to collect data from official administrative bodies of all 175 sovereign states with a population larger than 300,000 inhabitants that are listed in the State System Membership database (Correlates of War Project 2017). We translated the original questionnaire, presented in Appendix C.3, into four additional world languages to increase the response probability (Arabic, French, Portuguese, and Spanish). In the first stage, we sent the questionnaire to the Ministry of Foreign Affairs (or the Ministry of Development Cooperation) of each country. If this inquiry was unsuccessful despite follow-up e-mails, we e-mailed the questionnaire in the second stage to another ministry of relevance (such as the Ministry of Finance), the respective embassy in Germany (the country where this study was carried out), or both. In the third stage, we contacted the relevant institutions by phone. Using this procedure, we were able to gather information for 94 countries. In the fourth stage, we verified and completed our data with information provided on government websites, the academic literature, the grey literature, and media reports. The reliance on secondary sources is low, with data for only 25 countries fully relying on such information.

In the context of our study, we define development cooperation in turn as the provision of grants, concessional loans, technical assistance, and in-kind assistance with the main objective being the promotion of the economic development and welfare of another country. By applying this definition, we broadly follow the OECD definition of ODA. In contrast to the latter, however, our definition is for several reasons agnostic about the size of the grant element inherent in a country's development activities. First, for most countries, it is not possible to obtain the relevant information. Second, the

only (Tierney et al. 2011, AidData 2017).

[6]According to the 2018 Aid Transparency Index (Publish What You Fund 2018), China and the United Arab Emirates, the only autocracies included in the index, rank at the bottom.

computation of the grant element in ODA according to OECD definitions is subject to controversies in the development community (Barder & Klasen 2014). Finally, it is important to note that our definition of development cooperation excludes military aid, anti-terrorism activities, and humanitarian assistance.[7]

We employ two definitions to identify the year in which a country becomes an aid donor. As a starting point, considering the broadest possible definition, we define a country as an aid donor if it already has provided development assistance at least one time to another country. We thus obtain a binary variable that assigns a value of one in the year of the first development cooperation activity, and zero in all years preceding this event.[8] The first countries to provide development assistance were Mexico in 1943, the Netherlands in 1949, and China and the United States in 1950. By the end of 2015, 91 countries had assumed the role of a donor of development assistance according to this broad definition. The countries that most recently entered the club of aid donors were Paraguay and Timor-Leste in 2014.

The downside of our broad definition is that even countries that have only provided a single small development project or a tiny amount of aid money would fall under it. One could argue instead that only countries that have institutionalized their aid giving should be defined as aid donors. This is why our second definition only codes countries as aid donors if they have set up an administrative body whose main responsibility is the management of outgoing development assistance. This includes departments within a country's Ministry of Foreign Affairs, a separate Ministry for Development Cooperation, and aid agencies operating independently.[9] The resulting dependent variable thus takes a value of one in the year a country establishes its first aid institution, i.e., the first administrative body for the provision of aid (or

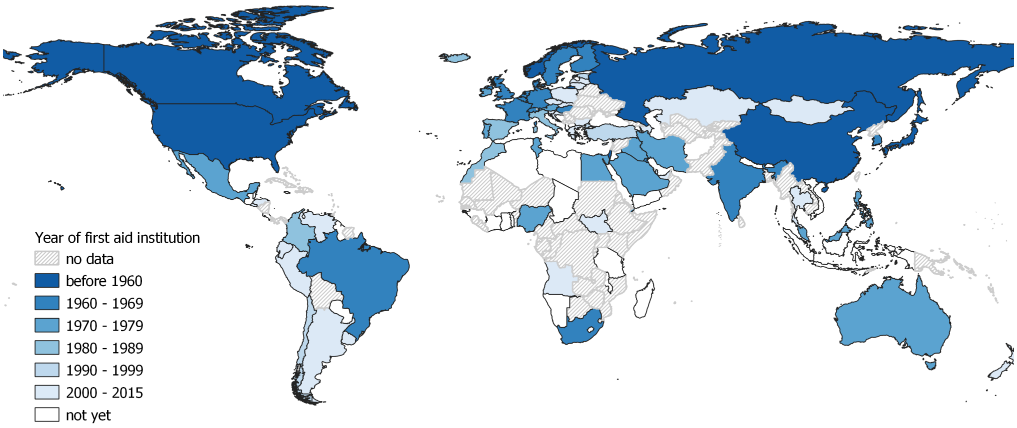
[7]The exclusion of military aid and anti-terrorism activities follows OECD definitions. Humanitarian assistance differs from general development assistance in that it is the response to an immediate, short-term need rather than aiming at more long-term development targets. What is more, humanitarian assistance is often not dealt with within the same administrative bodies as general development assistance.

[8]We did not attempt to gather systematic information on when countries ceased aid giving. While this seems to be a very rare event, in the course of our data collection, we noted two cases. First, Iraq stopped providing aid via the Iraqi Fund for External Developments in 1982. Second, Cyprus stopped its aid activities in 2011 only 5 years after starting them due to the impact of the financial crisis.

[9]For instance, Finland (Department for Development Policy) and Honduras (Dirección General de Cooperación Internacional) organize their development aid via a department within the Ministry of Foreign Affairs. Countries such as Brazil (Agência Brasileira de Cooperação) and Kuwait (Kuwait Fund for Arab Economic Development) maintain independent aid agencies.

redefined the main purpose of an existing administrative body such that it falls under our definition). The first countries to set up aid institutions were the United States in 1950, Norway in 1953, and Japan in 1954. By the end of 2015, 76 countries had assumed the role of a donor of development assistance according to this narrow definition. The last country entering this club was Venezuela in 2015. Figure I.1 shows a world map that graphically displays the time period in which countries became an aid donor according to this narrow definition of aid donorship.[10]

Figure I.1: Year of first aid institution by country (1950-2015)



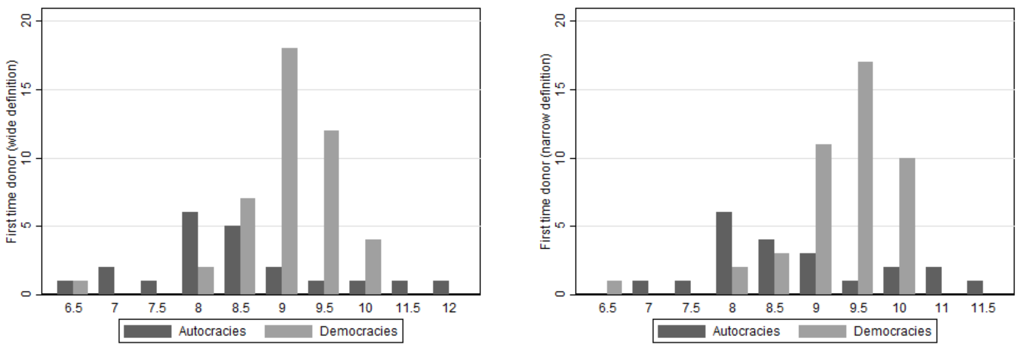
Source: Authors' dataset (see text and appendix for details). Country boundaries originate from the Cshapes dataset (Weidmann et al. 2010, Weidmann & Gleditsch 2016).

Since establishing an aid institution signals a commitment for repeated aid deliveries, the narrow definition of our dependent variable is our preferred definition. Nevertheless, we show regressions that employ the broad definition for comparison. In our empirical analysis below, we assume that all countries for which we found no indication that they act as an aid donor have not yet provided aid.[11] We believe that this is a plausible assumption as countries are only missing from the original dataset if neither literature

[10]Appendix Figure A.4.2 shows the corresponding map for the broad definition. Appendix Figure A.2.1 provides a list of all countries with the respective year of their first aid delivery and establishment of an aid institution. Six countries for which we found evidence that they are active as donors of development aid but could not determine the year of their first aid project were coded as missing values and thus excluded from the regression analysis below. These are Algeria, Bahrain, Iran, Pakistan, Peru, and Vietnam.

[11]This is the case for 61 countries for the first aid delivery variable (broad definition) and 65 countries for the aid institution variable (narrow definition).

Figure I.2: Aid initiation by income group
Panel (a) First aid delivery Panel (b) First aid institution



Source: Authors' dataset (see text and appendix for details). Data on logged GDP per capita from Penn World Tables 9.0 (Feenstra et al. 2015, 2019).

searches, internet research, nor direct contact with the ministries could confirm or disconfirm the existence of an aid institution. It is very unlikely that we would not have gathered information on a donor despite a country's active engagement in development cooperation. As a test of robustness, however, we also show regression results with a "limited sample," where we treat these cases as missing values and obtain similar results.

To illustrate our main argument with our new data, Figure I.2 plots histograms of the logged per-capita income level at which countries become new aid donors (according to both definitions) separately for democracies and authoritarian regimes. As can be seen, new authoritarian donors have on average a lower income level compared to their democratic counterparts at the time of aid initiation. This is first descriptive evidence that would be in line with our hypothesis that the lack of democratic institutions facilitates the introduction of new aid programs at low levels of income.

I.3 Empirical Approach

We estimate the probability of becoming an aid donor in a given year. As outlined above, we expect that a country's political institutions have heterogeneous effects on the probability of becoming an aid donor depending on its level of economic development. Therefore, we analyze an interaction effect of democracy and income. We estimate a linear probability model since the interpretation of interaction effects and the estimation with fixed effects is not straightforward in non-linear models (Ai & Norton 2003, Greene 2010). Our model takes the following form:

$$Pr(donor_{it} = 1|D_{it-1}, G_{it-1}, X_{it-1}) = \beta_1 D_{it-1} + \beta_2 G_{it-1} + \beta_3 D_{it-1} \times G_{it-1} + X'_{it-1}\beta_4 + H(\cdot) + \gamma_i + \delta_t, \tag{I.1}$$

where $donor_{it}$ is a binary variable that takes the value one in the year t in which a country i becomes a donor of development aid, and zero in the years before, $D_{i,t-1}$ is a measure of democracy, $G_{i,t-1}$ is the natural logarithm of country i's per-capita GDP, and $X_{i,t-1}$ is a vector of control variables for country i in year $t-1$. The function $H(\cdot)$ controls for duration dependence with the inclusion of a cubic time trend, which begins either at the beginning of our sample, or – if a country reaches independence later than 1950 – at the year of independence.[12] Finally, γ_i and δ_t are full sets of country- and year-fixed effects. Countries generally enter the sample in 1951, which is the beginning of our period of observation due to data constraints. They drop out of the sample after the country has become an aid donor. Countries that gained independence after 1951 enter the sample at their respective year of independence. Standard errors are clustered at the country level.

Data on logged per-capita GDP come from the Penn World Tables (Feenstra et al. 2015, 2019). As our measure of democracy, we construct the consolidated dichotomous measure proposed in Acemoglu et al. (2019) that intends to overcome measurement error in its constituent variables.[13]

As in most non-experimental studies, our analysis has to deal with concerns of endogeneity. Both democracy and the initiation of an aid program are linked to a country's institutional and political characteristics. Hence, it is possible that changes in both variables are spuriously correlated due to a third variable that drives the effect. For instance, social unrest could trigger an autocratic government to suppress opposition forces and thus make a democratization process *less* likely. At the same time, social unrest also *raises* the government's incentives to buy external support via development aid.[14]

[12] We expect that countries have a low probability to start an aid initiative just after reaching independence, but this probability is likely to increase over time in a process of institution building.

[13] A country is coded as democratic if it is considered as "Free" or "Partially Free" by (Freedom House 2016) and receives a positive score in the Polity IV database (Marshall et al. 2016). Also following Acemoglu et al. (2019), we fill missing entries in one of these data sources with the dichotomous democracy measure of Cheibub et al. (2010), which has been updated by Bjørnskov & Rode (2020), and adopt all manual corrections suggested by Acemoglu et al. (2019). See maps in Appendix Figure A.2.1 for a graphical representation of the resulting measure.

[14] This is also visible in the extent of aid given. For example, the pro-democracy demonstrations on Beijing's Tian'anmen Square in 1989 triggered a sharp response by the Chinese government which made democratization less likely. At the same time, the Chinese gov-

Such a spurious negative correlation between democratization and new aid donorship could lead to a downward bias of our OLS results. While many endogeneity concerns can be mitigated through the inclusion of control variables, the risk of simultaneity bias stemming from unobserved variables that vary across countries and time remains.

To address endogeneity concerns, we employ an instrumental-variables approach suggested by Acemoglu et al. (2019). Their instrumental variable builds on the idea that democratization processes often result from regional waves of democratization (Huntington 1991, Markoff 1996). For example, countries in Latin America and the Caribbean experienced a wave of reversal from democracy in the 1970s and moved collectively back to democracy in the late 1980s and early 1990s. More recently, the Arab Spring began in 2010 in Tunisia and quickly spread over to other countries in the Middle East and North Africa. The dominant explanation in the literature for these waves of democratization is that democratization processes can influence citizens' demand for democracy in countries with a similar culture, political history, and with close informational ties.

Building on this argument, we exploit exogenous variation in democracy that results from regional waves of democratization. To do so, we group countries according to the seven geographic regions of the World Bank Country Classification (World Bank 2016).[15] The instrumental variable $Z_{i,t}$ is then constructed as the lagged average level of democracy within a peer group of countries. Algebraically, this can be written as

$$Z_{it} = \frac{1}{n} \sum_{j=1}^{n} D_{jt-1}, \tag{I.2}$$

where n signifies the number of countries j in the peer group of country i. Following Acemoglu et al. (2019), we define the peer group as all countries j within the same region whose regime type coincides with i's regime type at the beginning of the sample period. For countries that reached independence after 1950, we determine the peer group at the respective year of independence. The resulting instrumental variable is a continuous measure that ranges from 0 to 1.

ernment increased its development aid to buy political support from recipient countries to shield China against pressure from Western countries in international fora (Dreher & Fuchs 2015).

[15] The seven world regions are East Asia and Pacific, Europe and Central Asia, Latin America and the Caribbean, Middle East and North Africa, North America, South Asia, and Sub-Saharan Africa.

The instrumental variable is excludable if the regional wave of democratization Z_{it} has no effect on a country's likelihood to initiate an aid program other than through its political regime type.[16] While it is hard to come up with arguments why the exclusion restriction could be violated, we discuss likely concerns. First, it is possible that not only democracy, but also aid donorship moves in regional waves. If there are regional waves of aid donorship, these could spread across the same channels as democracy and be driven by the same domestic forces. For instance, it is possible that the demand for more civilian rights and the demand for development aid are driven by the same moral forces within the population (see Lumsdaine 1993, for a similar argument). Since development cooperation is a low-salience issue in domestic politics (Lundsgaarde 2013, Szent-Iványi & Lightfoot 2015), we judge such a violation of the exclusion restriction unlikely but possible.

Second, it could be possible that regional economic booms both cause regional waves of democratization and increase the likelihood of any single country to begin a development aid program due to increased income. However, Acemoglu et al. (2019) note that scholars agree that waves of regional democratization are not caused by regional economic trends. This would imply that while economic growth in any single country might increase its likelihood to democratize, regional waves of democratization are exogenous to a country's current income level. The argument seems plausible since channels by which democratization waves spread are likely orthogonal to regional economic trends. Nevertheless, we test both potential violations of the exclusion restrictions below.

We run specifications with and without the following control variables.[17] First, we include a country's government share of GDP to control for the availability of government resources to start an aid program (Bueno de Mesquita & Smith 2009). Second, we control for the political distance vis-à-vis the United States (measured using voting behavior in the United Nations).[18] We expect countries that are less distant to the United States to be more

[16] Although we believe that our instrument for democracy is a valid instrument for a range of other dependent variables that could affect aid donorship, such as economic growth as analyzed in Acemoglu et al. (2019), endogeneity via such transmission channels does not threaten the identification of the total, direct and indirect, effect of democracy on aid donorship.

[17] Data from Gleditsch et al. (2002), Voeten et al. (2009), Themnér & Wallensteen (2013), Feenstra et al. (2015), Bailey et al. (2017).

[18] United Nations General Assembly voting data are frequently used to measure political relations between countries (e.g. Alesina & Dollar 2000, Dreher et al. 2008). Specifically, we take the difference between the United States and the potential donor country of their ideal point estimate along a single dimension that captures the position vis-à-vis a "US-led liberal order."

likely to be convinced (or coerced) to follow the United States' model in setting up an aid program. Third, we use logged total population size in expectation that larger countries are more willing to give aid, while smaller countries have stronger incentives to free-ride on the aid efforts of their larger peers. Fourth, trade openness (defined as the sum of exports and imports as a percentage of GDP) controls for domestic business interests in setting up an aid program. Finally, a binary variable that marks every year during which a country was involved in an internal or internalized conflict over territory allows us to control for a country's interest in buying political support through aid.[19]

Table I.1: Descriptive statistics

Variable names	**Observations**	**Mean**	**Std. Dev.**	**Min**	**Max**
Dependent variables					
Aid donorship (broad definition)	4,601	0.0111	0.105	0	1
Aid donorship (narrow definition)	5,193	0.0114	0.106	0	1
(log) Aid disbursements (OECD)	1,344	6.655	1.971	0.308	10.45
(log) Aid commitments (AidData)	977	6.577	2.284	0.00171	10.44
Explanatory variables (in alphabetical order)					
Cold War dummy	5,193	0.570	0.495	0	1
(log) Colony population	4,941	0.919	3.761	0	20.38
Democracy (baseline)	5,193	0.445	0.497	0	1
Democracy (DD)	5,140	0.400	0.490	0	1
Democracy (ethnic winning coalition)	4,678	0.803	0.257	0.0200	1
Democracy (electoral democracy)	4,797	0.401	0.255	0.0158	0.933
Democracy (instrument)	5,193	0.425	0.370	0	1
Democracy (Polity IV)	4,929	0.500	0.356	0	1
Democracy (winning coalition)	4,865	0.544	0.296	0	1
Donor spatial lag (by geographic distance)	5,193	0.140	0.0894	0	0.545
Donor spatial lag (by democracy peer group)	5,045	0.328	0.328	0	1
GDP spatial lag (by geographic distance)	5,193	8.786	0.640	6.407	10.14
GDP spatial lag (by democracy peer group)	5,125	8.671	0.836	6.182	10.97
Duration	5,193	28.88	16.72	3	71
(log) GDP per capita	5,193	8.313	1.117	5.085	12.33
Government share of GDP	5,193	0.149	0.0729	0.0144	0.944
Intrastate conflict over territory	5,193	0.0416	0.200	0	1
Militarized interstate dispute	5,193	0.126	0.332	0	1
Openness	5,193	0.696	0.436	0.0359	4.110
Political distance to Russia	5,193	1.698	1.164	0.0010	5.215
Political distance to US	5,193	2.509	1.037	0	4.986
(log) Population	5,193	15.41	1.458	11.72	19.98

Notes: The descriptive statistics are based on the estimation sample of Table 2, column 6, for all variables except from (log) Aid disbursement (OECD) and (log) Aid commitments (AidData). The latter are based on regression samples from Appendix B7.

[19]For example, the Africa Research Bulletin (2017) notes that "Morocco is now using mega-projects to mend ties with East African countries long at odds with Rabat over the Western Sahara issue."

Table I.1 provides descriptive statistics on the variables used in this paper. On average, one percent of countries that have not yet become aid donors start aid giving in a given year. Appendix Table A.2.2 displays the correlation between our variables. Finally, Appendix Table A.2.3 provides details on the definitions and sources of all variables employed in the analysis.

I.4 Results

I.4.1 Main Results

Table I.2 presents our main results. As a benchmark, in columns 1–2 we show the additive effects of democracy and per-capita income on aid donorship without interaction. We start with ordinary-least squares (OLS) regressions in column 1 and apply the instrumental-variables strategy using two-stage least-squares (2SLS) regressions in column 2.[20] The instrument is powerful as suggested by the first-stage F statistic which is above the rule-of-thumb value of ten. The results show a positive relationship between a country's income level and the likelihood of becoming an aid donor. The corresponding coefficient is statistically significant at the one-percent level in both specifications. Quantitatively, a country that is twice as rich, such as Germany compared to Guatemala in 1950, has a probability of becoming an aid donor that is 1.1 percentage point higher (column 2).[21] Given that the average probability of initiating aid giving is also 1.1 percent, this effect is sizable.

At the same time, we find no evidence that democracies are, on average, more likely to initiate aid giving. The corresponding coefficient on democracy is even significantly negative in column 1, suggesting that countries are less likely to initiate aid giving when they are under democratic rule. However, according to the 2SLS results in column 2, the coefficient on democracy is close to zero and does not reach statistical significance at conventional levels. This considerably less negative 2SLS estimate is in line with the expected downward bias of OLS caused by omitted third variables (e.g., social unrest that causes both a shift towards authoritarianism and an aid program to buy political support).

To test our hypothesis that democratic institutions make it unlikely that poorer countries become aid donors, we replicate the specifications from columns 1–2 while adding an interaction of democracy and income. As the results presented in columns 3–4 show, the coefficient on the interaction

[20] We report the corresponding first-stage regression results in Appendix Table A.3.1.

[21] 0.0155*ln(2).

Table I.2: Democracy, income, and aid donorship (1951-2015, baseline)

	(1) OLS FE Narrow	(2) 2SLS FE Narrow	(3) OLS FE Narrow	(4) 2SLS FE Narrow	(5) OLS FE Narrow	(6) 2SLS FE Narrow	(7) OLS FE Broad	(8) 2SLS FE Broad
Democracy	-0.0068**	-0.0008	-0.0810**	-0.4119***	-0.0771**	-0.4580***	-0.0428	-0.3123***
	(0.0031)	(0.0269)	(0.0317)	(0.0997)	(0.0350)	(0.1274)	(0.0332)	(0.1034)
(log) GDP per capita	0.0148***	0.0155***	0.0104**	-0.0090	0.0059	-0.0144	0.0091	-0.0047
	(0.0049)	(0.0051)	(0.0049)	(0.0080)	(0.0051)	(0.0089)	(0.0061)	(0.0080)
Democracy # (log) GDP per capita			0.0093**	0.0524***	0.0088*	0.0580***	0.0042	0.0408***
			(0.0041)	(0.0141)	(0.0045)	(0.0178)	(0.0044)	(0.0142)
Government share of GDP					-0.0175	-0.0090	-0.0114	-0.0047
					(0.0186)	(0.0222)	(0.0172)	(0.0198)
Political distance to USA					-0.0109**	-0.0116**	-0.0087*	-0.0090
					(0.0044)	(0.0051)	(0.0050)	(0.0058)
(log) Population					-0.0284	0.0010	-0.0427***	-0.0241
					(0.0211)	(0.0227)	(0.0143)	(0.0148)
Openness					0.0067	0.0124*	0.0013	0.0033
					(0.0063)	(0.0071)	(0.0059)	(0.0066)
Intrastate conflict over territory					0.0145	0.0209	0.0158	0.0261
					(0.0146)	(0.0159)	(0.0196)	(0.0205)
Country and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,502	5,377	5,502	5,377	5,300	5,193	4,764	4,658
Number of countries	147	145	147	145	141	140	131	126
R-squared	0.0261		0.0267		0.0272		0.0253	
Kleibergen-Paap F-stat		22.54		10.19		11.76		13.10

Notes: The dependent variable in columns 1–6 is a binary variable that takes a value of one in the year a country establishes its first aid institution (narrow definition). The dependent variable in columns 7–8 is a binary variable that takes a value of one in the year of undertaking the very first activity of development aid (broad definition). Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels.

between democracy and per-capita GDP is—in line with our expectations—always positive and statistically significant at least at the five-percent level.

Our main findings are largely unaffected when we include the set of control variables in columns 5–6 of Table I.2. Concerning the controls, we find that countries that are politically distant to the United States are less likely to initiate aid giving. Being one ideal point closer to the United States, such as Israel compared to Cuba in 1960, increases the likelihood of setting up the first aid institution by 1.2 percentage points (column 6). Population size, trade openness, government share of GDP and intrastate conflict over territory do not appear to robustly affect aid initiation, at conventional levels of statistical significance.

The upper left panel of Figure I.3 visualizes the heterogeneous effects based on the results in column 6 of Table I.2. Using the 90% confidence interval, we find that democracies with a logged per-capita GDP above 9.8, such as Spain in 1980, have a significantly larger probability to initiate aid giving than authoritarian countries at the same income level, such as Belarus or the Maldives in 2012. Conversely, democracies with a logged per-capita GDP below 7, such as Mali in 1996, are significantly less likely to become aid donors than their authoritarian counterparts, such as Benin or China in 1960. This supports our hypothesis that democratic institutions promote aid initiation in richer countries, while they prevent poorer countries from becoming an aid donor. We also evaluate the impact of democracy at the sample median of (log) per-capita GDP, 8.3 (see vertical line in Figure I.3). Again in line with the expected downward bias of OLS, the marginal effect at median income is larger in the 2SLS (0.0230) than in the OLS estimation (-0.0038). Since the 90% interval plotted in the upper left panel of Figure I.3 includes 0, we conclude that a democracy with an average income level is not more or less likely than an authoritarian regime to initiate aid giving.

Columns 7 and 8 of Table I.2 show regression results for the broad definition of aid donorship based on the year of the first aid delivery—rather than for the narrow definition of aid donorship based on the year of the first setup of an institution that manages aid giving. Again, we find a positive coefficient on the interaction of income and democracy, which reaches statistical significance at the one-percent level in our preferred specification where we control for endogeneity (column 8). Among high-income countries, democracies are more likely to provide their first aid project, whereas among poorer countries authoritarian regimes are more likely to start aid giving. We plot the corresponding marginal effects with 90% confidence interval in the upper right panel of Figure I.3.

Causal identification with 2SLS requires that the exclusion restriction is satisfied. As discussed in Section 2, there is little reason to expect that the exclusion restriction is violated in our case. Nevertheless, we test the robustness of our results against potential violations.

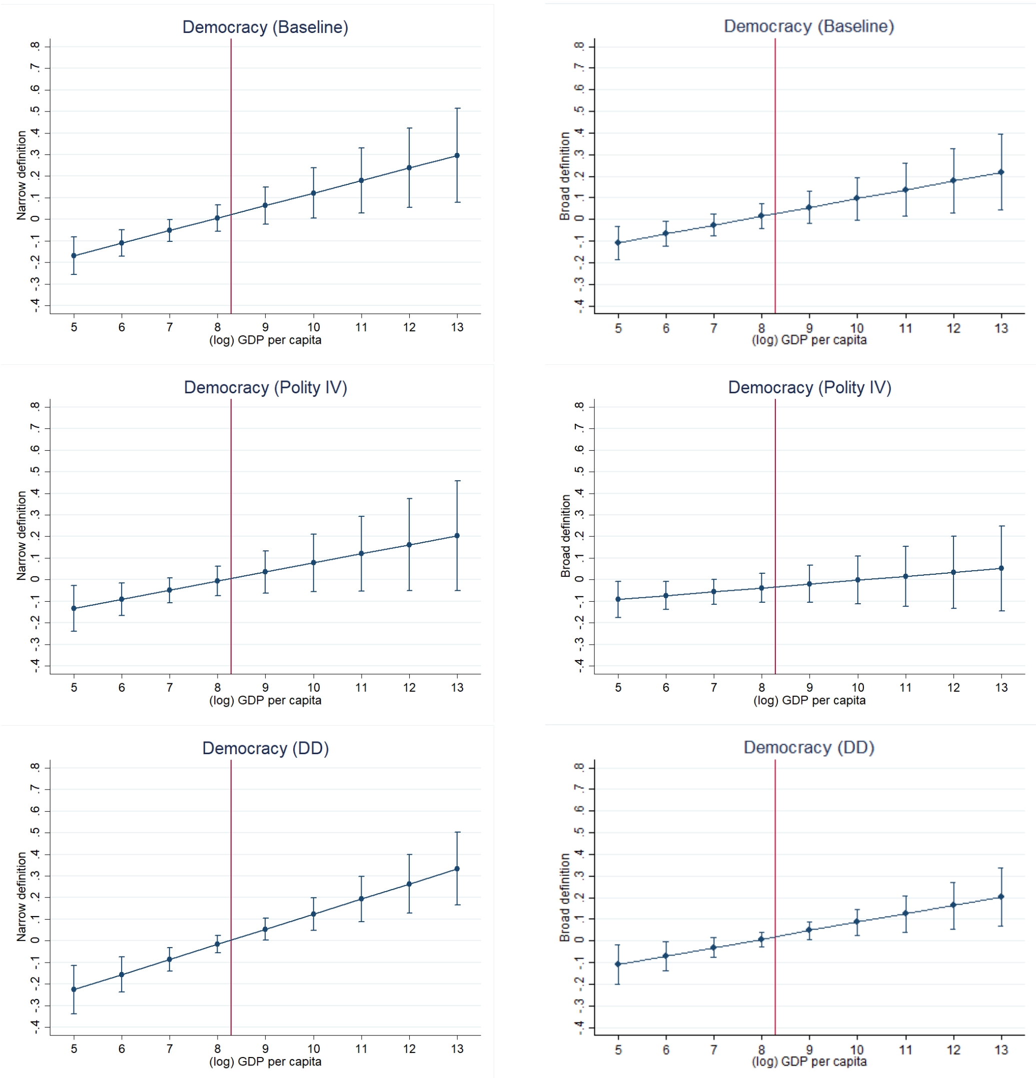
First, if the initiation of aid initiatives follows regional waves that run in parallel to democratization waves, then our democracy instrument would not be excludable. To control for regional waves of aid donorship, we introduce a spatial lag of our dependent variable. In columns 1 and 2 of Table I.3, we use the inverse geographical distance between countries as weights.[22] In column 3 and 4, we apply a spatial lag of aid donorship based on the weighting mechanism of our democracy instrument, i.e., building the weighted average of aid donorship among the peer group of countries j within the same geographic region and a similar political history as country i. The respective inclusion of each measure does not affect our main findings. This makes us confident that our main results are not driven by a spurious correlation caused by regional waves of aid donorship.

Second, the exclusion restriction would be violated if regional economic trends were the underlying drivers of both waves of democratization and an increase in the regional share of donors. We therefore include a spatial lag

[22] A significantly positive coefficient on the spatial lag could either hint at competition, learning, or emulation as drivers of policy diffusion. For example, Gulrajani & Swiss (2017) explain the spread of aid donorship with a normative diffusion process in which countries strive to graduate from recipients to donors to signal "developed country status."

Figure I.3: Marginal effect of democracy on aid donorship across income levels

(a) First aid institution (b) First aid delivery



Notes: The figure displays the marginal effects of democracy on a country's likelihood to become an aid donor (left panel using the narrow definition; right panel using the broad defintion) at different levels of per-capita income based on 2SLS regressions. Each subfigure uses one of three alternative measures of democracy, as indicated in its header. *Democracy (baseline)* is our baseline measure of democracy, i.e., the democracy dummy as in Acemoglu et al. (2019), *Democracy (Polity IV)* is a discrete ordinal score of a country's regime type on a democracy-autocracy scale, which we normalized between 0 and 1, and *Democracy (DD)* is the democracy dummy developed by Cheibub et al. (2010) and updated by Bjørnskov & Rode (2020). The figure also displays 90% confidence intervals. The vertical lines indicate the sample median of per-capita income. Full regression results are reported in Appendix A.3.2.

Table I.3: Democracy, income, and aid donorship (1951-2015, controlled for spatial lags)

	(1) OLS FE	(2) 2SLS FE	(3) OLS FE	(4) 2SLS FE	(5) OLS FE	(6) 2SLS FE	(7) OLS FE	(8) 2SLS FE
Democracy	-0.0788**	-0.4752***	-0.0753**	-0.3703***	-0.0720**	-0.4536***	-0.0687*	-0.4638***
	(0.0339)	(0.1255)	(0.0358)	(0.1156)	(0.0349)	(0.1285)	(0.0360)	(0.1314)
(log) GDP per capita	0.0049	-0.0169*	0.0074	-0.0068	0.0075	-0.0138	0.0026	-0.0187**
	(0.0054)	(0.0090)	(0.0054)	(0.0067)	(0.0055)	(0.0091)	(0.0054)	(0.0095)
Democracy	0.0090**	0.0605***	0.0088*	0.0474***	0.0082*	0.0575***	0.0077*	0.0594***
# (log) GDP per capita	(0.0043)	(0.0173)	(0.0046)	(0.0156)	(0.0045)	(0.0179)	(0.0046)	(0.0183)
Donor spatial lag	0.3823***	0.3922***						
(by geographic distance)	(0.1097)	(0.1059)						
Donor spatial lag			0.0254**	0.0216				
(by democracy peer group)			(0.0127)	(0.0137)				
GDP spatial lag					-0.0183	-0.0077		
(by geographic distance)					(0.0194)	(0.0203)		
GDP spatial lag							0.0167*	0.0137
(by democracy peer group)							(0.0095)	(0.0091)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,193	5,193	5,045	5,045	5,193	5,193	5,125	5,125
Number of countries	140	140	134	134	140	140	138	138
R-squared	0.0476		0.0434		0.0418		0.0427	
Kleibergen-Paap F-stat		11.75		10.06		11.73		12.88

Notes: The dependent variable is a binary variable that takes a value of one in the year a country establishes its first aid institution (narrow definition). We show the corresponding results for the broad definition in Online Appendix C2. All regressions include all control variables as in columns 5–8 of Table 2. Standard errors are clustered at the country level and reported in parentheses.*** 1%, ** 5%, and * 10 % significance levels.

of GDP, which we first weight by geographic distance (column 5 and 6) and then by democracy peer group (column 7 and 8). Our main findings also prove robust to this test. It seems therefore unlikely that regional economic trends bias our analysis.[23]

Finally, it is possible that the selection of our democracy measure drives our findings. We therefore test the robustness of our results by replacing our baseline measure based on Acemoglu et al. (2019) with alternative measures of political institutions. First, we use the Polity 2 score of the Polity IV project (Marshall et al. 2016) and measure democracy on a 21-point scale ranging from 0 (hereditary monarchy) to 1 (consolidated democracy). Second, we employ the binary Democracy-Dictatorship (DD) index (Cheibub et al. 2010, Bjørnskov & Rode 2020). Countries count as democracies if the executive is directly or indirectly elected via the legislature, the legislature itself is directly elected, a multi-party system exists, and the executive power alternates between different parties under the same electoral rule.

The bottom four graphs in Figure I.3 display the marginal effect of these alternative measures of democracy on aid institution across levels of income.

[23] Online C2 replicates Table I.3 for the broad definition of aid donorship. We come to the same conclusions.

Using 2SLS, we find in all but one specification the expected pattern. The exception is the Polity IV regression with the broad definition of aid donorship, in which case the regime indicator does not reach statistical significance across all income levels. We conclude that our main findings are robust to the choice of the specific democracy measure with our preferred, narrow measure of aid donorship. However, the extent to which our findings hold for the broad definition of aid donorship depends on the chosen measure of democracy. We come to similar conclusions when we analyze the robustness to democracy measurement with OLS rather than with 2SLS regressions (Appendix Table A.3.2).[24]

I.4.2 Extensions and further robustness tests

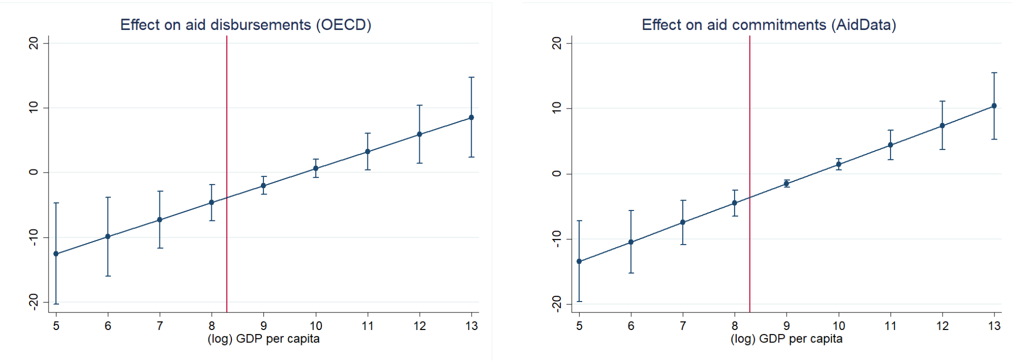
We test the robustness of our main results with respect to the treatment of missing values, temporal aggregation, the set of control variables, and the exclusion of EU accession countries. First, to test robustness with respect to the treatment of missing values, we no longer assume that all countries missing from our dataset on aid donorship have not yet provided aid. Second, we run regressions with our data averaged over three-year periods rather than using annual observations. Both robustness tests confirm our earlier findings (Appendix Table A.3.3).

Third, we also test the robustness of our main results against several extensions of the set of explanatory variables employed. One, we control for political distance to the Soviet Union, or its legal successor Russia after its dissolution, in addition to the political distance to the United States. Two, we include a binary variable that takes a value of one in years prior to 1991 to account for different dynamics during the Cold War.[25] Three, we control for years during which a country is involved in a militarized conflict (data from Maoz et al. 2019, Palmer et al. 2020). Governments may use aid to buy international support during wars and other militarized conflicts (Lundborg

[24] In Appendix Table A.3.2, we show and discuss results for three additional measures of democracy. As third alternative measure, we use data from Marshall et al. (2016) and Banks & Wilson (2016) and include the size of countries' winning coalition, defined as the group of citizens whose support the leader needs to retain office (Bueno de Mesquita et al. 2005, Bueno de Mesquita & Smith 2009). The fourth variable is the winning coalition size calculated based on the population share of ethnic groups that are included in a country's executive (Bormann et al. 2017, Cederman et al. 2009). Fifth, we add an index of electoral democracy from the V-Dem project (Coppedge et al. 2016), which measures the degree to which electoral competition makes rulers responsive to citizens.

[25] The end of the Cold War is said to have reduced the strategic motives for giving aid (e.g., Meernik et al. 1998) and led to a reduction of aid effort by OECD countries (Tingley 2010).

Figure I.4: Marginal effect of democracy on aid volumes across income levels (2SLS, 1971–2013/2015)



Notes: The figure displays the marginal effects of democracy on a donor country's aid volume at different levels of per-capita income. Each subfigure uses one of two alternative measures of aid volume, as indicated in its header. The left column uses a donor country's logged annual total ODA disbursements over the 1971–2015 period as reported by the OECD (in millions of constant 2017 US dollar). The right column uses a donor country's logged annual total aid commitments over the 1971–2013 period as reported by AidData (in millions of constant 2011 US dollar). The figure also displays 90% confidence intervals. The vertical lines indicate sample median per capita income. Full regression results are reported in Appendix Table A.3.6.

1998). Last, we include a variable for the total population living in former colonies of a country (data from Mayer & Zignago 2011, Feenstra et al. 2015, 2019). Countries could have stronger incentives to establish a development aid initiative as a substitute for their colonies when they reach independence. All of these variables, however, do not appear to matter for aid initiation (see Appendix Table A.3.4). Our main findings are qualitatively unchanged.

Finally, we investigate whether our results could be driven by EU accession countries. Countries could have introduced an aid program to please the EU Commission and member states in view of the accession negotiations. Szent-Iványi & Lightfoot (2015, 21) note that "[t]he EU has played an especially important role in 'convincing' [...] ECE [Eastern and Central European] countries to restart their international development policies during the accession negotiations."[26] When we exclude these countries and those with ongoing accession negotiations, we come to the same qualitative conclusions (see Appendix Table A.3.5). This further increases our confidence in the findings.

I.4.3 Democracy and aid volumes

Having so far focused on the initial decision to become an aid donor, we now test whether a country's political institutions also affect the size of its development cooperation once it belongs to the group of aid donors. Specifically, we re-run our main analysis with a new dependent variable: the donor country's aid volume. In analogy to our previous line of argumentation, we expect that democratic institutions dampen the volume of aid provided by poorer democracies and increase aid volumes by richer democracies. The analysis of aid volumes comes with the caveat that data are not available for a large set of countries. We did not attempt to collect a comprehensive database on aid volumes, as the gathering of data on the much simpler aid initiation variables for a global sample since 1945 already proved to be very challenging. The results should thus be interpreted with caution as they may be subject to sample selection biases. As highlighted in the introduction to this paper, democratic countries are more transparent about their aid activities than authoritarian regimes.

Our new dependent variables are a donor country's logged annual (1) total ODA disbursements as reported by the OECD (2019), and (2) total aid commitments as reported by AidData (Tierney et al. 2011), both measured in millions of constant US dollars. Figure I.4 graphically displays the results from OLS regressions.[27] Specifically, we show the marginal effects of democracy on the extent of aid across income levels for both measures of aid volumes. We obtain a consistent pattern. According to the 90% confidence interval, democratic institutions reduce aid commitments if the donor country has a logged income below 9.3 ($\approx$US\$10,938), and increase it above 10.6 ($\approx$US\$40,135).[28] To give an example, democratic countries with an income just above the latter threshold, such as Denmark in 1999, have significantly larger aid budgets than similarly rich autocratic countries such as Saudi Arabia. As can be seen from the vertical lines that again indicate the median income of all countries in our sample, a democracy with an average income level provides significantly more aid than an authoritarian regime. Summing up, we find that richer democratic donor countries provide more aid, while poorer democracies give less aid than their authoritarian counterparts, which

[26]This was confirmed by our own expert interview with an official at Poland's Department of Development Cooperation, Warsaw, September 6, 2017.

[27]We provide full regression results in Appendix Table A.3.6. Note that we do not report 2SLS results since the power of our instrument is low in our aid volume regressions, which rely on a much smaller number of countries and years compared to the aid initiation regressions above.

[28]Analogously, democratic institutions reduce aid disbursements if the donor country has a logged income below 9.4, and increase it above 19.6.

is in line with our findings for aid initiation.

I.5 Conclusions

In this article, we have shed a new perspective on aid giving. Rather than taking the set of donors of development aid as exogenous, we have built a new global database on aid donorship and analyzed the determinants of countries' decision to become an aid donor in the first place. We argued and showed empirically that democratic institutions support the setup of an aid program in richer countries but undermine its establishment in poorer countries—–in line with the theoretical expectation that public opinion on aid is more likely to affect political decisions in democracies than in authoritarian regimes. To address endogeneity concerns, we followed Acemoglu et al. (2019) and built an instrumental variable based on the idea that democratization spreads in waves. Our main finding is robust to alternative treatment of missing values, changes in temporal aggregation, a broader definition of our dependent variable, several extensions of the set of explanatory variables, and the exclusion of EU accession countries as potential outliers. In line with these findings on donor status, we also find that poorer democracies provide less aid funding than poorer autocracies and, conversely, that richer democracies provide more aid funding than richer autocracies.

Our results provide a starting point for further research. First, while our analysis with the limited available aid budget data suggested that the mechanism discussed in this paper applies to the extent of aid as well, researchers should re-investigate the determinants of the intensive margin of aid with a global sample of donors once data availability allows such investigations. Second, while the focus of our work is on bilateral aid, future research could also study the role of political institutions in the emergence of multilateral donors (Pratt 2017) and the creation of trust funds (Eichenauer & Reinsberg 2017). Finally, in the same way as political institutions appeared to affect a government's decision to start an aid initiative, more research is warranted on whether political regime type also affects the quality of aid that is being provided (Faust 2008). Initial studies indeed suggest that the source of funding – originating from a more or less democratic donor —- matters for the effects of aid (Bermeo 2011, Isaksson & Kotsadam 2018). In times in which autocracies grow and countries experience autocratic reversals (or at least more vivid populist movements), this is a particularly relevant avenue for future research.

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II. Move on up – Electrification and Internal Migration

Bibliographic Information

This chapter is single-authored and has not been published elsewhere.

Abstract

This study uses the large scale roll-out of electric transmission infrastructure in Nigeria from 2009 to 2015 to quantify the effect of electrification on internal migration. I address endogenous location of electricity infrastructure by estimating effects on peripheral households not directly targeted by the policy in combination with instrumenting for the actual grid path by a hypothetical least cost grid. Results show an increase in individual migration propensity by 6 percent and a reduction of household size by 0.8 individuals, mainly driven by young adults and older teenagers. Theoretically, this result can be explained by rising household incomes with a coinciding lack of employment generation for this sub-population. Results from a gravity model of migration show a reduction in movement costs and a rise in migration to rural, electrified destinations following the electricity supply shock.

JEL: H54, J60, L94

Keywords: electrification, migration, agricultural production

II.1 Introduction

This paper analyzes the effect of a local electricity supply shock on internal migration. Investments in rural infrastructure are an important instrument to foster development without relying on urban centers as the sole engines of growth. Yet, little is known about the effect of efficiency gains from infrastructure investments on population dynamics. While local growth effects might reduce out-migration incentives (for instance, as documented for the United States in Lewis & Severnini 2020), a rise in incomes in a developing country context could also enable out-migration by overcoming credit constrains (McKenzie & Rapoport 2007, Bryan et al. 2014, Angelucci 2015, Bazzi 2017, Clemens 2020). This is relevant given that rural infrastructure investments are seen as an alternative to rapid urbanization, which in the case of Sub-Sahara Africa, is often an unplanned and uncoordinated process resulting in congestion, low connectivity and environmental pressures (World Bank 2016).

The context of this paper is Nigeria in the years 2009 to 2016, where conditions are favorable to expect large productivity gains from electrification. Access to modern electricity has a high priority on the global agenda, with nearly 1 billion people living without (IEA 2019), but the academic literature finds mixed results regarding its development effects (see Bayer et al. 2020, Lee et al. 2020*a*, for surveys of the literature). Large scale investments in transmission infrastructure and generating capacity along household connections are thought to produce the largest effects (Lee et al. 2020*a*). In addition, complementary factors such as pre-existing industries and market access are assumed to be crucial. For instance, Fetter & Faraz (2020) find a positive electrification effect only in regions that experience a simultaneous shock in demand for local commodities. In the case of Nigeria, the investments in electricity infrastructure were both large scale and in response to the wide gap between existing supply and demand.

Understanding the effects of electrification on population dynamics is particularly important in light of the large gap in productivity and standard of living between urban and rural areas across the developing world (Gollin et al. 2014, Young 2013). Many scholars see this gap as evidence that moving workers out of the agricultural (rural) sector into the more productive (urban) sector can create large productivity gains (Gollin et al. 2014, Bryan & Morten 2019). In addition, a high degree of unequal distribution of economic activity across space is associated with low levels of development (Alesina et al. 2016, Lessmann 2014). One solution is to reduce barriers to migration, as has been the focus of a growing body of research (Allen & Arkolakis 2014, Bryan & Morten 2019, Baum-Snow et al. 2020, Bryan et al. 2014, Angelucci

2015, Lagakos et al. 2018, Bryan et al. 2021, Bah et al. 2020). However, migration might not be desirable for everyone,[1] and can lead to unintended outcomes both at the sending communities (e.g., Baum-Snow et al. 2020) and the receiving urban centers (Henderson 2002). Thus, investing instead in rural infrastructure as means of fostering country-wide development and to close the rural-urban gap is a common strategy across the developing world. Whether these investments also slow down internal migration has political significance.

To analyze this question, I rely on data from Nigeria's General Household Survey which offers a rich geo-coded household panel that tracks households and individuals over time. For identification, I use a first-difference estimation conditional on state-wave fixed effects and a number of geographic controls. Endogenous allocation of the transmission infrastructure is addressed in two ways. First, I exploit the fact that transmission lines are large scale connections between two local substations that transport high voltages across long distances.[2] At the local substation, electricity is fed into the local distribution grid, which makes them both an important determinant of the grid locations but also a highly endogenous variable.[3] However, households located between two of these substations were not the ultimate target of the intervention. Yet, these households benefited greatly from the grid expansion, since distribution lines often follow the path of transmission lines to save costs. This approach builds on Faber (e.g., 2014), who estimates the effect of road construction in China on peripheral cities.

Second, I construct a hypothetical least cost path as an instrumental variable for the actual grid path. Given that the path of each transmission line is mainly dictated by the location of the respective substations, it is still possible that policy-makers use the little wiggle room they have to favor certain locations – be it for winning voters or for favoring the villages with the

[1]In an early research article, Sjaastad (1962) pointed out that migration comes with non-monetary costs, including the disutility from leaving *"familiar surroundings, family, and friends."* In a similar vein, Blanchard & Kirchberger (2020) muse that *"movement from rural to urban areas may involve loss of social connection or information insurance, or the loss of claims to land and other resources in rural areas. There may be barriers for rural people – particularly those who are older – in learning new kinds of work or new way of life."* These psychological costs are difficult to quantify and if sufficiently large could explain lower levels of observable migration than expected by theoretical models – without implying resource misallocation.

[2]Transmission lines constructed during the sample period measure on average around 100km.

[3]Notable attempts to exploit exogenous variation in substation location exist (Lipscomb et al. 2013), but they are sensitive to model assumptions and more credible for historical grid construction, than for the expansion of an existing grid.

highest economic potential. The least cost approach overcomes this concern by isolating supply side factors of infrastructure provision based on the costs of its construction given the characteristics of the terrain. This approach draws heavily on Faber (2014), while variations of the least cost approache find increasing applications in economics (e.g., Banerjee et al. 2020, Kassem 2020).

Results from first-difference and instrumental variable regression show that the electricity supply shock reduced household size by between 0.3 and 0.8 household members. This decrease is particularly driven by older teenagers aged 13 to 18, while household heads show no increase in migration propensity. At the individual level, migration propensity increased by 6 percent. Moreover, I find a significant increase in work-related migration by 30 percent for male adults, and of 12 percent for minors. These results seem linked to a combination of increased access to credit with limited job creation for the youth. While household income proxied in logarithmic food consumption increased by 23 percent, total working hours and employment outside subsistence agriculture increased only for the household heads, but not of other subgroups.

I complement this analysis with results from a gravity model of migration. These show that also at municipality level, migration flows increased after grid construction. What is more, the effect of movement costs on migration went down to approximately a third, in line with the existence of barriers to migration in the form of credit constraints. In addition, I find that migrants from municipalities that received a new grid were more likely to migrate to rural destinations that also just received a new electricity grid.

This paper is the first rigorous empirical analysis of the impact of electrification on internal migration in a developing country context. Previous studies have either focused on rich countries or applied less empirical rigor. Lewis & Severnini (2020) analyze the effect of the historical expansion of the electricity grid on internal migration in the United States. They find a significant positive effect on population linked to productivity gains in the agricultural sector. Fried & Lagakos (2021) construct a multi-sector spatial model that predicts a reduction of out-migration in electrified villages due to productivity gains. They offer empirical results from difference-in-differences estimation on Ethiopian villages in line with this prediction. However, a simple difference-in-differences estimation is likely to suffer from selection bias as outlined above.

In addition, my paper differs from previous studies by considering credit constraints in the theoretical predictions. While previous theoretical models focus on productivity effects (Lewis & Severnini 2020, Fried & Lagakos 2021), the existence of credit constraints might imply sub-optimal migration levels *ex ante* which are adjusted when incomes rise. This can ultimately lead to a net increase in out-migration. This theoretical prediction draws on literature about the income-migration relationship which has mainly focused on the effect of cash transfers (Bryan et al. 2014, Angelucci 2015, Molina Millán et al. 2020). These studies typically find a positive effect of alleviating credit constraints, particularly for poor households (see Adhikari & Gentilini 2018, for a survey of this literature). While these studies are useful to understand the isolated role of credit constraints, they do not tell us much about increasing the opportunity costs of migration by raising incomes at home. However, given the current policy debates on ways to slow down rapid urbanization, the question of opportunity costs is highly salient. Bazzi (2017) explores the effect of income shocks from variations in rainfall patterns in Indonesia and finds a positive effect on labor migration. While Bazzi's study is closely linked to this paper, short-lived income increases from rainfall shocks do not change incomes at home for more than one period and will therefore affect the opportunity cost of migration to a limited degree.

Finally, my study contributes to the wide literature on the effects of electrification. Most studies have focused on the effect of electrification on income, employment, health or education (Dinkelman 2011, Grogan & Sadanand 2013, van de Walle et al. 2013, Burlig & Preonas 2016, Lenz et al. 2017, Lee et al. 2020*b*). My results suggest that employment benefits from electrification do not occur homogeneously across sub-populations, particularly in an environment of high underemployment. This might explain why some studies tend to find small to no employment effects (Burlig & Preonas 2016, Lenz et al. 2017, Lee et al. 2020*b*) while others find large effects (Dinkelman 2011). In addition, investments in the migration of younger household members might not always be accounted for correctly in the assessment of household welfare and the lack thereof might obscure positive effects from electrification.

The remainder of this paper is organized as follows. Section 2 describes the context of the study; Section 3 discusses the data sources of the study; Section 4 describes the empirical strategy both at the household-level and at the grid-cell level; Section 5 presents the main results; Section 6 reports robustness tests; Section 7 reports results from the gravity model and finally Section 8 concludes.

II.2 The context

Nigeria's labor market is characterized by a lack of adequate earning opportunities. In 2011, the World Bank estimated that 53 million Nigerians between the ages of 15 and 64 were working, but half of them in low-productivity agriculture (World Bank 2016). Despite a moderate level of unemployment, household earnings are often not sufficient to meet basic needs such that a third of the population continues to live below the poverty line. The low earnings are caused by a general lack of labor demand in the formal wage sector. Most work is informal and either self-employed or for a family-owned business. High population growth and rising inequality across regions add additional stress to the labor market.

However, during the study period of this paper, sectoral transformation was already on its way. Spurred by macroeconomic growth, from 2007 to 2011, the share of employment in agriculture fell from 58 to 50 percent, with new jobs emerging in the private and public wage sector (World Bank 2016). Wage employment in agriculture is low, with only 1 in 20 workers being wage labor in 2011. In addition, the World Bank report finds that youth faces barriers to entering the labor market after completing education, potentially due to a mismatch between skills acquired at school and skills required for potential jobs.

This lack of adequate work, particularly for the youth, is one of the driving forces of internal migration. Using a migration census, Mberu (2005) show that on average 58.3 percent of Nigeria's rural-born population are migrants, meaning they reside in a different location than they were born. Of these, 37 percent are rural-urban migrants and 63 percent are rural-rural migrants, illustrating that rural-rural migration constitutes the main share of permanent migration. Amrevurayire & Ojeh (2016) find that in the Ughelli South Local Government Area of Nigeria, migration is highest for the age cohorts 15-25 and 26-35 years and decreases in age. Moreover, the authors identify unemployment, a search for education and a lack of basic infrastructure as the reasons for migration. In addition, Dillon et al. (2011) find that agricultural households use the migration of male household members to respond to negative income shocks. These findings suggests that improving earning possibilities and income diversification in remote areas should slow down migration.

While migration might be an optimal strategy for the individual household, outcomes for the sending communities are not always positive. A study in the Niger Delta region shows that rural out-migration leads to sizable labor shortages in the agricultural sector which results in incomplete harvest and foregone revenue (Ofuoku et al. 2017). This mirrors the findings of Baum-Snow et al. (2020) in China that an increase in migration can have detrimental effects on the economies of origin locations. Thus, migration is not only a result but also a driver of the increasing rural-urban gap.

Infrastructure development is an important component of Nigeria's rural development efforts. In particular, the electricity sector holds a crucial position given that increases in power generation capacity have been slow over the last three decades and have not kept track with economic and population growth (Gatugel et al. 2015). Nigeria's electricity consumption was in 2015 one of the lowest in the world with only 156 kWh per capita (World Bank 2017). Particularly rural areas are under-supplied, and the low level of electricity supply hampers productivity across sectors. What is more, it is estimated that the connected population more than half the time faces power problems (Sadiq et al. 2015). Many businesses rely on private electricity generators for production when grid electricity is unavailable or unreliable, raising their costs of production (Pestana et al. 2014).

To address these issues, the Electric Power Sector Reform Act from 2005 demanded the privatization of the entire power sector to create incentives for investments in generation and transmission infrastructure. Among other changes, the state-owned Power Holding Company had been unbundled into multiple entities. Since then, electric transmission has been managed by the Transmission Company of Nigeria (TCN) (NERC 2019), which immediately started to undertake efforts to improve grid supply.

In 2007, regional efforts to strengthen the coordination of the energy sector in the ECOWAS region led to the construction of a new transmission line in the South-West at Ikeja West substation and Sakete in Benin. In addition, in 2009 the World Bank committed a credit worth approximately 200 million US dollars for the power sector for the funding period 2009–2014. The proposed project consisted of the extension of the generation capacity, the expansion and rehabilitation of the transmission infrastructure and best-practice investments in distribution infrastructure (World Bank 2009). Out of this, 180 million US dollars are solely dedicated to the enhancement of the transmission and distribution grid. As a consequence, a number of major transmission lines were constructed between 2009 and 2015 in context of the World Bank funded Nigeria Electricity and Gas Improvement Project (NEGIP). These investments went along with major investments in generating capacity.

Rural electrification holds a high priority for Nigeria, reflected in the creation of the Rural Electrification Agency in 2005 which lists "driving economic development" as one of its policy objectives according to its website (Rural Electrification Agency 2021). The website elaborates that this goal consists in "empowering[ing] local industries to play a larger role in the supply chain from materials, manufacturing, construction and operation of the assets" – illustrating that the spatial redistribution of economic activity is an intended consequence of Nigeria's rural electrification efforts. While slowing down migration is not a declared objective, population dynamics are not likely to remain unaffected.

II.3 Data

In order to analyze how the expansion of the electricity grid affects productivity and migration, I rely on Nigeria's General Household Survey which was collected by the Nigerian National Bureau of Statistics in partnership with the World Bank Living Standards Measurement Study. While the General Household Survey was initiated in 2006, since 2010 it has been collected in a panel structure, following the same approximately 5,000 households over time, and forms a representative sample of the Nigerian population. This study uses 3 waves from the years 2009/2010, 2012/2013, and 2015/2016.[4] It provides detailed information on household consumption, income generating activities, agricultural plots owned by the household and information on each household member together with geographic coordinates.

Data on grid expansion and substation location comes from the Energy Database published by the Rural Electrification Agency of Nigeria (Rural Electrification Agency 2020). This database offers data on various indicators related to energy supply, including the exact location and electric tension of substations and main transmission lines as well as the year of construction of the latter.[5] A number of long distance transmission lines were constructed between 2009 and 2015 (Figure II.1). They typically measure more than 100 km in length and have a voltage of 132 kV or 329 kV.

My main definition of an electricity supply shock assumes all households affected that were within a 15 km distance of a new transmission line. Ac-

[4] A 4th wave was collected in 2018/2019, but the high degree of attrition from the original panel makes the data useless for the purpose of this study.

[5] For quality assurance, internet research was carried out to verify the construction year of each transmission line. Based on this the following adjustments were made: The transmission line between Dutse Substation and Azare Substation in Jigwara was originally coded as existing in 2000. An alternative source from the World Bank did not report this transmission line. Additional sources reported the construction year of Azare Substation to be 2010. Therefore, the construction year of this line was coded as 2010. The same World Bank map reported the line between Dutse and Kumbotso as existing in 2008, while the Energy Database reported the year of construction as 2010. In combination the wrong year from the neighboring line between Dutse and Azare, it seems that dates of these two lines were accidentally swapped when coded. Therefore, the construction year of the line between Dutse and Kumbotso was re-coded as 2008. The transmission line between Ihovbor and Okada was coded as existing in 2000 and changed to 2018, because the substation construction was found to be only finalized in 2018. The extension of the Odugunya substation was coded as 2010 and changed to 2018, because the additional substation was only created in this year.

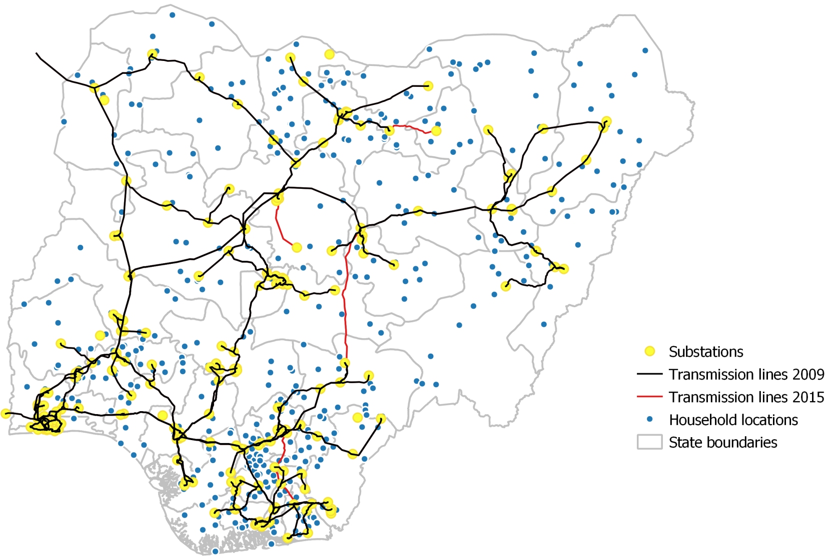

Figure II.1: Location of households, transmission lines and substations cording to this definition, 139 households experienced an electricity supply shock during the observation period (69 between wave 1 and 2 and 70 between wave 2 and 3). These were located across 10 of the 37 states of Nigeria.[6] These households can be interpreted as the treatment group. Households located in the same states that did not experience an electricity supply shock constitute the control group against which the treatment effect is estimated.

A balancing test on wave 1 observations shows that treatment and control households do not differ significantly across most baseline characteristics (Table II.1). On average, only 38 percent of the control group households were electrified in 2010. They were, on average, located 17.59 km away from the closest transmission grid line and 40.30 km from the closest substation. Treatment households were slightly less likely to be already electrified and were located slightly closer to any existing grid line and slightly more distant from any substation - but none of these differences reaches statistical significance. Importantly, also other geographic characteristics are balanced between both groups. Neither the distance to any major road or the state capitals differ significantly, nor does population density or the percentage of cropland and urban land. Given that the identification strategy relies heavily on geography, balancing of geographic characteristics makes it unlikely that differences in time trends across geographic locations bias the results.

However, the test shows a few statistically significant differences in household characteristics, particularly in building materials of the accommodation. Treatment households were more likely to have an iron roof and concrete walls and less likely to have a grass roof and unburnt or burnt brick walls. The differences are small and seem uncorrelated with wealth, since agricultural wages and production values do not differ significantly between groups. In addition, the test shows a 10 percent significance difference in the use of a diesel generator for lighting and the number of elderly household members. Overall, the significant differences between treatment and control households appear small enough to be driven by chance. A test of joint significance yields very large p-value of 0.998. Nevertheless, controlling for potential bias, I test all main regressions against the inclusion of all significant difference of the balancing test (Appendix Tables B.2.1 - B.2.3). In order to avoid problems of multicollinearity, these covariates are omitted from the main specifications.

[6]These 10 states are Abia, Akwa Ibom, Bauchi, Benue, Ebonyi, Enugu, Imo, Jigara, Kano, and Nasarawa.

Table II.1: Balancing between treatment and control households in 2009

		control	SD	treatment	SE	N
Electrified		0.380	0.485	-0.061	0.066	3,615
(Log) grid distance		9.775	1.317	0.182	0.249	3,632
(Log) substation distance		10.604	0.640	-0.050	0.146	3,632
(Log) distance to capital		11.003	0.776	-0.499	0.311	3,632
(Log) road distance		4.007	1.496	-0.287	0.272	3,632
% of cropland		0.426	0.317	0.011	0.027	3,632
Population density		2.109	3.129	-0.866	0.652	3,632
% of urban land		0.003	0.016	-0.000	0.001	3,632
Rural		0.824	0.381	-0.050	0.089	3,632
House value		975,496.668	2,743,567.399	310,147.835	245,754.801	2,867
Roof	Grass	0.209	0.407	-0.085**	0.041	3,611
	Iron sheets	0.702	0.457	0.103**	0.042	3,611
	Clay tiles	0.015	0.123	0.003	0.009	3,611
	concrete	0.010	0.099	0.005	0.014	3,611
	Plastic sheeting	0.009	0.093	-0.001	0.001	3,611
	Asbestos sheet	0.020	0.140	-0.002	0.002	3,611
	Other	0.035	0.184	-0.024	0.017	3,611
Walls	Grass	0.077	0.267	-0.010	0.024	3,603
	Mud	0.445	0.497	-0.059	0.064	3,603
	Compacted earth	0.035	0.184	-0.012	0.016	3,603
	Mud bricks (unfired)	0.065	0.247	-0.049**	0.021	3,603
	Burnt bricks	0.014	0.118	-0.015*	0.008	3,603
	Concrete	0.347	0.476	0.146**	0.070	3,603
	Wood	0.012	0.110	-0.001	0.001	3,603
	Iron sheets	0.004	0.061	-0.001	0.001	3,603
Lighting fuel	Collected firewood	0.092	0.289	-0.018	0.022	3,607
	Purchased firewood	0.031	0.174	0.001	0.017	3,607
	Kerosene	0.390	0.488	0.062	0.054	3,607
	Electricity	0.203	0.402	-0.029	0.039	3,607
	Generator	0.032	0.176	-0.014*	0.008	3,607
	Battery	0.210	0.407	0.010	0.047	3,607
	Other	0.042	0.202	-0.012	0.020	3,607
(Log) agri production value		10.366	3.145	0.162	0.351	2,767
(Log) daily wage, men		3.945	3.517	-0.242	0.226	1,821
(Log) daily wage, women		2.151	3.133	-0.368	0.234	1,542
# of paid workers (men)		2.393	5.567	-0.094	0.351	2,715
# of paid workers (women)		1.449	6.593	-0.391	0.275	2,715
# of plots		1.560	1.397	-0.075	0.261	3,632
HH size		5.900	3.016	0.088	0.335	3,632
# of elderly		0.075	0.311	0.044*	0.026	3,632
# of children		3.236	2.479	0.024	0.298	3,632

	F-stat	p-value
Test of joint significance	0.43	0.998

Notes: Balancing is tested using a regression with state fixed effects and standard errors clustered at the village-level as in the main regressions. (*** p<0.01, ** p<0.05, * p<0.1)

II.4 Empirical Strategy

II.4.1 Difference-in-differences estimation

I begin the analysis with a difference-in-differences estimation at the household level. Changes in the main outcome variables are explained by changes in the proximity to a new transmission line conditional on the distance to the closest substation, other geographic control variables and state-wave fixed effects. Algebraically, it takes the following form:

$$\Delta Y_{ijt} = \alpha \Delta D_{ijt} + \beta' X_{ij} + \gamma_{jt} + \epsilon_{ijt} \tag{II.1}$$

where Y_{ijt} is a vector of outcome variables at household i in enumeration area j at time t. My independent variable D_{ijt} is a dummy variable that takes a value of 1 if the household was located within a 15km distance of any newly constructed transmission line.[7] Alternatively, I run regressions using a continuous measure of the negative logarithmic distance to the closest new transmission line. The negative sign ensures ease in interpreting the results as done in similar studies (Lewis & Severnini 2020). X_{ij} are household specific time-constant geographic control variables which are outlined below. Most importantly, these include the distance to the closest electric substation. γ_{jt} are state-wave fixed effects. The error term ϵ_{ijt} is clustered at the enumeration area (which is in most cases equal to the village) to correct for correlated errors due to the sampling structure of the data. Households within a 10 km distance from any substation were excluded from the sample to control for the fact that these might have been directly targeted by the policy. In addition, 7 households were excluded from the dataset which migrated as a whole during the observation period, in order to satisfy the exclusion restriction.[8]

All regressions control for the distance to the closest electric substation. This accounts for the fact that substations were directly targeted by the policy, as nodes where electricity was fed into the low voltage distribution grid.

[7] The 15km buffer was selected based on first stage regressions that tested the correlation between distance to the transmission grid and household electrification.

[8] Household migration is very rare in this dataset. Overall in the survey, there were 45 households that moved during the sample period, but only 7 households moved in the treatment states. Due to the fixed effects structure of the main estimation strategy, this number is too low to analyze household migration quantitatively. When analyzing individual level migration, cases where the whole household migrated where excluded, because the identification strategy relies on geographic factors remaining constant. Given the exclusion of household migration, estimates from individual level migration therefore constitute a lower bound for total migration.

The locations are strategically chosen in areas of high electricity demand and are therefore highly endogenous. In addition, I present results before and after controlling for a number of additional geographic variables. In particular, these include distance to the respective state capital,[9] distance to the closest major road in 2009, population density within a 40 km buffer, percentage of cropland and percentage of urban land within a 40 km buffer. Details on data sources and metrics of the control variables can be found in Appendix Table B.1.1.

These are included to address concerns of non-parallel trends. Since geographic variables tend to be correlated, there is a risk of non-parallel trends based on geographic location when exploiting geographic variation in the main explanatory variable. For instance, Bensch et al. (2020) find that the instrumental variable for electrification in the seminal work of Dinkelman (2011) also predicts road access which could drive the results. The geographic controls of this paper reflect that locations might trend differently depending on their market access, political importance, urbanization rate and sectoral composition. However, the risk from non-parallel trends across geographies seems limited since the balance test (Table II.1) yields only small, statistically insignificant differences between treatment and control households.

At the individual level the regression takes a very similar form of:

$$\Delta Y_{cijt} = \alpha \Delta D_{ijt} + \beta' X_{ij} + \beta gender_{cij} + \gamma_{jt} + \epsilon_{cijt}, \qquad \text{(II.2)}$$

where Y_{cijt} are outcomes at the individual level, D_{ijt} is the respective measure of proximity to a newly constructed grid line, X_{ij} are geographic controls. At the individual level, I control for $gender_{cij}$ since employment and migration behavior is expected to differ greatly between genders. In addition, I run regression separately based on gender and age group or relationship to the household head. The relationship to the household head is relevant for the main outcomes. Every household member inhibits a different role based on social norms and is expected to contribute to a different degree to the household income.

II.4.2 Instrumental variable estimation

Estimating equations II.1 and II.2 by OLS would risk bias, if the path of the transmission lines was not assigned at random but followed economic and

[9] In some states, the state capital is not the most populated city. Due to the multicollinearity of both variables, I do not include both the distance to the state capital and the distance to the largest city in the same regressions. However, results do not depend on which of both measures is used.

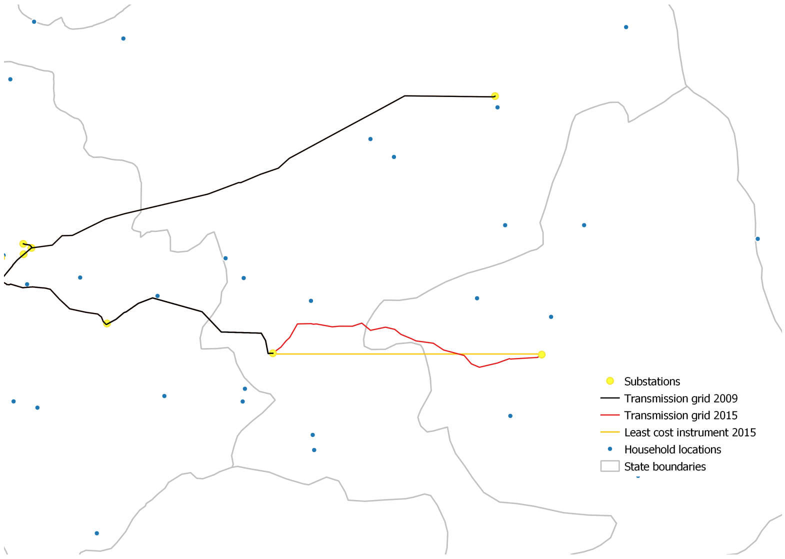

Figure II.2: Example of actual and least cost grid

political considerations. To address this concern, I implement the least cost path approach introduced in Faber (2014). This approach isolates supply-side factors of infrastructure provision. In particular, I determine for every new transmission line which path it should have followed in order to connect the terminal substations most cost-effectively. The construction costs are based on characteristics of the terrain that needs to be crossed. Following Faber (2014), I employ gridded land-cover data together with elevation data to measure land gradient. High construction costs are assigned to pixels with a high slope and to pixels that classify as urban areas, waterbodies or wetland.[10] Next, the algorithm selects a path to connect the terminal substations that results in the lowest construction costs. A detailed description of this method can be found in Appendix B.3.

Figure II.2 shows a visual illustration of this approach for the states Jigawa and Bauchi. The new grid line that was actually constructed is approximately concave, while the hypothetical least cost grid is in this case simply a straight line. The difference between both paths suggests that actual grid construction was biased and favored households in the North.

The least cost grid is then used to instrument for the treatment variable D_{ijt} of equations II.1 and II.2. The two-stage least squares version of equation II.1 takes the following form:

[10] This simple algorithm adopted from Faber (2014) finds its original motivation in the transport engineering literature (Jha & Schonfeld 2001, Jong & Schonfeld 2003).

$$\Delta Y_{ijt} = \alpha \Delta \hat{D_{ijt}} + \beta' X_{ij} + \gamma_{jt} + \epsilon_{ijt}. \tag{II.3}$$

First stage equations:

$$\Delta D_{ijt} = \alpha \Delta L_{ijt} + \beta' X_{ij} + \gamma_{jt} + \epsilon_{ijt}, \tag{II.4}$$

where $\hat{D_{ijt}}$ are the fitted values of proximity to the actual grid and L_{ijt} indicates proximity to the hypothetical least cost grid.

First stage results are reported in Table II.2 Panel B. Columns (3) and (4) report results for a continuous measure of proximity to any new transmission line, while columns (7) and (8) report results for a dummy variable that turns 1 if the new grid (and the hypothetical least cost grid respectively) was within 15 km proximity. All specifications yield very similar estimates of 0.855 to 0.866 points that are highly statistically significant. Kleibergen-Paap F-statistics indicate that the continuous measure of grid proximity leads to higher statistical power, but also the binary measures result in large F-statistics of 55.66 (66.89 respectively). Besides being a strong instrument, the exclusion restriction appears to be satisfied. Proximity to the least cost grid only affects outcomes via proximity to the actual new grid. It would be violated if proximity to the least cost grid correlated with other factors such as sectoral composition that lead to different trends in treatment locations. This seems less likely given the main regressions already control for a number of geographic covariates. In addition, I conduct a series of robustness tests to test the validity of the exclusion restriction.

First, I include baseline covariates that showed significant differences between treatment and control households in the balance test of Table II.1. Second, I test the main results against a specification with household fixed effects that relies on even weaker assumptions than the first-difference regression. Third, I run a placebo test on future grid lines. If grid locations trended differently from non-grid locations, proximity to future grid lines (instrumented by their hypothetical least cost paths) should lead to similar effects as the actual grid. Finally, I test against variation in proximity to road infrastructure.

II.5 Main Results

II.5.1 Electricity

Since grid expansion is only a crude measure of electricity access, I first test whether the new transmission lines increased local electrification. Low

Table II.2: The effect of new transmission lines on household electrification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	- (Log) grid distance				Dummy grid			
	OLS		2SLS		OLS		2SLS	
Panel A: Main regression								
INDICATOR	0.057*	0.055	0.108***	0.104***	0.180**	0.177**	0.539***	0.536***
	(0.034)	(0.033)	(0.039)	(0.040)	(0.085)	(0.084)	(0.139)	(0.153)
Substations	-0.029	-0.037	-0.033	-0.034	-0.028	-0.036	-0.034	-0.031
	(0.029)	(0.032)	(0.033)	(0.033)	(0.028)	(0.030)	(0.036)	(0.036)
Capital		0.012		0.002		0.012		-0.013
		(0.018)		(0.020)		(0.017)		(0.022)
Road distance		0.003		0.004		0.002		0.000
		(0.006)		(0.006)		(0.006)		(0.008)
% Cropland		0.071		0.077		0.065		0.066
		(0.081)		(0.082)		(0.080)		(0.084)
Population density		0.005		0.006		0.006		0.009
		(0.006)		(0.006)		(0.006)		(0.006)
% Urban		1.571		1.213		1.452		0.385
		(6.115)		(6.190)		(5.985)		(6.100)
Observations	2,289	2,289	2,289	2,289	2,289	2,289	2,289	2,289
R-squared	0.029	0.032			0.033	0.036		
Panel B: First stage results								
Least cost IV			0.8633***	0.8551***			0.866***	0.863***
			(0.0512)	(0.0477)			(0.106)	(0.116)
Substations			0.2059*	0.1739*			0.037	0.035
			(0.1130)	(0.0988)			(0.038)	(0.036)
Capital				0.0667**				0.020
				(0.0299)				(0.021)
Road distance				-0.0092				0.008
				(0.0115)				(0.007)
% Cropland				-0.1836				-0.020
				(0.1150)				(0.041)
Population density				-0.0197*				-0.010**
				(0.0110)				(0.005)
% Urban				-0.4542				-1.269
				(7.1669)				(2.421)
F-stat			284.3	321.3			66.89	55.66

Notes: All regression control for wave-state fixed effects. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

reliability of the electricity network and high costs often undermine demand (Lee et al. 2020*a*). Grid expansion affects electrification both at local farms, businesses, and private households. However, the data allow only to test for household electrification. While not the only channel, this offers suggestive evidence of changes in local electricity use.

Table II.2 reports results of grid expansion on household electrification status for the continuous and the binary treatment measure using both the observed grid and the hypothetical least cost grid. All specifications show a positive correlation between the treatment variable and household electrification. This effect is larger for regressions that rely on the hypothetical least cost grid, implying that actual grid location might have favored economically prosperous regions that were already better supplied with electricity. The binary treatment measure shows much larger effects than the continuous measure, implying that electrification benefits from new transmission lines fall to 0 after a certain distance threshold. Therefore, the binary indicators form my preferred treatment measure.

Households located within a 15 km buffer of a new transmission line increase household electrification by 18-54 percent. In all specifications, control variables show no significant effects on changes in household electrification. This suggests that time trends in household electrification are mainly driven by grid construction, i.e., supply side effects. Demand side factors such as urbanization seem to only matter as long as they lead to new grid construction. In addition, I analyze the effect of the new transmission lines on household fuel choices (Appendix Table B.1.2). Results from the preferred specification show that grid expansion led to a 26 percent increase of electricity, a 31 percent reduction of kerosene use and a 17 percent reduction of battery use as main lighting fuel. The finding underlines that grid expansion created an economically relevant shift in local electricity supply.

II.5.2 Migration

Table II.3 reports the effect of the electricity supply shock on household composition. The table shows OLS and 2SLS results with and without geographic control variables. Across specifications, the electricity supply shock reduced the number of household members by between 0.33 to 0.78 persons. This effect is large given that the average household consisted of approximately 6 persons. The effect seems to be partly driven by children. The number of older teenagers aged 13 to 18 went down by 0.14 to 0.335 individuals. Since teenagers in Nigeria often enter the workforce at age 15, this could be both education or work related migration. In either case, given the high unemployment rates among Nigerian youth, migration of this age group is probably

Table II.3: The effect of new transmission lines on household composition

	(1)	(2)	(3)	(4)	(5)
	Baseline	OLS		2SLS	
	mean	no controls	controls	no controls	controls
# of household members	5.963	-0.330**	-0.350**	-0.691***	-0.778***
		(0.140)	(0.150)	(0.196)	(0.226)
# of elderly	0.071	-0.061*	-0.060*	-0.038	-0.045
		(0.033)	(0.034)	(0.032)	(0.036)
# of children (total)	3.259	-0.300***	-0.329***	-0.506***	-0.576***
		(0.102)	(0.099)	(0.148)	(0.142)
# of children (age 0-5)	1.176	-0.207**	-0.222**	-0.174*	-0.259**
		(0.093)	(0.096)	(0.096)	(0.099)
# of children (age 6-12)	1.301	0.064	0.053	-0.020	0.003
		(0.071)	(0.070)	(0.102)	(0.102)
# of children (age 13-18)	0.802	-0.137	-0.137	-0.335**	-0.335**
		(0.089)	(0.089)	(0.133)	(0.135)
Observations		2,259	2,259	2,259	2,259
F-stat				66.430	55.295

Notes: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

linked to a household investment in the young member of the household that was previously impossible due to credit constraints. At the same time, the result implies that the productivity shock did not raise employment potential for young people so much that staying and pursuing wage employment would, on average, be preferred over migration. In addition, the number of young children below the age of 5 decreases by 0.17 to 0.26 individuals.

Turning to results at the individual level, grid construction increased individual migration propensity by 5.6 percent in the preferred specification (Table II.4). The effect is smaller and statistically insignificant when using the actual grid path, suggesting some bias in the way the actual grid path was selected. At the individual level, I distinguish between the role of the household member within the household, such as household head, spouse etc. Assuming decision-making at the household level, this creates more homogeneous subgroups than grouping by age and/or gender. When analyzing these groups separately, an interesting pattern emerges. Across all specifications, the migration of the household head is not affected by the productivity shock. This was expected given that migration of whole households was rare and household head migration would typically imply migration of the whole household. The subgroup that mainly showed an increase in migration propensity is the group of children of the household head. Their likelihood to migrate increased by between 10 percent in the preferred specification.

Table II.4: The effect of new transmission lines on migration (individual level)

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
All HH members	0.019	0.015	0.016	0.054*	0.056*	15,993	100.701
		(0.016)	(0.017)	(0.027)	(0.028)		
HH head	0.003	0.005	0.006*	0.011	0.012*	2,716	68.260
		(0.003)	(0.003)	(0.007)	(0.007)		
HH spouse	0.035	-0.018	-0.019	-0.057**	-0.058***	2,536	96.434
		(0.017)	(0.016)	(0.024)	(0.020)		
HH child	0.091	0.030	0.030	0.099**	0.102**	9,338	102.018
		(0.023)	(0.024)	(0.041)	(0.043)		
HH grandchild	0.159	0.103	0.194**	0.023	0.210*	564	164.612
		(0.089)	(0.086)	(0.074)	(0.110)		
Other	0.180	0.058	0.045	0.130	0.163	828	43.684
		(0.081)	(0.083)	(0.233)	(0.238)		

Notes: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

This finding is in line with the results from Table II.3. However, in this table children of the household head are not defined by age, so this group includes also young adults. The oldest 25 percent of this group are aged 17 to 37. This provides additional evidence for households investing newly gained resources in the migration of young household members. For spouses of the household head, migration propensity decreased by 5.8 percent in the preferred specification. An interpretation of this result will be discussed below.

To get a clear picture of the migration surge, it is crucial to understand the motives behind out-migration. Given that the rise in out-migration is mainly driven by older teenagers and young adults, work is not the only possible motive. In addition, migration could be linked to a pursuit of education. In both cases, however, the expected returns to migration, must have exceeded expected returns from staying. Appendix Table B.1.3 reports results on an analysis of migration reasons among the sample of migrants. Since this analysis is performed on the sample of migrants, it does not achieve very high statistical power, but seems nevertheless informative. For this analysis, the sample is grouped by gender and age to keep the sub-samples as large as possible. For both adult male migrants and under-aged migrants there is an increase in work-related migration after the productivity shock. This confirms the theoretical expectation that earning potential at the destination is one of the main pull factors of migration. For adult men, the migration motive "for work" increases by 30 percent relative to other reasons and is significant at the 5 percent level. For under-aged household members, this motive increases by 12 percent, again significant at the 5 percent level. This suggests that work related migration is at least partly responsible for the increase in migration among older teenagers which is visible in Table II.3.

In addition, the migration of children seemed to be driven by the reason "to join family". This category increased for under-aged migrants by 33 percent (significant at the 5 percent level) and is consequently much larger than the increase in the migration motive "for work". Without additional details, the answer "to join family" is difficult to interpret. Possibly the rise in the earnings potential of the adults of the household increased the opportunity cost of child care to such a degree that relatives were charged with this task. This explanation would be in line with the decrease in young children below 5 observed in Table II.2 which is most likely not work- or education-related.

Finally, the results from Appendix Table B.1.3 offer some insight into the reduction of spousal migration. Among female adults, the migration reason "divorce/separation" went down by 13 percent and the effect is significant at the 5 percent level. This is in line with the general notion that divorces rise with economic pressures.

II.5.3 The employment channel

Table II.5: The effect of new transmission lines on employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-farm work		Farm work		Working hours		Obs	F-stat
	OLS	2SLS	OLS	2SLS	OLS	2SLS		
All	0.002	-0.011	-0.040	-0.012	-0.488	1.329	12,808	146.481
	(0.019)	(0.019)	(0.045)	(0.071)	(1.670)	(1.822)		
HH head	0.075**	0.121**	0.001	-0.008	4.870**	10.072***	2,696	68.115
	(0.031)	(0.048)	(0.069)	(0.076)	(2.215)	(2.868)		
HH spouse	0.000	0.025	0.010	0.084	-3.379	5.541	2,387	92.864
	(0.065)	(0.066)	(0.065)	(0.141)	(5.032)	(5.393)		
HH child	-0.026*	-0.046**	-0.052	-0.012	-0.476	0.356	6,808	197.905
	(0.014)	(0.019)	(0.069)	(0.101)	(1.816)	(2.945)		
Other	0.084	-0.012	-0.055	0.204	1.116	-1.081	594	1,469.557
	(0.060)	(0.133)	(0.097)	(0.252)	(3.234)	(4.109)		

Notes: All regressions control for wave-state fixed effects and the full set geographic covariates. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Next, I analyze the impact on employment and productivity as channels of the migration effect. Table II.5 reports individual level employment effects. While on average, across all household members, there is no significant effect on employment, there is a significant effect on the employment of the household head. The study distinguishes here between non-farm and farm work to reflect the fact that not unemployment is the major challenge for Nigeria's labor market, but underemployment and deadlocked employment in subsistence agriculture. The variable farm work comprises all cases of work on a family-owned farm. Non-farm work comprises all types of wage work or self employment, including wage employment in agriculture. For the household head, non-farm work increased by between 7.5 and 12.1 percent, while farm work remained unaffected by the productivity shock. In addition, working hours of the household head increased by between 4.9 and 10 hours. For their spouses, the likelihood of employment and the total working hours seem largely unchanged. This inelastic response to spousal employment is probably linked to traditional expectations about gender roles. Finally, the likelihood that children of the household head are working in non-farm employment decreased by 2.6 to 4.6 percent. These findings confirms the expectation that employment opportunities did not emerge equally for all subgroups. Older teenagers and young adults did not seem to benefit from the increase in labor demand experienced by household heads. While the productivity shock increased access to credit for the household, it did not increase the opportunity costs of migration of this subgroup to a relevant degree. In addition, the negative effect suggests that previously undesirable employment of children was now stopped.

To understand where new employment was generated, I analyze the sector of employment in Appendix Table B.1.4. It provides suggestive evidence of sectoral transformation. At the baseline 25 percent of the sample population worked in agriculture as their primary sector of employment, 5.9 percent worked in retail and manufacturing and personal services employed 2 percent respectively. When using the full sample (column (2)) the results for most sectors are close to zero. Employment in agriculture diminished by 10 percent, but fails to reach statistical significance. Average employment in retail increased by 3.6 percent and employment in transport by 1 percent, though these estimates reach only 10 percent significance.

Analyzing the sub-groups of household members reveals some nuance. In particular, there were positive employment effects for the personal services sector and the retail sector. The electricity supply shock increased the employment of the household heads in retail by 11 percent (at 10 percent significance) and by 20 percent for their spouses (at 1 percent significance). In addition, employment of the household head in personal services rose by 5.6 percent (at 1 percent significance) and employment in transport rose by 3 percent (at 10 percent significance). Agricultural employment of the household head fell on average by 9 percent, but does not reach statistical significance. For their spouses, employment in agriculture fell by a similar magnitude (11 percent), again without reaching statistical significance. Moreover, spousal employment in retail increased by a highly significant 21 percent. Most other sectors seem unaffected for spouses. Since the fall in agricultural employment of spouses is smaller than the rise of their employment in retail, it appears that spouses partly moved out from unemployment or under-employment into employment in the retail sector. For children of the household head, we can also observe a statistically insignificant reduction in agricultural employment of 7.6 percent, while the other sectors seem unaffected. In addition, there is a 9 percent reduction of grandchildren's employment in the personal services sector and a negative coefficient on agricultural employment.

Table II.6: The effect of new transmission lines on agricultural production

		(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
Output	(Log) production value	10.121	0.203	0.101	0.532	0.545	1,876	36.634
			(0.518)	(0.505)	(0.913)	(0.910)		
Factor inputs	(Log) labor costs	1.436	0.048	0.015	0.939**	0.856**	1,885	40.376
			(0.460)	(0.464)	(0.387)	(0.364)		
	(Log) # of paid workers	0.600	-0.092	-0.092	0.066	-0.002	1,885	40.376
			(0.094)	(0.088)	(0.149)	(0.150)		
	# of plots	1.784	0.166	0.212	0.680*	0.766**	2,323	55.700
			(0.258)	(0.263)	(0.338)	(0.357)		
Profit	(Log) food consumption	4.011	0.081**	0.082**	0.257***	0.271***	2,250	55.241
			(0.039)	(0.038)	(0.074)	(0.085)		

Notes: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

To understand the employment effect, I next analyze the effect on productivity. Table II.6 presents the results on the productivity of the agricultural sector for which data were readily available in the GHS panel. For agricultural production, there is a significant increase in inputs, in the form of costs of agricultural laborers and plots per household. Labor costs rose by approximately 85 percent in the preferred specification, while the number of wage laborers remained constant, suggesting an increase in agricultural wages. The number of agricultural plots per household increased by 0.76 units. This implies an efficiency gain in agricultural production, since the ratio of workers per lot decreased. What is more, it suggests that the rise in wages was no pure price effect. Surprisingly, the value of agricultural production did not increase to a statistically significant degree. This seems partly driven by poor data quality because the measure shows very large standard errors. In addition, it could mean that rising labor demand in other sectors created a labor shortage in agriculture, leaving harvest incomplete as observed by Ofuoku et al. (2017) as a consequence of migration in the Niger-Delta Region of Nigeria. Finally, I proxy household income by logarithmic food consumption per capita which increased by between 8 and 27 percent.

It is therefore evident that the productivity shock had a positive impact on the household's earnings and in turn extended their credit line. Overall, these results suggest that productivity gains in the agricultural sector might have freed time - particularly of the household head - to follow other income generating activities.

II.6 Robustness

II.6.1 Additional baseline controls

In order to address the concern of non-parallel trends between treatment and control households, I test the main results of Tables II.2, II.3, and II.4 against the inclusion of additional baseline controls. The balance test discussed in section II.3 shows only marginal differences in most baseline controls between both groups, therefore a violation of the parallel trends assumption is not likely. Statistically significant differences appear for building materials of roof and walls, main lighting fuel and the number of elderly household members. In Appendix Tables B.2.1 – B.2.3, I present replications of the main results while controlling for these baseline covariates. Results of the effect on household composition, individual migration and agricultural production do not change substantially after the inclusion of additional baseline control variables.

II.6.2 Individual-level fixed effects

Next, I test the main results against an alternative specification using unit fixed effects instead of first-differences. Controlling for the impact of time-constant geographic covariates is algebraically more simple in the first-difference approach. In addition, the first-difference estimator is less sensitive to the strict exogeneity assumption in short panels (Wooldridge 2010). Thus, comparing the main results to the fixed effects result provides some indication about the presence of bias. The fixed effects equivalent of equation II.1 reads:

$$Y_{ijt} = \alpha D_{ijt} + \beta' X_{ij} \times wave_t + \mu_i + \gamma_{jt} + \epsilon_{ijt} \tag{II.5}$$

where μ_i indicates household fixed effects and the time-constant geographic control variables X_{ij} are interacted with the respective wave $wave_t$ to produce a similar specification to the first-difference estimation.

Results of this exercise are presented in Appendix Tables B.2.4, B.2.5, and B.2.6. Neither the point estimates nor the standard errors differ greatly between the fixed effects and the first-difference specification. F-statistics of the instrumental variable approach are however smaller by approximately a factor of 0.5. For household composition, results are qualitatively the same, but the effect for older children aged 13 - 18 loses statistical significance, caused by a slightly larger standard error and a slightly smaller coefficient. At the individual level, the results confirm a positive effect on migration propensity on average and on the children of the household head, as well as a negative effect on migration propensity of their spouses. For agricultural production, the results confirm a positive effect on household food consumption. For the other outcomes, however, results differ to a relevant degree. While the effect on labor costs is positive, it is smaller than the first-difference estimate (0.262 compared to 0.856) and not statistically significant. The same applies to the number of plots (0.083 compared to 0.766). Given that the fixed effects approach is more sensitive to bias, the first-difference results for these two indicators should come closer to the true effect.

II.6.3 Future grid lines

Finally, I test the conditional exogeneity of grid locations to the main outcomes by regressing them on future grid lines. If grid lines are conditionally exogenous to the main outcomes, future grid lines should have no effect on the main outcomes.

Data on future grid lines come from the same data set as actual grid lines (Rural Electrification Agency 2020). Future grid lines were planed for the years 2018, 2020 and 2025. I code grid lines planned for 2018 and 2020 as occurring between the first and the second, and grid lines planned for 2025 as occurring between the second and the third wave of the household sample. Then I replicate Tables II.3 – II.5 using future grid lines instead of actual grid lines. Results are presented in Appendix Tables B.2.7, B.2.8, and B.2.9. Future grid lines have no statistically significant effect on household composition (Appendix Table B.2.7). The coefficient on household size is negative – as is the effect of actual grid lines – but very small at a reduction of 0.18 persons (compared to 0.78 persons in the main results). For the group of older teenagers between the ages of 13 to 18, the point estimate is even smaller, decreasing by 0.01 persons. This makes it unlikely that the negative effect of the main estimates on migration is purely driven by unobserved characteristics of the grid locations.

At the individual level, future grid lines show no effect on average nor the subgroups of the spouses, the children and the grandchildren of the household head. The results show a small and weakly significant effect on the migration of the household head. The latter seems negligible given its small size and the lack of significant effects on the other subgroups.

Results on agricultural productivity show no effect on most indicators, except for household food consumption. The latter is however substantially smaller than the coefficient of the main results (Appendix Table B.2.9).

II.6.4 Road construction

While the use of the least cost path instrument addresses demand side factors of electrification, it cannot solve the issues that cost assessments for the construction of other types of infrastructure would favor the same location. This would bias the results if other types of infrastructure were constructed during the treatment locations during the same time period.

This concern can be addressed by directly controlling for the construction of alternative types of infrastructure. Road infrastructure is the most obvious suspect for an omitted variable bias, since the costs of construction are determined by very similar features. To test for a potential bias from road construction, I run regressions on main outcomes controlling for all primary and secondary roads constructed during my sample period. During the time period of my study, the government implemented a large federal road maintenance program that resulted in a number of restored primary and secondary roads. Data on the date of constructions stems from publicly available materials by the Nigerian Federal Road Maintenance Management Agency (FERMA). I combine information on newly constructed or restored roads with their current geographic locations based on OpenStreetMaps (OpenStreetMap 2020). I then define a binary road treatment variable as being within 15 km of a newly constructed road – similar to the definition of the grid treatment variable.

Next, I replicate Tables II.3, II.4 and II.6 while controlling for the road treatment variable. Results are presented in Appendix Tables B.2.10, B.2.11 and B.2.12. to allow for easy comparison with the main results, Panel A in each table shows a replication of the respective main results table while controlling for road construction. Panel B of each table shows the coefficient of the road construction variable from the same regression as Panel A. Across all three tables, point estimates of grid treatment hardly differ from their original results in original specifications of tables II.3, II.4, and II.6. This shows that road construction and grid construction did not happen in tandem during the observation period. A possible explanation might be that the electricity grid and the road network are managed by separate ministries and in each case different donors were involved.

The regression results show no effect of road construction on household composition. At the individual level, the coefficient for the average household member even shows a negative significant effect of -4 percent. This is a relevant finding since there is limited evidence on the effect of road construction of migration dynamics. Baum-Snow et al. (2017) and Baum-Snow et al. (2020) build the exception but find a positive effect on migration. This invites further research into the mediating factors that exclaims the diverging results in the case of Nigeria.

For agricultural production, the effect of roads shows a negative and highly significant coefficient on the agricultural production value. Moreover, household food consumption decreases slightly by 10 percent (significant at the 10 percent level). This shows that new roads affect the main outcome variables completely differently, making an omitted variable bias unlikely.

II.6.5 Media use

It is possible that media access caused omitted variable bias. Media access could have increased because the related infrastructure was constructed during the same time or because access to electricity made device ownership more attractive. Previous studies have shown that access to mobile phones increases seasonal migration and remittances by reducing information frictions (Aker et al. 2011, Batista & Narciso 2018). In contrast, access to private television has been linked to reduced internal migration (Farré & Fasani 2013). I, therefore, regress ownership of media devices on grid expansion. Results show a statistically significant increase in TV ownership of 17 to 18 percent (Appendix Table B.2.13). This is probably due to the rise in income rather than an expansion of the television network. However, it can not be discarded that by wider use of television information friction was reduced. Since this should lead households to correct their expected returns from migration downwards, it should not be a concern for the quality of my main results. In addition, the estimate of internet usage shows a statistically significant negative effect. In the first wave, only 4 households owned an internet connection. In the previous waves, internet ownership increased in both the control and the treatment group but remained low. Therefore, the negative point estimate is unlikely to be causal. Importantly, I do not find a statistically significant change in mobile phone ownership. Therefore, the increase in migration is unlikely to be caused by improved connectivity between locations.

II.6.6 Mobile network coverage

Finally, I test for omitted variable bias from mobile phone infrastructure. As outlined above, infrastructure investments often happen in tandem. While mobile phone ownership shows no increase over the sample period, it is possible that improvements in mobile phone signal drive the effect. To test this, I use data on the 3G mobile phone network from the Collins Bartholomew – Mobile Coverage Explorer (Collins Bartholomew 2021). These data offer annual shapefiles for the area covered by the mobile networks. The observation period saw the introduction of the 3G network in Nigeria, which greatly reduced information frictions (Aker et al. 2011, Batista & Narciso 2018). I replicate Tables II.4 – II.5 controlling for a dummy variable indicating whether a household location was within reach of the 3G mobile network. Results are reported in Appendix Tables B.2.14 - B.2.16. In each table, Panel A reports the respective point estimates for the grid dummy and Panel B reports the corresponding point estimates for the 3G mobile network dummy from the same regressions. The introduction of the 3G dummy control variables affects the size of the main results only slightly. Point estimates for the mobile network dummy show a different pattern than the grid dummy, making it unlikely that mobile phone access drives the main results. The only indicator of the main results that just loses statistical significance is the number of children aged 13-18, while its effect size and standard error remain very close to the main results. Since the remaining outcomes remain statistically significant including the migration propensity of the children of the household head, this does not affect the main conclusions.

II.7 Gravity model

This section uses a gravity model of migration to analyze how the electricity supply shock affects dyadic migration patterns. Following the convention in the literature (Bryan & Morten 2019, Blanchard & Kirchberger 2020), I construct a directional dyadic mobility measure as follows:

$$m_{odt} = \frac{i_{odt}}{i_{ot}} \times 100, \tag{II.6}$$

where i_{od} is the number of individuals that were reported to have moved from origin district o to destination district d at time t and i_o the number of individual that where reported to reside in origin district o at time t. On average, the mobility measure m_{odt} is 0.01 percent, since 99.77 percent of the dyadic flows are 0 (1,000,507 out of 1,002,850 observations). For non-zero flows, the average is 4.03 percent. Aggregated over all potential destinations, 6.35 percent of a municipality's population moves to another municipality in every wave. To interpret the mobility measure correctly, a few features of its construction need to be considered. First, the measure captures only migration that happened since the last survey wave, i.e., within the last 3 years. Other studies often focus on lifetime migration and find substantially larger numbers. For instance, an older estimate for Nigeria by Mberu (2005) assumes that 58.3 percent of the rural-born population are living as migrants in 1993. This number includes migrants that eventually return to their home location. As Lucas (2021) notes, a substantial share of African migrants returns within 5 years of leaving their origin destination. A study by Bryan & Morten (2019) finds a somewhat smaller number for Indonesia, where on average of 35.8 percent of the population migrate during their lifetime. Second, it does not include within-district migration. While this number can be expected to be sizable, the measure is by construction ignorant to this type of migration. Particularly, moves from rural or peri-urban areas to the closest urban centers are not captured in the measure. Third, the measure is ignorant about the permanence of a migration move. The questionnaire simply asks respondents whether or not a household member currently resides with the household. Thus, some share of seasonal migration will be contained in the measure.

Following Bryan & Morten (2019) I run regressions of the form:

$$log(m_{odt}) = \gamma_o + \gamma_d + \gamma_t + \beta Grid_{ot} + \delta log(dist_{od}) + \lambda X_{dt} + \epsilon_{odt} \qquad \text{(II.7)}$$

where γ_o are origin fixed effects, γ_{dt} are destination fixed effects, γ_t are year fixed effects, $Grid_{ot}$ is a dummy variable that indicates new grid construction at origin district, $dist_{od}$ is the distance between origin and destination district, X_{dt} is a vector of destination characteristics in year t and ϵ_{odt} is the error term.

The destination characteristics include the percentage of the land area of the destination district covered in cropland and the percentage covered in urban land as a proxy for urban/rural characteristics of the location. Data on cropland and urban land comes from the Climate Change Initiative Land Cover Maps dataset (CCI-LC) by the European Space Agency (European Space Agency 2019). The data provide annual global land cover information for 22 different land cover categories defined by the UN Land Cover Classification System at a spatial resolution of $300m \times 300m$. I define every pixel as agricultural area that is classified as “cropland, rainfed” or “cropland, irrigated or post-flooding” in the CCI-LC dataset, while “urban” constitutes an existing class in the dataset. Due to the fixed effects structure of the regressions, the estimates refer to changes in rural (or urban) area, respectively. In addition, I include a dummy variable for grid construction at destination d in year t. Origin and destination municipalities are coded as receiving a grid in year t if one of the new transmission lines intersects with the boundaries of the administrative area. A balancing test between municipalities that received a new grid and those that did not finds no significant difference in road density, population, cropland and urban area (Appendix Table B.1.5).

Table II.7: Gravity model estimates – effect on (log) migrants (pooled)

Dependent variable = $log(m_{odt})$	(1)	(2)	(3)	(4)	(5)
$Grid_{ot}$		0.001**	0.003**	0.003**	0.001**
		(0.001)	(0.001)	(0.001)	(0.001)
$log(dist_{od})$	-0.007***	-0.007***	-0.014***	-0.014***	-0.007***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
$\%\ Cropland_{dt}$			-0.232***		
			(0.033)		
$\%\ Urban_{dt}$				0.211***	
				(0.023)	
$Grid_{dt}$					-0.002
					(0.001)
Destination FE			x	x	x
Origin FE		x	x	x	x
Wave FE			x	x	x
Destination-Wave FE	x	x			
Origin-Wave FE	x				
Observations	1,001,556	1,001,556	498,493	498,493	1,001,556

Notes: Standard errors are two-way clustered at the year and municipality level and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table II.7 represents average effects from all origin-destination pairs, while Table II.8 presents a sample split between origins with a new grid and those without. Column (1) of Table II.7 shows the average effect of the distance variable in my sample. On average, a 1 percent reduction in migration costs in the form of distance between two municipalities results in an approximately 0.01 percent increase in migrants. This relatively small effect is partly driven by the fact that dyadic migration flows are on average only 0.01 percent of the origin location. The effect amounts to approximately 2.5 times the standard deviation of the dependent variable. For this reason, the effect is sizable. In addition, it could indicate that migration costs are not the main barrier to migration in Nigeria. The results on grid construction at the origin municipality are qualitatively in line with previous results from the household panel. After grid construction, the out-going migration flow increases significantly by 0.001-0.003 percent. The small effect size amounts to between 0.4 to 1 standard deviation of the dependent variable and seems therefore relevant. Percentage of cropland and percentage of urban area of the destination municipality have large effects on migration flows. An increase of cropland by 1 percent reduces out-going migration by 0.23 percent, an increase of urban land by 1 percent increases out-going migration by 0.21 percent. Grid construction at the destination has on average no significant effect on migration flows.

Table II.8: Gravity model estimates – effect on (log) migrants (sample split)

	(1) $Grid_{ot} = 0$	(2) $Grid_{ot} = 1$	(3) Difference (2) - (1)
Panel A: Heterogenous effect of cropland			
$Log(dist_{od})$	-0.0173***	-0.0054***	0.0120***
	(0.0007)	(0.0007)	(0.0010)
$\%\ Cropland_{dt}$	-0.2794***	-0.0938**	0.1856***
	(0.0419)	(0.0418)	(0.0592)
Panel B: Heterogenous effect of urban land			
$Log(dist_{od})$	-0.0173***	-0.0053***	0.0120***
	(0.0007)	(0.0007)	(0.0010)
$\%\ Urban_{dt}$	0.2578***	0.0824***	-0.1754***
	(0.0293)	(0.0248)	(0.0384)
Panel C: Heterogenous effect of new grid			
$Log(dist_{od})$	-0.0094***	-0.0025***	0.0068***
	(0.0004)	(0.0003)	(0.0005)
$Grid_{dt}$	-0.0031**	0.0033*	0.0064***
	(0.0013)	(0.0019)	(0.0023)
Observations	749,232	252,324	1,001,556

Notes: Standard errors are two-way clustered at the year and municipality level and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Next, I perform a sample split to analyze how destinations that have received a new grid differ from those that did not. Results are reported in Table II.8. Column (1) shows results for origins that did not receive a new grid in time t, column (2) shows results for origins that did. Column (3) reports the difference between these coefficients. Across specifications, the effect of distance is 3 to 4 times smaller for origins that have received a new grid. This can be interpreted in two ways: first, migrants from origins that have received a new grid migrate over a larger distance, and second, the effect of migration costs on migration flows seems to fall. This provides additional evidence on the role of credit constraints. Given that the productivity shock increased household incomes by 23 percent, this would imply that raising incomes by 1 percent would increase migration by 13 to 17 percent on average. What is more, migrants from origins that have a new grid are 3 times more likely to go to destinations with expanding agricultural area and 3 times less likely to go to destinations with expanding urban area than those from origins that did not receive a new grid. This finding indicates that the productivity shock changes preferences over destination characteristics. While the average Nigerian migrant showed strong preferences for migration to urban areas, new migrants from origins that received a productivity shock seem to prefer destinations that share more characteristics with their origin. These characteristics are not only linked to the agro-climatic conditions or sectoral composition, but also to the production technology. It is therefore in line with the hypothesis that the productivity shock increased task-specific human capital such that returns to migration increased in destinations that possess the same sectors and technologies.

II.8 Concluding remarks

This paper provides evidence on how investments in electricity infrastructure affect internal migration. Using the expansion of the electric transmission grid in Nigeria in the years 2009 to 2015, I show that the intervention had a significant positive effect on out-migration. This seems driven by an increased access to credit with simultaneous lack of employment generation for the youth. While household food consumption increased substantially, the economic boom did not seem to benefit everyone equally. Young adults and older teenagers that suffer particularly from underemployment in Nigeria did neither show an increase in employment nor in working hours. The rise in labor demand only affected older, more experienced workers. Instead, we observe a rise in out-migration by this subgroup. The results suggests that this migration spike is mainly labor migration. I also observe that the effect of movement costs on migration decreased by factor 3 for these migrants. This suggests large efficiency gains from easing credit constraints.

Overall, the findings suggest that closing the rural-urban gap with infrastructure investments is extremely difficult. Despite large income gains of the intervention, for a large subgroup of the population employment creation was not sufficient. While raising productivity through public investments is an important tool to harmonize economic activity across space, in the short term, youth unemployment might best be tackled by easing credit constraints to enable migration. Policy-makers should, therefore, combine rural infrastructure investments with migration-oriented cash transfers to address the rural-urban gap effectively.

The findings of this paper are, however, limited to the short term. While in the short term, employment opportunities might be limited, demand for young, less experienced workers might rise in the long term. It is also not clear whether the observed youth migration is permanent. Since personal costs of living away from one's origin seem to be high, it is possible that young migrants return to their origin locations after collecting more work experience and/or education. In the long term, population dynamics might therefore reverse. This is, however, only possible if economic growth at origin continues, highlighting again the importance of structural investment. Further research is warranted to understand these long-term effects.

Finally, the paper sheds new light on how the electricity shocks affect the ordinal preferences for a destination. Following an electricity supply shock, migrants are more likely to migrate to rural destinations that also received new grid infrastructure. This finding suggests that the intervention changed not only the household budget, but also the relative expected returns from migration to each destination. This could be linked, for instance, to human capital effects in the form of learning-by-doing or task-specific human capital that is tied to characteristics of the location. Therefore, additional research is needed to understand how infrastructure investments affect ordinal preferences for migration destinations, particularly as a tool to channel migration flows consciously.

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III. Broken Promises – Evaluating an Incomplete Cash Transfer Program

Bibliographic Information

This chapter is co-authored with Utz Pape and Laura Ralston.

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Abstract

This study uses an unconditional cash grant program in South Sudan that had to be terminated due to conflict to assess the socio-economic, behavioral and psychological consequences of operational problems in development programs. We combine survey data from face-to-face interviews and data from lab experiments to study the unintended impacts of the program cancellation. Results from LATE estimations show that those participants that failed to receive the grant display a reduction in their consumption level and their trust level. Women of this subgroup also display an increase in their risk aversion. Participants that received the grants as intended increased their consumption and savings, while business skills and employment did not increase.

JEL: C93, D13, D81, O2A

Keywords: unconditional cash transfers, trust attitudes, risk aversion, impact evaluation, violent conflict

III.1 Introduction

Operational problems are a common issue for policy interventions in developing countries. Weak institutional capacity, limitations in human capital and low quality of public infrastructure pose challenges to the smooth implementation of any intervention. Moreover, many developing countries suffer from political instability, which in turn raises operational risks. With an increasing share of the world's poor living in fragile states (Kim 2019), operational problems are a major obstacle to eradicating global poverty. In extreme cases, operational problems lead to the unplanned interruption or even cancellation of an intervention. Despite the growing importance of these cases, little is known about the effect of a program cancellation on intended beneficiaries. Rather than studying operational problems directly, the economic literature typically treats them as noise in the data. In contrast, we believe that operational problems are an integral part of development policy and should be studied systematically. Understanding the consequences on intended beneficiaries can help policy-makers take informed decisions about the costs and benefits of an intervention. In addition, information on the consequences of failed implementations can help to integrate mitigating features at the design stage.

This study uses the unplanned and unanticipated cancellation of a cash grant program in South Sudan to study the effect of operational problems on intended beneficiaries. We benefit from the fact that the intervention was designed as a randomized controlled trial where selection for the cash grant was randomized and data collection included a control group. We are interested in the effects on participants that were promised a cash grant, but ultimately did not receive it. Economic theory lends multiple reasons why outcomes for these participants should differ from outcomes in the absence of the program. First, the expectation of a future income increase in the form of a cash transfer might affect current consumption and investment decisions. Second, the cancellation might have unanticipated psychological and behavioral effects.

The South Sudan Youth Startup Business Grant Program consisted of an unconditional cash grant combined with a business and life skills training in the six states least affected by conflict. South Sudan has suffered from political instability and latent conflict since its inception in 2011. In this context, the youth struggled with declining livelihoods and a lack of economic opportunities. This put them at risk of participating in or becoming victims of crime and violence. In response, the program was designed by the World Bank in collaboration with the Ministry of Commerce to offer a cash grant worth US$ 1,000. The existing literature suggests that injections of capital are the most effective means of raising income in poor and fragile states (Blattman & Ralston 2015). Beneficiaries could access the grants denominated in local currency through a commercial bank account. Although the cash grant was aimed towards promoting (self-)employment and business development, beneficiaries were free to decide on its use. The only condition consisted in participating in a one-week business and life-skill training.

In late 2014, the program randomly selected 1,200 beneficiaries to receive the grant, with 60 percent of the grants reserved for women. A similarly sized control group was selected to enable the assessment of the program in an impact evaluation. Baseline data from both groups were collected before grant beneficiaries received the business and life skills training. After the training, participants were asked to open a bank account with the Kenya Credit Bank (KCB) who acted as partner in this project. The grant money could only be accessed via these bank accounts.

Escalating violence at the end of 2016 forced the program to terminate the disbursement of the grants before all participants had access. This was done to prevent that participants became targets of violence. In addition, the program wanted to eliminate the risk that the grant money was used to purchase arms if it got into the wrong hands.

Our study distinguishes between two *ex post* treatments. Individuals that participated in the training and received the grant money as originally planned build the "training and grant" group. Individuals that participated in the training, but did not receive the grant as promised build the "training, no grant" group. In addition, the study uses data on participants that were selected for the control group and knew that they were not eligible to receive the grant.

The identification of these *ex post* treatment effects is not straightforward. Selection into receiving the grant was partly endogenous, since participants had to initiate the grant disbursement via a formal application at their KCB bank branch. Consequently, participants that started this process early had a higher propensity to receive the money before the program was put to a halt. To address endogenous selection into the *ex post* treatment groups, we construct an interacted instrumental variable consisting of a common shock and a local exposure share. This type of instrument has gained popularity in panel data studies, but can also be applied to spatial frameworks.[1] In particular, we generate an instrumental variable by interacting the selection for the original treatment group with the distance to the closest KCB branch. Selection for the original treatment group represented an exogenous shock

[1] Notable examples include Nunn & Qian (2014), Hanna & Oliva (2015) and Dreher et al. (2021).

to participants' access to credit. However, whether participants ultimately received the grant depended on the transaction costs of initiating the grant disbursement. These differed by the distance to the closest KCB bank branch. Our instrumental variable approach compares outcomes for individuals who lived near a KCB bank branch (low transaction costs) with those who lived far from a KCB bank branch (high transaction costs).

We argue that conditional on a set of geographic controls, the interacted instrument is plausibly exogenous. As shown in the methodological discussion in Christian & Barrett (2017), the validity of an interacted instrument requires that the exposure share variable (KCB bank distance) is uncorrelated with the error. The main endogeneity concern in our setting consists in a potential correlation of KCB bank distance with remoteness – which we can directly address by including a set of geographic controls. KCB was only one of multiple commercial banks active in South Sudan and not every major city had a branch of the KCB bank. We show that after conditioning on the geographic controls, distance to KCB is uncorrelated with distance to *any* commercial bank and therefore "as-good-as-randomly" distributed across the participants in our sample. We follow suggestions in Christian & Barrett (2017) and Goldsmith-Pinkham et al. (2020) to test the validity of our design further.

Our results show evidence of some detrimental effects. Participants that did not receive their grants display a significant reduction in their consumption level and their trust level. This suggests both resource misallocation and psychological repercussions due to the program cancellation. Moreover, women of this subgroup are more averse to risk. We also observe some positive impacts of the originally planned intervention. In particular, consumption and savings increased among the participants receiving both the training and the grant. The results are robust to re-weighting observations based on their inverse probability of being featured in the endline and the inclusion of different measures of conflict exposure.

Our study fills an important gap in the literature. Despite the political relevance, there is limited evidence on failed implementations of development programs. Ghosh & Kochar (2018) builds a notable exception with their findings on an Indian maternity benefit program. In particular, the authors find that positive outcomes resulted from participants' response to a poor implementation rather than the originally planned intervention. They conclude that researchers risk spurious correlations if they rely for identification on the policy rule rather than *ex post* implementation. Beyond that, most studies focus on the reasons for, rather than consequences of implementation problems. Most commonly these are linked to remoteness and problems in "last mile" delivery (Brinkerhoff et al. 2018, Dussault & Franceschini 2006, Das

& Gertler 2007, Abate et al. n.d., Briggs 2018). In addition, implementation failures are often linked to weak governance (Campos et al. 2014).

Given the results of this study, we argue that greater concern should be given when planning programs in volatile environments. Our results suggest both economical and psychological disadvantages for beneficiaries affected by operational problems.

The remainder of the paper proceeds as follows. Section III.2 describes the original design of the invention and its context. Section III.3 outlines our theoretical considerations and places the study in the existing literature. Section III.4 discusses our study design. Section III.5 describes our empirical strategy and discusses the validity of the instrumental variable approach. In Section III.6, we describe the main results. Finally, Section III.7 concludes.

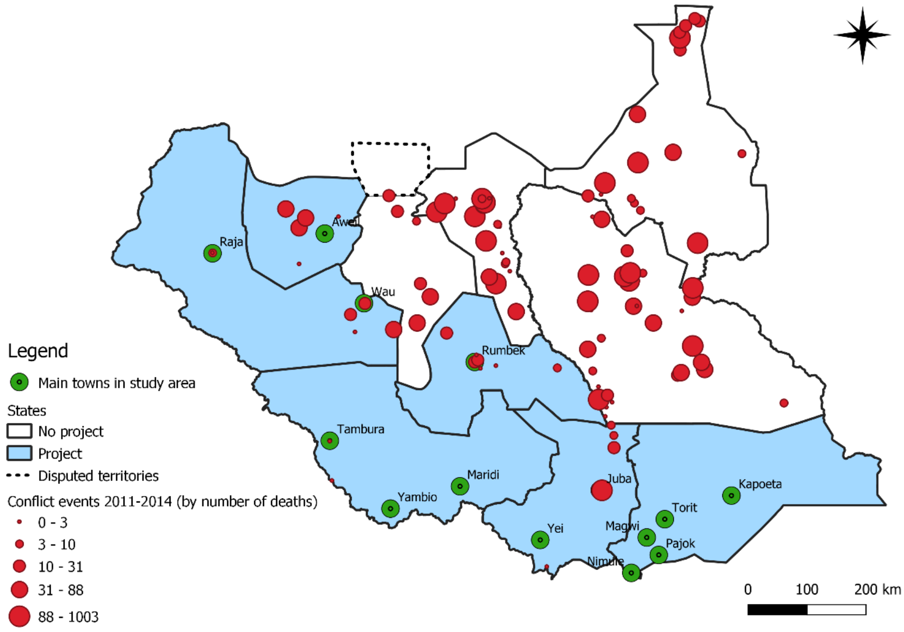
III.2 Context

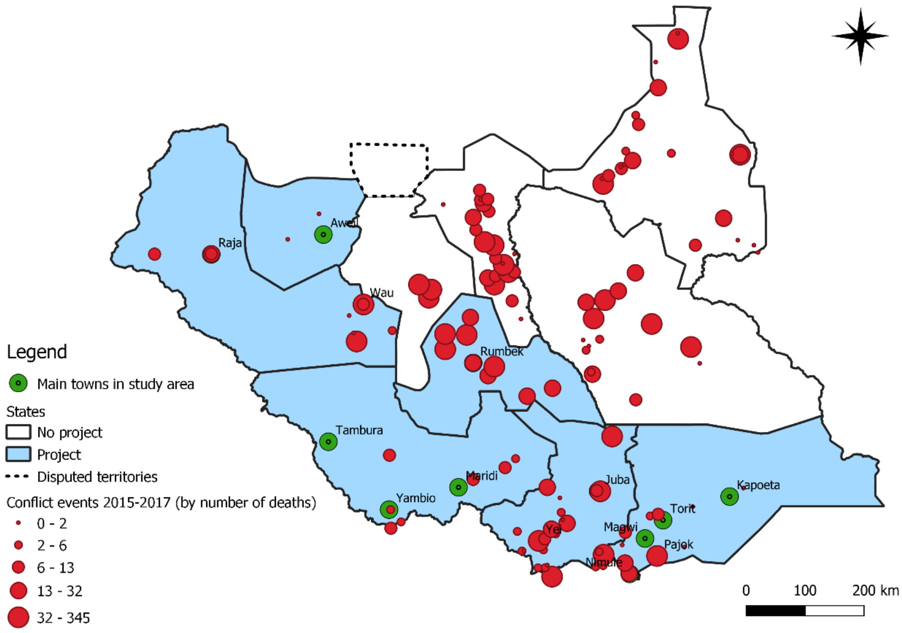
III.2.1 Conflict in South Sudan

Historically, the region that is today South Sudan had been marred by Africa's longest running civil war. While most of the violence ceased with independence from the Republic of Sudan in the North in 2011, much of the fighting had consisted of clashes between the 63 distinct ethnic and language groups within the territory of South Sudan. This legacy of violence had challenged the creation of adequate democratic institutions in the years after independence. What is more, large windfall profits from oil revenue had led to the creation of a large patronage network that maintained peace through financial flows to various parties. When oil prices fell and government payments faltered, only 2 years after achieving independence, South Sudan experienced a re-eruption of violence. Civil war broke out anew in December 2013 partly due to opposition to the concentration of power under South Sudan's President Salva Kiir who had drastically reorganized the country's political and military leadership. Despite efforts of the international community to mediate a peace agreement, the conflict lasted until 2018. The country is still considered highly fragile, ranking as the fourth most fragile country in the World in 2021 according to the Fragile States Index (Fund for Peace 2021). The deep ethnic divisions in the country and the breakdown of social cohesion are accompanied by low trust in the government. Combined with macroeconomic crises, the conflict had hampered all dimensions of development and had led to a sharp increase in the poverty rate from 51 percent in 2009 to 66 percent in 2015 (Pape et al. 2018).

Geographically, violence was first concentrated in the North of the coun-

Figure III.1: Map of conflict events before and during project period





try. Figure III.1 displays conflict events before 2015 and during our study period from 2015 to 2018. The six Southern states were selected for the cash grant program due to their relatively low exposure to the conflict prior to 2015. However, after program initiation the conflict moved further south and particularly the states Lakes and Central Equatoria, as well as the urban centers in Western Bahr el Ghazal and Eastern Equatoria became major conflict sites.

III.2.2 The Youth Startup Business Grant Program

The Youth Startup Business Grant Program was designed by the World Bank in cooperation with the Government of South Sudan in response to the high level of poverty and lack of employment opportunities, particularly for the youth. Since independence, South Sudan was faced with a huge development deficit, instability and continuing violence. Under these circumstances, the government struggled to build effective institutions and provide sufficient public services. The program was intended to increase (self-)employment opportunities to the youth, while ensuring low implementation complexity. Beyond benefits to individual beneficiaries, it was meant to create local economic spill-over effects and contribute to the creation of a stronger private sector. Since social security spending in South Sudan had been and still is mainly focused on emergency food aid, there was a need for interventions targeted at a longer development horizon. Pape (2015) conducted a report on the design and implementation of the program after baseline data collection and before the re-eruption of violence forced the program to a halt, which documents the original expectations from the program.

The program was implemented in the six states that were, up until then, least affected by the conflict: Eastern Equatoria, Central Equatoria, Western Equatoria, Lakes State, Northern Bahr el Ghazal, and Western Bahr el Ghazal. Figure III.1 displays a map of the selected states, the location of the partnering bank branches and beneficiaries' locations of residence at baseline.

The program consisted of a one-off lump sum payment worth US$ 1,000 combined with a one-week business skills training. Participation in the training was compulsory in order to access the grant. The amount of US$ 1,000 meant a very large sum for the beneficiaries. For comparison, weekly food expenditure at baseline in our sample consisted of only US$ 5.36. The financing followed an innovative mechanism designed to increase the probability that beneficiaries used the money productively. This was done by allowing participants to use the grant as a loan. To do so, the cash grants were channeled through the KCB bank and treated as loans. To access the money, participants had to open a bank account at a KCB branch and initiate grant disbursement by following the application procedure typically associated with taking a loan. Successful repayment of the money resulted in the possibility of receiving a new loan of US$ 1,000. In case of default, the loan was covered by a guarantee of the Government of South Sudan and the debt of the participants was cleared. These features meant that the financing ultimately consisted of a cash grant, while being framed in the context of a loan. Originally, it was planned to combine the cash grant with text message reminders that framed the grant either in the form of a loan or in the form of a grant. Due to the program failure, this program aspect was not implemented.

The eligible population of the program was the youth in the six program states with 60 percent of the grants reserved for young women. Eligible individuals had to be aged between 18 and 35 and possess proof of South Sudanese nationality. Eligibility was also conditional on submitting a one-page written proposal for a new small-scale business venture. Moreover, eligibility depended on the feasibility of this business idea. Despite the requirement to present a business idea in the application documents, the cash grant was not conditional on actually pursuing this plan. This was meant to allow the participants maximum flexibility in their investments. The proposal had to be written in English on a standardized form that was handed out by local authorities in each state. In addition, candidates had to promise on the application form that the financing would not be used for the purchasing of land, alcohol, weapons, or other harmful items. This application process was designed to incentivize positive self-selection into the sample.

From July to November 2014, the program was widely advertised by a comprehensive communication campaign. Communication channels included radio, television, posters, fliers, print advertisement and special events at women's and youth associations organized by local authorities. Most baseline participants reported to have heard about the program from a friend or relative (54%) and/or via the radio advertisements (48%). The program was advertised as a government program and framed as "financing" without specifying whether repayment of the fund was required. This was linked to the original plan to test different framing schemes via the text messages reminders.

Each state collected applications centrally from October to November 2014. Across the six states, we received 8,240 application and found 4,699 of these to be eligible. While the feasibility of the business idea had to be assessed manually, the remaining criteria were assessed automatically. The most common reason for ineligibility was the plan to use the financing for the purchase of land. Other reasons included blank or unrealistic business ideas (such as purchasing an airline), age listed outside the target range, no identification attached, or not being South Sudanese.

Selection was based on an over-subscription framework. This meant that 1,200 applicants were randomly selected for the treatment group and an equal number was randomly selected for the control group. The remaining applicants did not participate in the study. This process was communicated clearly during the application process. Applicants were informed that participation in the program would be based on a lottery. Selection was stratified by state and gender, meaning that 120 women and 80 men were selected for treatment in each program state. The selection mechanism was based on a simple and reproducible method. Within each state-gender strata, applicants were listed and ordered by phone number. A random number process then selected the first beneficiary from the list. The subsequent applicants were selected sequentially for treatment based on the list of phone numbers. After selecting sufficient applicants for the treatment group, the next applicants on the list were selected as replacements. For women, 12 applicants were picked for replacement, and for men, 8 in each state (i.e., 10% of the treatment group). After selecting the replacements, the control group was selected following the same mechanism.

Before accessing the grant, participants selected for treatment participated in a five-day business and life skills training. The training covered 7 components: (1) Entrepreneurial Motivation, (2) Business Ideation, (3) Business Communication, (4) Record Keeping, (5) Legal Aspects of Business, (6) Banking Services, and (7) Personal Management. It was delivered by individually contracted educators. These were recruited and selected by the Ministry of Trade, Industry and Investment. To prepare the educators for their task, they first had to participate in a two-week "training of the trainers". During this training of the trainers, the educators learned presentation and teaching skills, but also how to conduct each individual component of the business and life skills training. The business and life skills trainings had to follow a uniform structure and we provided all materials to the educators to ensure that training quality depended to a limited degree on the individual educator. Each program state hosted multiple separate trainings for 50 beneficiaries at a time, respectively. Educators were assigned to these trainings based on geography. In addition, educators were assigned to different modules of the training based on their own expertise. The trainings took place between March and April 2015.

After the training, participants had to open a formal bank account with the closest branch of the KCB bank. After opening the account at the respective branch, participants could access the grant in the form of a regular loan that had to be cleared by the bank first.

When the conflict re-erupted in early 2016, the grant disbursement was first frozen and ultimately cancelled. From our local partners, we learned that there was a failure of coordination between different bank branches. While some branches moved quickly with halting disbursements of grants, other branches did not. Unfortunately, when bank branches stopped the disbursement was not well documented and the information could not be obtained *ex post*.

III.3 Literature review and theoretical considerations

To understand the potential effects of a program cancellation, we first consider the intended effects of the program. Banerjee et al. (2017) list four theoretical explanations how cash transfers can increase employment: (1) cash transfer can enable participants to overcome the classic poverty trap by providing them with sufficient living standards to become productive workers, (2) they can reduce credit constrains that kept participants from starting an enterprise, (3) they can enable investments in profitable but risky endeavors – which applies to many business activities in conflict-affected regions – and (4) they can create local spill-over effects.

These theoretical predictions are supported by multiple studies in non-conflict affected areas (e.g., Bianchi & Bobba 2013, Banerjee et al. 2017). For conflict-affected environments, the evidence is more limited. Blattmann et al. (2014) find in the conflict-affected North of Uganda that a cash grant program targeted at generating self-employment among youth significantly increased their earnings after 4 years (Blattmann et al. 2014). A follow-up study found that groups converged over time in consumption, employment and earnings, but that there was a lasting effect on occupational choices (Blattman et al. 2020). These findings suggest that poverty gains are highest where occupational choices and access to credit are lowest as in the context of South Sudan.

However, cash transfers without explicit employment focus do not automatically generate employment (Baird et al. 2018). Therefore, the Youth Startup Business Grant Program in South Sudan combined the cash transfer with a business skills training. Research on the impact of business trainings is generally mixed and there is still a lack of evidence on the type of content that shows the best results (McKenzie & Woodruff 2014). Multiple studies found positive effects of business trainings on business knowledge (Mano et al. 2012, Bjorvatn & Tungodden 2010, Berge et al. 2015). There is also evidence that business trainings can help to overcome gender-based norms regarding entrepreneurship (Field et al. 2010). Moreover, business training seems most effective when combined with financial support (Cho & Honorati 2014).

In addition, the program had a gender dimension, with 60 percent of the grants recerved for women. A growing body of literature suggests that cash transfer programs can increase women's empowerment (see Peterman et al. 2020, for a systematic survey of the literature). Benefits include increased bargaining power in household decisions and increased self-esteem (Adato & Roopnaraine 2004, Handa et al. 2009, De Brauw et al. 2014). Less is known about the effectiveness of cash transfers to increase female employment, particularly in contexts where traditional gender roles are prevalent. De Brauw et al. (2015) find some evidence that cash transfers in the context of Brazil's *Bolsa Familia* program led to a reallocation of paid working hours away from women to men. In contrast, Bosch & Schady (2019) find no negative effects of welfare payments on female employment in Ecuador.

Economic theory offers multiple predictions on how the program cancellation might affect the welfare of intended beneficiaries. First, beneficiaries might experience a welfare loss due to resource misallocation. This draws on the permanent income hypothesis according to which rational agents should smooth consumption over time. Given that many agents in a developing country context are credit constrained, the risk of misallocating income or savings might have been limited. However, as Ghosh & Kochar (2018) suggest, credit constrained households can find versatile ways to adjust resource allocation to the expectation of future income gain. For instance, beneficiaries might decline employment opportunities due to the expected income increase.

Second, intended beneficiaries might experience a welfare loss due to the negative psychological repercussions. Current research understands poverty as much as a result of psychological factors as well as economic factors (Bertrand et al. 2004, Dalton et al. 2016). For instance, Quidt & Haushofer (2016) proposes a theoretical model where exogenous negative shocks can create pessimistic beliefs about the expected returns to effort, which can in turn induce reductions in labor supply and psychological well-being. In addition, it is possible that psychological well-being depends not only on absolute economic status, but also relative economic status compared to one's peer group (Luttmer 2005). For instance, Baird et al. (2013) find that psychological distress increased among untreated study participants in treatment areas of a cash transfer program.

Evidence from lab experiments shows that the experience of being lied to significantly reduces participants' trust level as well as their trustworthiness (Gawn & Innes 2018). Although the program promise to deliver a cash grant was no deliberate "lie," it is possible that disappointed participants perceived it as such. If so, the program cancellation could have created an erosion of trust. Subsequently, other outcomes such as employment or engagement in crime and violence could have been adversely affected. This mechanism would be particularly concerning, given the evidence that international organizations such as the World Bank sometimes enjoy more trust than governments – particularly if governments are seen as corrupt (Milner et al. 2016, Findley et al. 2017).

What is more, the program cancellation could have affected risk aversion. A large body of literature analyzes the effect of adverse shocks on risk preferences (see Chuang & Schechter 2015, for a review of the literature). An increase in risk aversion was found to be associated with shocks such as exposure to violence (Callen et al. 2014, Jakiela & Ozier 2019, Brown et al. 2019), natural disasters (Cameron & Shah 2015, Cassar et al. 2017) and macroeconomic shocks (Malmendier & Nagel 2011, Guiso et al. 2018).[2]

III.4 Study Design

Figure III.2: Treatment streams of original and new intervention

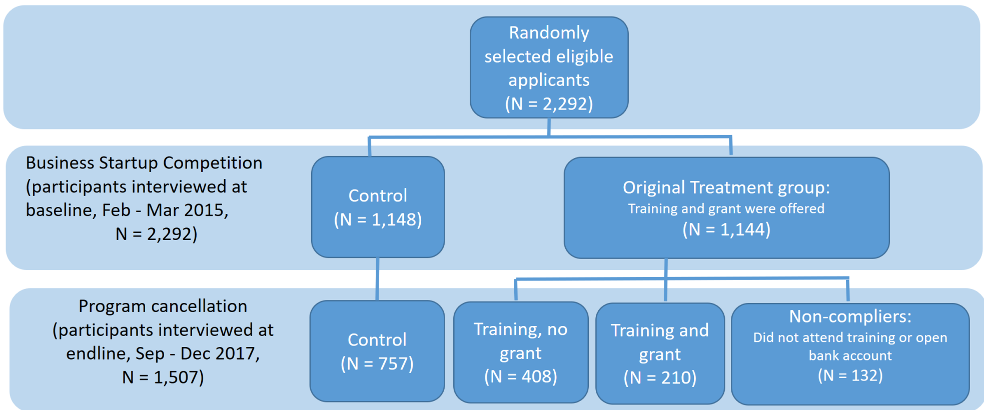


Figure III.2 illustrates the treatment arms of the original invention and the program cancellation. The baseline survey was conducted between January to March 2015 and data were collected from 1,144 treatment participants and 1,148 control participants. Approximately 4.5 percent of initially identified study participants could not be tracked and did not participate in either the baseline survey or the program. The baseline survey was concluded

[2]However, some studies find the opposite effect (e.g., Hanaoka et al. 2018).

before beneficiaries were informed whether they were selected for the grant and prior to the one-week training.

The intensification of violence between 2015 and 2017 forced about a quarter of the population of South Sudan to migrate during the study period, which made it difficult to locate all participants of the original control and treatment group. Before the endline survey, the World Bank conducted a phone survey in May 2017 that informed the grant beneficiaries of the halt of the program and assessed the feasibility of collecting endline data. The phone survey reached around 55 percent of the study participants (642 from the control group and 622 from the original treatment group), from which 99 percent agreed to participate in the endline.

Due to budget and logistical considerations, the endline survey targeted a sample size of 1,800 individuals randomly chosen from the list of participants after prioritizing the phone survey respondents who had agreed to be interviewed again. Endline data collection activities commenced in September 2017. After intensive tracking efforts over a period extending to four months,[3] 1,524 participants were located, and 1,507 participants completed the interviews. The respondents interviewed in the endline survey were given the opportunity to voice their concerns and opinions about the cash grant program, through short video testimonials that are publicly available online.[4] Out of all endline respondents, 1,045 had already been reached in the phone survey and 462 had been located through intensive tracking efforts based on information provided in the baseline.[5] Figure III.3 illustrates the timeline of the data collection and intervention steps.

At the end of the endline survey, there was approximately equal representation between the treatment (750) and control (757) groups, with 394 and 391 fewer observations from each group respectively. This was despite ongoing conflict keeping enumerators from going to a few counties due to insecurity.[6]

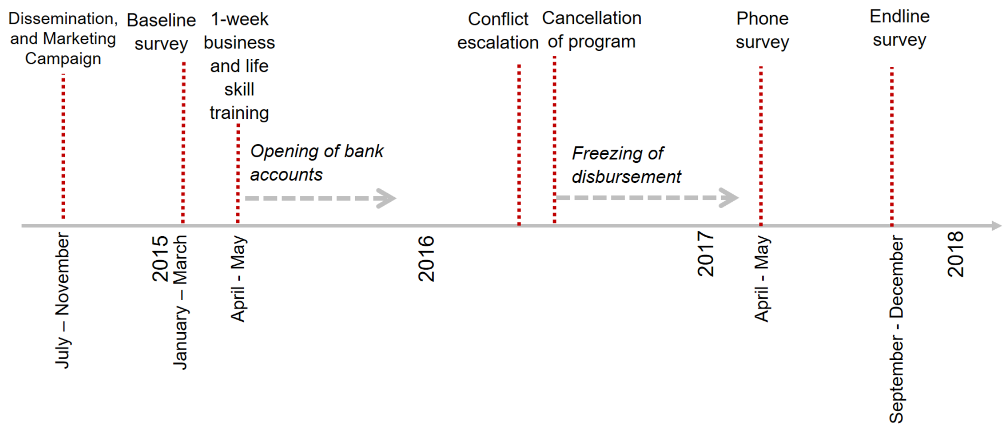
[3] The majority of data were collected between September and November 2017, but field teams remained on the ground until the end of December 2017 trying to locate and interview participants.

[4] The video testimonials from this study as well as other surveys conducted in South Sudan during this period are available at: www.thepulseofsouthsudan.com.

[5] Intensive tracking efforts included returning to the GPS coordinates for the baseline survey and looking for participants, contacting other contacts listed by the participant in their program application and through the baseline survey, asking other respondents, local officials, trade unions and the Chambers of Commerce about the location of difficult to find participants, and making at least five attempts to find persons over a period of several weeks.

[6] In WEQ: Mvolo, Mundri East, and MundriWest; in CEQ: KajoKeji, Morobo, and Lainya; in Lakes: Rumbek North (flooding during the time of data collection).

Figure III.3: Timeline of program implementation, cancellation and data collection



The main approach for measuring outcome variables was through face-to-face interviews that were conducted as part of the baseline and the endline surveys. In addition, we assessed risk preferences and trust attitudes using lab experiments collected during these face-to-face interviews from decisions reported over lotteries and trust games (see Appendix C.1 for full methodological details).

III.5 Method

III.5.1 Multiple hypothesis testing

Due to the exploratory nature of this study, we test a large number of outcomes. To address a potential bias from multiple hypothesis testing, we use two approaches. First, we reduce the number of tested hypotheses by summarizing outcome variables into grouped indices. (See Appendix C.2 for a detailed description of the method). It allows us to keep the number of outcome variables low with greater statistical power. The components of each index are described in Table III.1. Each index is standardized based on the control group to have a mean of 0 and a standard deviation of 1. This allows for easier interpretation of the results. Table III.2 reports summary statistics for all outcome indices.

Table III.1: Main outcomes of interest

	Outcome Name	Details
		Socio-economic outcomes
1	Employment index	Standardized weighted average of the number of hours spent on wage employed activities in the past 7 days, (log) cash wage received in the past 7 days, (log) outstanding wage from the past 7 days, (log) total wage in past 7 days, number of activities on wage employment in the past 7 days, number of hours spent on self-employed activities in past 7 days, (log) self-employed cash earnings in the past 7 days, (log) self-employed in-kind earnings in the past 7 days, (log) outstanding earnings from the past 7 days, (log) total self-employed earnings in the past 7 days, number of self-employed activities in the past 7 days, total number of employees, (log) business revenue during the past 4 weeks, (log) business sales yesterday, (log) aggregated business costs in the past 4 weeks
2	Consumption index	Standardized weighted average of the number of different food items consumed in the past 7 days, (log) total food expenditure in the past 7 days, (log) value of self-produced food in the past 7 days, (log) expenditure on non-food items in past 1 month, (log) expenditure on assets in past 1 month
3	Savings, investment and debt index	Standardized weighted average of having or sharing a formal bank account, currently saving any money, (log) amount held at bank account, (negatively coded) number of formal loans received, (negatively coded) other debt, (negatively coded) number of informal loans received in the past 1 month, (negatively coded) (log) total amount of formal loans, (negatively coded) (log) total amount of informal loans, business ownership, participation in training during the past 12 months, number of trainings done in the past 12 months
4	Business skills index	Standardized weighted average of frequency of visiting competitors, frequency of asking customers about other products they would like to be sold, frequency of setting sales targets, frequency of comparing targets to performance, frequency of recording purchase and sales, knowledge of the business register, knowledge of fees to register a business at cashier's office of the Business Register, knowledge of operating license from State government, knowledge of inspections from local authorities, knowledge of taxes, knowledge of bribes, knowledge of paying an intermediate person to take care of taxes, registration of company name at business register, registration at cashier's office of the Business Register, obtainment of operation license from the State government, experienced inspection by local authorities, payment of formal taxes, payment of bribes, payment of intermediary person to take care of taxes

Psychological and behavioral outcomes		
5	Psychological wellbeing	Standardized weighted average of happiness with education level, with family, with job and work, with earnings or income, with house they live in, with life as a whole, with community they live in, with security and with friends, "ladder of life" (self now), "ladder of life" (household now), "ladder of life" (self in 5 years), "ladder of life" (household in 5 years), internal locus of control score on the possibility to become a leader based on ability, on general events in life, on influencing the number of friends, on control over future events, on feeling protected, on planning ahead, on pleasing people above to get ahead, on (negatively coded) dependence on luck to become a leader, on working hard to get ahead, on the belief that own actions matter most, empowered decisions on food/clothing purchases for children, on opening a business, on taking a loan, on visiting a friend, on traveling to another town, on staying overnight at another town, on getting a child vaccinated, on purchasing small items, on paying school fees for relatives
6	Risk index	Standardized weighted average of (negatively coded) likelihood of sleeping under a mosquito net (negatively coded), likelihood to walk alone at night, likelihood to spend an afternoon waiting for a medical exam (negatively coded), likelihood to take a motorbike-taxi (boda-boda) if the driver is unknown, likelihood to engage in unprotected sex, likelihood to invest in a safe business accepting low profits (negatively coded), likelihood to invest into a business that has high profits but equal chance of failing, likelihood to take a loan if there were no restrictions, experimental data on number of times the more risky lottery was chosen
7	Trust index	Standardized weighted average of 13 trust items: trust to people in general, trust that people are helpful, (negatively coded) belief that people seek their own advantage, willingness to lend money, willingness to lend possessions, trust in family, trust in friends, trust in neighbors, trust in police, trust in NGO, trust in elders, trust in local government, trust in state government, experimental data on amount sent to the World Bank in a trust game and amount sent to another player in a trust game
8	Crime and violence index	Standardized weighted average of participation in a security group, frequency of participation in a security group, hours participated in a security group last week, experience of own cattle been stolen, number of times own cattle had been stolen in the past 1 year, knowledge of a least 1 home/market stall robbery, number of known home/market stall robberies, experience of harassment during past 1 month, number of times been harassed during past 1 month, experience of having been physically punished or beaten, feeling concerned that receiving money might foster crime or violence

9	Migration index	Standardized weighted average of having moved since baseline, living outside SSD in the past 1 year, living in a refugee camp in the past 1 year, living in a camp for internally displaced people in the past 1 year, having the wish to move

Second, we add p-values adjusted by false discovery rate to all results tables. We follow the two-step procedure introduced by Benjamini & Hochberg (1995).[7] This procedure controls for the expected proportion of rejections that are type I errors within a family of outcomes. The group of socio-economic and behavioral/psychological outcomes are employed as the two main families of outcomes.

Table III.2: Summary statistics of outcome variables for the control group

	N	Mean	SD	Min	Max
Main outcomes					
Employment index	763	0	1	-2.314	6.401
Consumption index	763	0	1	-1.58	5.037
Savings, investment and debt index	763	0	1	-4.013	2.984
Business skills index	763	0	1	-2.971	2.569
Psychological wellbeing index	763	0	1	-2.625	3.606
Risk index	763	0	1	-2.789	3.142
Trust index	763	0	1	-2.982	3.147
Crime and violence index	763	0	1	-1.214	5.667
Migration index	763	0	1	-0.838	3.740

Before aggregation, outliers and indicators with limited variation were excluded from the final sample. In order to exclude outliers, indicators were winsorized for all continuous non-negative indicators at 99 percent. In addition, we tested the indicators for limited variation and omitted questions from the analysis for which 95 percent of observations showed the same value within the relevant sample. This resulted in the exclusion of only 6 indicators.[8]

III.5.2 Selection into treatment arms

Selection into the treatment arms followed a two-stage process. In the first stage, we randomly selected participants from the control group and the

[7] See also Anderson (2008) for a discussion of adjusting p-values by controlling for false discovery rate versus controlling for family-wise error rate.

[8] Indicators excluded due to limited variation are: Engagement in cattle raids and frequency of cattle raids, number of times having been beaten during the past month, in-kind payment for wage employment, remaining amount from a formal loan and remaining amount from an informal loan.

original treatment group according to the originally planned intervention. A balance test on baseline study participants shows no systematic differences between the original control and treatment group across most covariates (Appendix Table C.3.1).

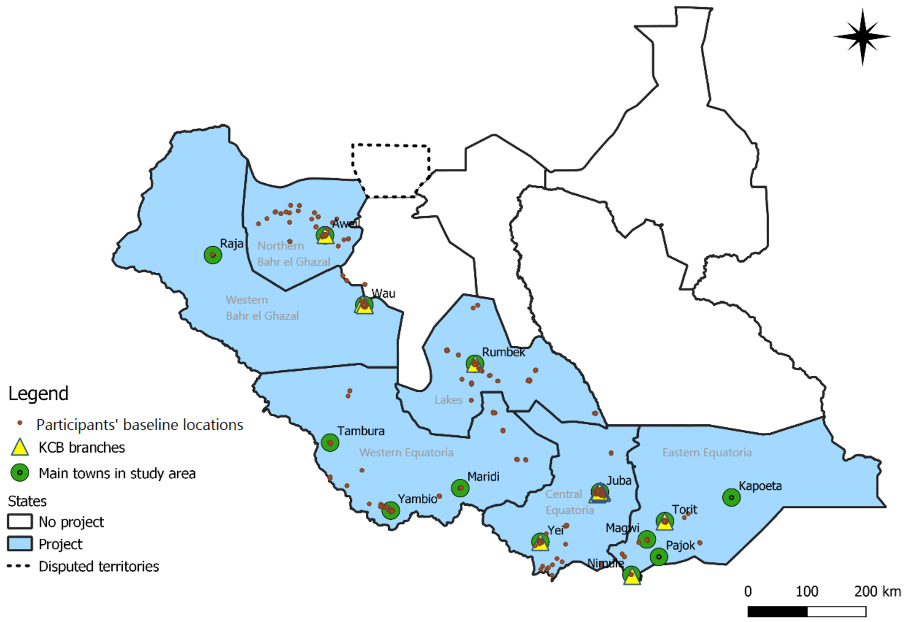
The second stage of the selection process decided which *ex post* treatment participants of the original treatment group received. Since the cancellation of the program was not planned, this process was not systematically controlled. Participants that initiated grant disbursement early had a higher chance to receive it. Among the original treatment group participants, we have three *ex post* groups. The "training, no grant" group consists of the 408 individuals that had not accessed their grants when the program was unexpectedly terminated in late 2016, the "training and grant" group consists of the 210 individuals that successfully accessed the grant, and the "non-compliers" group consists of the 132 individuals who did not attend the training and therefore could not access the grant.[9]

To assess the degree of endogenous selection into the "training, no grant" and "training and grant" groups, we test the balance on covariates between these two *ex post* treatment groups (Appendix Table C.3.2). We find some systematic differences. A joint test on orthogonality fails to reject the null hypothesis at the 5 percent level. The balance test shows that more educated participants with larger non-food consumption, larger amounts of informal debt that already held a formal bank account were more likely to access the grants. This suggests that those who accessed the grants were endogenously equipped via better education or prior banking experience. Importantly, we do not find any evidence that exposure to conflict determined whether participants could access the grant. A major determinant of *ex post* treament was their distance to the closest KCB branch. Participants that received the grant were, on average, approximately 40 percent closer to any KCB branch. This suggests that transaction costs to access the grant increased in bank distance. We exploit this variation in our instrumental variable strategy. Figure III.4 illustrates the spatial relationship between participants' baseline location and locations of KCB bank branches.

Moreover, we find some variation across program states. In Lakes and Western Bahr El Ghazal the majority of the eligible participants received the cash grants, while in Eastern Equatoria and Western Equatoria the majority did not receive the grants. In our main regression these differences will be absorbed by fixed-effects.

[9]Attending the training was a prerequisite to accessing the grant. Part of the training included financial literacy around opening and using the bank accounts, so only those participating had bank accounts opened for them.

Figure III.4: Map of selected program states, participants' baseline locations, and bank branches locations



III.5.3 Intent-to-treat estimation

We begin our analysis by estimating an intent-to-treat (ITT) effect, which gives the average effect of the intervention on all participants that were selected for the original treatment group. Since assignment to the original treatment group was randomized, the coefficient of the estimate has a causal interpretation. It indicates whether there was a negative net average effect of the intervention on any of the main outcomes. This gives us a first indication of whether the intervention created more "harm" than "good." The specification for the intent-to-treat effect is as follows:

$$y_{ij} = \alpha + \beta Z_i + X_i'\gamma + s_j + \epsilon_{ij}, \tag{III.1}$$

where y_{ij} is a vector of outcomes for individual i in strata j, Z_i is a dummy variable that takes a value of 1 if individual i was originally selected for the cash grant program, s_j are strata fixed effect and ϵ_{ij} is the error-term clustered at the baseline boma level.[10] X_i are covariates at individual-

[10]Bomas are the lowest administrative division in South Sudan. There are 277 bomas across the 6 states in the study.

and household-level that we collected at baseline. At the individual level, these include age, marital status, employment status, business ownership, individual food consumption, individual non-food consumption, holding a formal bank account, amount of formal and informal loans, education level, literacy level, and numeracy level. At the household level, the control variables include household size, number of children, number of rooms, number of buildings at baseline, and exposure to conflict events between baseline and endline.

III.5.4 Local average treatment effect estimation

A. Design Specifics

To understand the effects of "training and grant" or "training, but no grant," we cannot rely on treatment-on-the-treated (TOT) estimations since these are likely biased due to self-selection into treatment. While, we report results on TOT estimates for comparison in Appendix C.4, we estimate local average treatment effects (LATE) by employing an interacted instrumental variable consisting of a common shock and a relative exposure variable.

In particular, we instrument the positive supply shock of the cash grant by interacting selection for the treatment group – as positive common supply shock – with distance to the closest KCB bank branch – as relative exposure to the shock. This is similar to Hanna & Oliva (2015) who use distance to a refinery interacted with the refinery closure as exogenous variation in exposure to air pollution. Our estimation strategy exploits the fact that receiving the grant was conditional on holding a formal bank account at the KCB bank. During the study period, KCB operated only 15 bank branches and not in every large city. However, KCB was only one of at least 8 commercial banks active in South Sudan since independence (Bank of South Sudan 2010). This led to some variation in the transaction costs of accessing the grant. As described in the previous section, the distance to any KCB bank branch was a major determinant of the *ex post* treatment group. Importantly, the identity of the commercial bank that would partner in the program was not known to participants at the application stage. Therefore, KCB distance should not have affected self-selection into the program.

Since we have control group observations that did not experience the "treatment group" shock, we can control for level differences in outcomes along the distance dimension by including KCB bank distance directly as control variable.

In addition, we address non-compliance with the program by using the original selection for the treatment group as an instrument for compliance with the program, i.e., that participants went to the training and opened a bank account. This leaves us with two endogenous regressors and two instruments.[11]

Algebraically, our estimation strategy for "training and grant" and "training, no grant" reads as following.

Second stage equation:

$$y_{ij} = \alpha + \mu \widehat{Treatment}1_i + \beta \widehat{Treatment}2_i + \lambda KCBDist_i + X_i'\gamma + s_j + \epsilon_{ij}, \tag{III.2}$$

First stage equations:

$$Treatment1_i = \alpha + \mu Z_i + Z_i \times KCBDist_i'\sigma + \lambda KCBDist_i + X_i'\gamma + s_j + \epsilon_{ij}, \tag{III.3}$$

$$Treatment2_i = \alpha + \mu Z_i + Z_i \times KCBDist_i'\sigma + \lambda KCBDist_i + X_i'\gamma + s_j + \epsilon_{ij}, \tag{III.4}$$

where y_{ij} is a vector of outcomes for individual i in strata j, X_i are baseline and geographic covariates, s_j are strata fixed effects, ϵ_{ij} is the error-term clustered at the boma level, and $Treatment1_i$ and $Treatment2_i$ are dummy variables indicating treatment streams as described above. Equations III.3 and III.4 display the first-stage equations, which instrument $Treatment1_i$ and $Treatment2_i$ with the original assignment to treatment Z_i as well as the interaction between Z_i and the logarithmic distance to the closest KCB branch $KCBDist_i$. The LATE of $Treatment1_i$ and $Treatment2_i$ is estimated by parameters μ and β respectively. Our control variables include the same individual and household level covariates as equation III.1 together with a number of geographic covariates. In particular, they include distance to the closest city center, distance to the closest road, average land gradient, conflict exposure before the program as well as the interactions of all geographic covariates with Z_i.

Our first stage regressions in Table III.3 demonstrate that the interaction term is a strong predictor of whether participants received "training and

[11] Since interpreting and assessing the validity of regressions with more than one endogenous regressor can be tricky, we also tested regressions where treatment 1 was replaced by the random original selection to the treatment group and only treatment 2 (receiving training, but not the grant) was treated as endogenous regressor using the interaction between original selection to the treatment group and distance to the KCB bank branch as an instrument. In this specification, the coefficient on original treatment group selection can be interpreted as an intention-to-treat effect for treatment 1, because non-compliers are included. The results did not differ much from our preferred specification with two endogenous regressors.

grant" or "training, but no grant." In contrast, the non-interacted distance to any bank branch is statistically insignificant conditional on treatment being zero. This is not surprising. It tells us that distance to the bank branch did not determine whether participants ended up in the control group or either of the treatment groups.

Table III.3: First stage results from LATE estimation of Table III.6 and Table III.7

		(1) "Training, no grant"	(2) "Training and grant"	(3) "Training, no grant"	(4) "Training and grant"	(5) "Training, no grant"	(6) "Training and grant"
Instrument 1	Treatment	0.4226***	0.3860***	0.4196***	0.3875***	0.4414***	0.4254***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)
Instrument 2	Treatment x (log) Distance to KCB branch	0.0517***	-0.0450***	0.0523***	-0.0442***	0.0716***	-0.0620***
		(0.002)	(0.001)	(0.003)	(0.001)	(0.000)	(0.000)
	(log) Distance to KCB branch	-0.0032	0.005	-0.005	0.0081	-0.0093	0.0143
		(0.661)	(0.454)	(0.549)	(0.261)	(0.418)	(0.107)
	Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
	Individual controls	No	No	Yes	Yes	Yes	Yes
	Geography controls	No	No	No	No	Yes	Yes
	Observations	1,500	1,500	1,474	1,474	1,474	1,474

Notes: This table displays the first stage results for LATE estimates of Tables III.6 and III.7. Columns (1) and (2) correspond to LATE estimates of column (4) in Tables III.6 and III.7. Columns (3) and (4) correspond to LATE estimates in column (5) in Tables 8 and 9 and columns (5) and (6) to column (6) respectively. P-values are in parenthesis displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

B. Validity of the Research Design

To analyze the validity of the interacted instrument, we draw on Christian & Barrett (2017) who note that an interacted instrument is only exogenous if "either the exposure variable is uncorrelated with the error term or the correlation is constant across time and space." This parallels the theoretical findings in Goldsmith-Pinkham et al. (2020) who show for Bartik-type instruments – a more general case of interacted instruments where the instrument consists of the sum of multiple common shocks multiplied with their relative exposure variables – that validity can be derived from the exogeneity of the exposure shares.

In our study exogeneity of the exposure variable requires that the expected outcome of receiving treatment 1 (training and grant)[12] is the same for all participants regardless of their distance to any KCB bank branch.

[12] Or treatment 2 (training, no grant), respectively.

The main risk of violation would be a correlation of KCB distance with various dimensions of remoteness. For instance, if KCB bank distance was correlated with market access, participants with higher market access would have more possibilities to spend their newly gained income from the grant. Consequently, their consumption level might rise more strongly than for participants that lived further away from markets. Assuming that receiving treatment 2 (training, no grant) led to a consumption decrease compared to the control group, this would downward bias the estimate and lead to a larger effect size. Another violation of the identifying assumption would occur if KCB bank distance was correlated with access to general banking services. In that case participants that lived closer to a KCB bank branch could potentially possess more financial literacy due to previous banking experience. Therefore, they might be better equipped to use the money for investments and savings rather than consumption. In that case, the bias would decrease the estimated effect size of receiving "training, no grant" on the consumption indicator.

To address this potential bias, we include a number of geographic control variables as outlined above. We assume that after controlling for these geographic characteristics the distance to the KCB bank is uncorrelated with market access, access to banking services and any other spatial characteristics that might bias the results. In the following, we present multiple tests to support this assumption.

First, we follow the suggestion in Goldsmith-Pinkham et al. (2020) to test for correlation of KCB distance with covariates. We do this in form of a balance test between "distant" and "close" participants and report results in Appendix Table C.3.4. Goldsmith-Pinkham et al. (2020) note that correlation of the exposure variable with covariates is *per se* not problematic. It becomes problematic if the covariate might affect outcomes via the same channel (*ex post* treatment group) as the instrument. While we control for some potential confounders – such as distance to the closest city center – directly via the inclusion of our set of geographic controls, some risk of endegeneity remains. In particular, it would be concerning if after conditioning on controls, general access to commercial banking services differed between participants close or distant to a KCB bank branch. To run the balance test, we convert the continuous distance measure to a binary variable that takes a value of 1 for any distance larger than the 75 percentile of 59 km.[13] We present results in Appendix Table C.3.3. Columns (1) and (2) display baseline means and standard deviations for the "close" group. Columns (3)

[13]The 75 % percentile was selected based on testing the percentiles 25, 50 and 75 against their predictive power for our first stage regression.

and (4) report results on regressing baseline characteristics on the "distant" dummy conditional on state-fixed effects and columns (5) and (6) repeat this exercise while including our set of geographic control variables. The results from the first test indicate a number of statistically significant differences between "close" and "distant" endline participants. "Distant" participants are more likely to own a business, have a lower nonfood consumption and are less likely to have a university degree. Moreover, they are on average 1.9 km further away from any bank branch (not just KCB branches), 8.5 km further away from any city center,[14] and 3.4 km further away from any major road.[15] A joint test of orthogonality shows a highly significant difference between the groups. Results from columns (5) and (6) show that the inclusion of the geographic control variables removes the statistically significant differences. None of the baseline covariates shows a statistically significant coefficient and the joint test of orthogonality does not reject the null hypothesis. Importantly, the difference in distance to any bank branch goes down to 45 m[16] and loses statistical significance. Thus, financial inclusion – as main channel through which the common shock affects the outcomes – appears balanced across groups. Taken together, these results suggest that conditional on geographic covariates, the instrument is uncorrelated with the most obvious sources of endogeneity.

Second, we test whether levels of outcomes differed between "distant" and "close" participants of the control group. Christian & Barrett (2017) suggest to test the validity of the instrument by comparing pre-trends in outcomes. Since this is not possible in our setting, we compare *levels* of outcomes instead. While balance in levels of outcomes is not necessary for the exclusion restriction (Christian & Barrett 2017, Goldsmith-Pinkham et al. 2020), the absence of *level* differences provides suggestive evidence against the existence of unobserved omitted determinants of outcome *changes*. The results are reported in Appendix Table C.3.4. They show no statistically significant differences in outcome indices between participants located distant or close to a KCB bank branch. All but one point estimate remain under the threshold of 0.2 standard deviations, which would indicate an economically relevant effect size (Cohen 1988). A joint test of orthogonality yields no statistically significant difference between the groups.

Third, we build on Christian & Barrett (2017)'s suggestions to employ placebo tests for assessing the validity of the instrument. In particular, we run a placebo test of the first-stage equations III.3 and III.4 by replicating

[14] $8.5 = (e^{2.251}) - 1$
[15] $3.4 = (e^{1.489}) - 1$
[16] $0.045 = (e^{0.044}) - 1$

Table III.3 while replacing distance to the closest KCB bank branch with distance to any bank branch. If the instrument is valid, the interaction of treatment group assignment and distance to any bank branch should not predict *ex post* treatment group assignment. Being selected for the treatment group should only have differential effects on the likelihood to accessing the grant based on the transaction costs of doing so (proxied by distance to a KCB branch). The results of this test are reported in Appendix Table C.3.5. The table shows results on regressions without controls (columns (1) and (2)), with baseline controls (columns (3) and (4)), and with baseline controls and geographic controls as our preferred specification (column (5) and (6)). The coefficients on the interaction between treatment group assignment and bank distance are close to zero and statistically insignificant across all specifications. The Kleibergen-Paap F-statistics are also close to zero. Thus, our instrumental variable does not simply capture the effect of access to banking services.

III.5.5 Minimum detectable effects

To assess the risk of a type II error, we computed the minimum detectable effect (MDE) size of the ITT and the LATE estimates. This approach has found wide acceptance in the randomized controlled trials literature to deal with null findings (Haushofer & Shapiro 2016, Duflo et al. 2007, e.g.). For *ex post* calculations for a power of 80% and a significance level of 5%, the MDE is given by the simple formula (Samii 2014):

$$MDE = 2.8 \times SE(\hat{\beta}). \tag{III.5}$$

We use this formula to derive MDEs for all main outcomes of the ITT and LATE estimations.

III.5.6 Attrition

Due to resource constraints, we did not interview all baseline participants at the endline. Thus, the study suffers from a high attrition rate. While we intended randomization at this stage, participation in the endline survey might still be endogenous. We find some systematic differences between attritors and endline participants (Appendix Table C.3.6). Attritors were more likely to be female, had higher food consumption, larger formal and lower informal debt, came from smaller households and were less educated. Importantly, conflict exposure does not differ significantly between attritors and non-attritors.

However, the difference in endline participants and attritors does not undermine the causal interpretation of our treatment effects. We find no evidence that attrition depended on the selection for the original treatment group (Appendix Table C.3.7), nor differential attrition across covariates between these two groups (Appendix Table C.3.8). There is no evidence that participants in the control group accessed either the training or the grants. The low geographic concentration of program participants makes spill-over effects unlikely. Hence, control group outcomes can plausibly be regarded as counterfactual outcomes for endline treatment group participants.

We address the high attrition rate in two ways. First, we derive Lee bounds[17] for ITT estimates to get a better notion of the effect size for the original program target population. Second, we re-weight our main regressions based on the inverse probability to be sampled at the endline. We follow Doyle et al. (2017) by calculating inverse probability weights based on baseline characteristics.[18]

III.6 Results

III.6.1 ITT estimates on average program outcomes

Table III.4 reports results for average program outcomes on socio-economic indicators. We find a large and statistically significant effect of the program on the savings, debt and investment index. On average, participants that were randomly selected to the treatment group increased their savings, debt and investment index by 0.27 standard deviations. Even after p-value adjustment, the effect is significant at the 99 percent level. This finding is in line with existing evidence on the effects of cash transfers and their effect on savings (e.g., Banerjee et al. 2017). The effect is partly mechanical, since bank account ownership and bank account balance enter positively into the index, while both being directly affected by accessing the grant. Therefore, the positive effect on the savings, debt and investment index is expected. However, it shows that potential financial loss of “training, no grant” participants – which were approximately twice as many – did not outweigh the

[17]A procedure for bounding average treatment effects in presence of sample selection proposed (Lee 2009).

[18]New approaches suggest using information on tracking efforts to generate sampling weights. This approach is based on the assumption that difficult-to-track endline participants are more similar to attritors than easy-to-track participants (Molina Millán & Macours 2017). However, in our study we do not find that difficult-to-track participants share more characteristics with attritors than the average endline participant (compare Tables C.3.9 and C.3.6).

Table III.4: Intention-to-treat effects of the original intervention on main socio-economic outcomes

	(1) ITT (no controls)	(2) ITT (controls)
Main outcomes -– Socioeconomic		
Employment index	0.063	0.067
	(0.281)	(0.242)
	[0.375]	[0.323]
Consumption index	0.094	0.086
	(0.12)	(0.153)
	[0.240]	[0.307]
Savings, investment and debt index	0.274***	0.271***
	(0.000)	(0.000)
	[0.001]	[0.001]
Business skills index	0.016	0.018
	(0.747)	(0.735)
	[0.748]	[0.735]
Observations	1,523	1,495

Notes: All regression control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level. Adjusted Benjamini-Hochberg p-values are reported in square brackets.

financial gains of "training and grant" participants.

For the remaining socio-economic indicators, we find no statistically significant effects. Notably, we find no improvement in business skills. This is surprising given that both *ex post* treatment groups received the training. The derived *MDE* of $0.139 - 0.148$ for ITT estimates shows that the null result is not caused by a lack of power (Appendix Table C.5.1). While business skills trainings show relatively mixed results in the literature, it is not clear what hampered the effectiveness of the training in the context of our study.

ITT estimates for psychological and behavioral indicators are reported in Table III.5. All point estimates are below 0.1 standard deviations. Except for the crime and violence index, all results are also statistically insignificant. In the specification with control variables, the crime and violence index shows a reduction of 0.09 standard deviations that is weakly significant at the 10 percent level.

Table III.5: Intention-to-treat effects of the original intervention on main psychological and behavioral outcomes

	(1) ITT (no controls)	(2) ITT (controls)
Main outcomes — Psychological and behavioral		
Psychological wellbeing index	-0.009	0.002
	(0.845)	(0.965)
	[0.845]	[0.965]
Risk index	-0.043	-0.052
	(0.501)	(0.383)
	[0.741]	[0.639]
Trust index	-0.035	-0.055
	(0.482)	(0.274)
	[0.741]	[0.639]
Crime and violence index	-0.080	-0.089*
	(0.119)	(-0.090)
	[0.595]	[0.450]
Migration index	-0.026	-0.015
	(0.593)	(0.767)
	[0.741]	[0.960]
Observations	1,523	1,495

Notes: All regression control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level. Adjusted Benjamini-Hochberg p-values are reported in square brackets.

III.6.2 LATE estimates on *ex post* treatments "training and grant" and "training, no grant"

Tables III.6 and III.7 report results for LATE estimations. Both tables show results from specifications without control variables, with baseline control variables and with a combination of baseline and geographic control variables, while we only assume the later to be valid as outlined above. The Kleibergen-Papp F-statistics also show that only this specification is adequately powered. Thus, we focus mainly on a description of the last three columns of both tables.

Despite the employment focus of the intervention, we find no statistically significant effect on the employment index on either *ex post* treatment group. Both groups show a positive coefficient of similar size (0.23 standard deviations for the "training, no grant" group and 0.28 standard deviations for

Table III.6: Local average treatment effects of the "training and grant" vs. "training, but no grant" on main socio-economic outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(no controls)		(controls)		(controls + geography controls)		
	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	(5) - (6)
			Main outcomes — Socio-economic				
Employment index	-0.069 (0.770) [0.766]	0.369 (0.399) [0.590]	-0.050 (0.820) [0.988]	0.338 (0.397) [0.626]	0.231 (0.325) [0.553]	0.286 (0.351) [0.553]	-0.056 (0.902) [0.902]
Consumption index	-0.389** (0.034) [0.073]	1.042** (0.044) [0.073]	-0.350** (0.036) [0.077]	0.933** (0.034) [0.077]	-0.411** (0.014) [0.040]	0.708** (0.036) [0.081]	-1.119*** (0.004) [0.008]
Savings index	-0.166 (0.294) [0.551]	1.282*** (0.004) [0.005]	-0.171 (0.285) [0.556]	1.270*** (0.002) [0.005]	-0.133 (0.483) [0.639]	1.094*** (0.001) [0.005]	-1.227*** (0.001) [0.003]
Business skills index	-0.113 (0.530) [0.595]	0.267 (0.455) [0.590]	0.003 (0.988) [0.988]	0.046 (0.903) [0.988]	0.038 (0.804) [0.918]	-0.029 (0.927) [0.928]	0.066 (0.862) [0.902]
Observations	1,500		1,474		1,474		
F-stat	5.372		5.090		14.844		

Notes: All regression control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level. Adjusted Benjamini-Hochberg p-values are reported in square brackets.

the "training and grant" group) which suggests a small, but relevant effect size. These results are inconclusive since the estimation was only powered to detect large effect sizes (Appendix Table C.5.2). However, it seems unlikely that the program cancellation created *negative* effects on employment, since the estimated difference between the treatments is close to zero.

Moreover, we find a heterogeneous effect on consumption. While participants that received the cash grant increased their consumption by 0.7 standard deviations, participants that failed to receive their cash grant show a reduction in consumption by 0.4 standard deviations. Both effects are statistically significant after p-value adjustment. The negative consumption effect for "training, no grant" participants is economically relevant and suggests resource misallocation due to the program failure. It provides first evidence that the program created worse outcomes for this *ex post* treatment group than in the absence of the program. The positive consumption effect of the "training and grant" group is not surprising. The existing literature on cash grants typically finds large consumption effects (Manley et al. 2013, Haushofer & Shapiro 2016).

For savings, the LATE results show that the positive ITT effect is driven

Table III.7: Local average treatment effects of the "training and grant" vs. "training, but no grant" on main psychological and behavioral outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(no controls)		(controls)		(controls + geography controls)		
	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	(5) - (6)
	Main outcomes — Psychological and behavioral						
Psychological wellbeing index	-0.238	0.397	-0.080	0.131	-0.217*	0.225	-0.442
	(0.173)	(0.249)	(0.617)	(0.674)	(0.076)	(0.377)	(0.136)
	[0.312]	[0.312]	[0.773]	[0.773]	[0.343]	[0.595]	[0.341]
Risk index	-0.441	0.702	-0.408	0.605	-0.256	0.507	-0.764
	(0.126)	(0.215)	(0.113)	(0.239)	(0.426)	(0.233)	(0.255)
	[0.312]	[0.312]	[0.351]	[0.385]	[0.595]	[0.595]	[0.426]
Trust index	-0.020	-0.098	-0.020	-0.153	-0.412***	0.115	-0.527
	(0.922)	(0.796)	(0.924)	(0.697)	(0.006)	(0.677)	(0.124)
	[0.921]	[0.880]	[0.924]	[0.773]	[0.040]	[0.675]	[0.341]
Crime and violence index	-0.470	0.578	-0.554*	0.692	-0.167	0.282	-0.449
	(0.122)	(0.269)	(0.071)	(0.193)	(0.472)	(0.412)	(0.367)
	[0.312]	[0.312]	[0.351]	[0.370]	[0.595]	[0.595]	[0.443]
Migration index	-0.258	0.449	-0.292	0.543	-0.126	0.188	-0.314
	(0.142)	(0.243)	(0.114)	(0.166)	(0.518)	(0.539)	(0.443)
	[0.312]	[0.312]	[0.351]	[0.370]	[0.595]	[0.595]	[0.443]
Observations	1,500		1,474		1,474		
F-stat	1.886		5.090		14.844		

Notes: All regression control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level. Adjusted Benjamini-Hochberg p-values are reported in square brackets.

by the "training and grant" group. This is not surprising. Participants that received the grant improved their savings indicator by more than 1 standard deviation. Under p-values adjustment, the result is still highly significant at the 1 percent level. In contrast, we find a negative but statistically insignificant effect for participants that received "training, no grant."

What is more, we find no improvement in business skills neither in the group "training and grant" nor in the group "training, no grant." The difference between the point estimates is close to zero. This suggests that the lack of improvement in business skills had nothing to do with the cancellation of the grant disbursement.

LATE results on psychological and behavioral outcomes are reported in Table III.7. For general psychological wellbeing, we find a weakly significant reduction of 0.2 standard deviations for the group that failed to receive the grant. However, the effect is only significant at the 10 percent level without p-value adjustment. For the "training and grant" group the results show a positive effect of 0.2 standard deviations that fails to reach statistical significance. The regression was powered to detect a small effect of 0.33 standard deviations for "training, no grant," while for "training and grant" it is powered to detect only a large effect of 0.7 standard deviations (Appendix Table C.5.2). Thus, the statistical power might be too low to detect positive psychological effects of the cash transfer that can typically be found in the literature (Haushofer & Shapiro 2016, Ozer et al. 2011).

In addition, we find a 0.4 reduction in the trust index for participants that failed to receive their grant. With adjusted p-values, the results are significant at the 5 percent level. The finding is in line with our expectation that the program cancellation led to negative psychological repercussions. We find no effect on trust for the "training and grant" group.

For all other psychological and behavioral outcomes, we find no statistically significant effects on either subgroup.

III.6.3 LATE estimates on *ex post* treatments "training and grant" and "training, no grant" by gender

Next, we split the sample across gender and report all estimates for both subsamples separately. We also test whether point estimates of female and male participants differ significantly by means of a Wald-test. Our sample consists of 547 men and 948 women, out of which 85 men received "training and grant," 133 received "training, no grant," 199 women received "training and grant," and 277 received "training, no grant."

Socio-economic outcomes of LATE estimates by gender are reported in Table III.8. While we find no employment effect on either *ex post* treatment group when genders are polled, the gender split suggests a difference of the program effects between genders. Women that received "training, no grant" show a weakly significant increase of employment by 0.48 standard deviations. Women that received "training and grant" show a statistically insignificant increase of similar size. In contrast, male participants of both *ex post* treatments show smaller effect sizes. Given the statistically significant positive effect on women that did not receive the grant, a potential employment effect seems to be linked to the training, not the grant. Due to low statistical power, these results are only suggestive.

For consumption, we find that the negative consumption effect of the "training, no grant" group is larger for women than for men (0.9 standard deviations for women compared to 0.14 standard deviations for men) suggesting that the program cancellation hit women more heavily. The positive coefficients on men and women that received "training and grant" are of similar size.

Table III.8: Effects of the "training and grant" vs. "training, but no grant" on main socio-economic outcomes by gender

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		LATE for males			**LATE for females**			Coeff equality (3) vs. (6)
		(no controls)	(controls)	(controls + geo controls)	(no controls)	(controls)	(controls + geo controls)	
Employment index	"Training, no grant"	0.105	-0.001	0.173	-0.131	-0.049	0.488*	0.316
		(-0.781)	(0.998)	(0.723)	(0.581)	(0.807)	(0.081)	(0.506)
	"Training and grant"	-0.040	0.074	0.221	0.637	0.456	0.369	0.148
		(0.952)	(0.901)	(0.625)	(0.191)	(0.284)	(0.289)	(0.791)
Consumption index	"Training, no grant"	-0.323	-0.319	-0.139	-0.374*	-0.303	-0.899**	-0.759
		(0.307)	(0.327)	(0.587)	(0.067)	(0.133)	(0.025)	(0.161)
	"Training and grant"	0.616	0.521	0.337	1.241**	1.073**	0.460	0.123
		(0.302)	(0.361)	(0.431)	(0.025)	(0.036)	(0.192)	(0.828)
Savings, investment, and debt index	"Training, no grant"	-0.429	-0.450	-0.407	-0.049	-0.021	-0.228	0.179
		(0.203)	(0.174)	(0.116)	(0.768)	(0.898)	(0.402)	(0.614)
	"Training and grant"	1.715***	1.603***	1.212***	0.975**	0.903**	0.632*	-0.581
		(0.008)	(0.008)	(0.008)	(0.013)	(0.019)	(0.064)	(0.306)
Business skills index	"Training, no grant"	-0.046	-0.035	0.681**	-0.167	-0.016	-0.109	-0.790
		(0.895)	(0.929)	(0.011)	(0.406)	(0.941)	(0.780)	(0.122)
	"Training and grant"	0.340	0.320	0.427	0.250	-0.090	-0.171	-0.598
		(0.551)	(0.603)	(0.373)	(0.584)	(0.846)	(0.641)	(0.369)
Observations		547	541	541	953	933	933	
F-stat		4.510	4.568	15.53	5.330	5.268	8.486	

Notes: All regression control for state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

The positive savings effect for participants that received the grant is larger for men than for women (1.2 standard deviations versus 0.6 standard deviations). However, the difference is not statistically significant.

For business skills, we find a statistically significant increase of 0.68 standard deviations for men that received "training and grant." The coefficient for men of the "training and grant" group is also positive at 0.42 standard deviations, but fails to reach statistical significance. A Wald-test shows no statistically significant difference between the groups. For women, coefficients for both *ex post* treatment groups are small and negative. These results suggest that the training failed to increase business skills among women, while for men the results are inconclusive.

Turning to psychological and behavioral outcomes (Table III.9), we find that the reduction in trust for the "training and grant" group is larger among women. Point estimates for women show a reduction of 0.52 standard deviation reduction in the trust index, significant at the 5 percent level (compared to a 0.13 reduction for men). An estimate of the difference between men and women however fails to reach statistical significance. The coefficient for women that received the grant is close to zero. Results for men that did receive the grant are inconclusive. The coefficient suggests a medium positive effect (0.424 standard deviations), but the results are statistically insignificant due to the small sample size for male participants.

Table III.9: Effects of the "training and grant" vs. "training, but no grant" on main psychological and behavioral outcomes by gender

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		LATE for males			**LATE for females**			Coeff equality
		(no controls)	(controls)	(controls + geo controls)	(no controls)	(controls)	(controls + geo controls)	(3) vs. (6)
Psychological wellbeing index	"Training, no grant"	-0.013	0.089	-0.356	-0.370**	-0.153	-0.182	0.174
		(0.969)	(0.802)	(0.294)	(0.042)	(0.342)	(0.478)	(0.729)
	"Training and grant"	0.257	0.075	0.201	0.527	0.130	0.194	-0.007
		(0.618)	(0.885)	(0.620)	(0.290)	(0.757)	(0.560)	(0.991)
Risk index	"Training, no grant"	-0.371	-0.438	0.286	-0.47	-0.366	-1.156**	-1.442***
		(0.306)	(0.238)	(0.401)	(0.113)	(0.147)	(0.025)	((0.001)
	"Training and grant"	0.577	0.639	0.129	0.792	0.605	0.601	0.472
		(0.333)	(0.290)	(0.755)	(0.235)	(0.294)	(0.154)	(0.319)
Trust index	"Training, no grant"	-0.095	-0.133	-0.132	-0.012	-0.053	-0.521**	-0.389
		(0.792)	(0.729)	(0.747)	(0.948)	(0.767)	(0.044)	(0.486)
	"Training and grant"	0.210	0.177	0.424	-0.277	-0.278	0.028	-0.396
		(0.679)	(0.734)	(0.309)	(0.557)	(0.545)	(0.937)	(0.498)
Crime and violence index	"Training, no grant"	-0.371	-0.434	-0.107	-0.543*	-0.642**	-0.419	-0.312
		(0.332)	(0.306)	(0.763)	(0.092)	(0.044)	(0.297)	(0.508)
	"Training and grant"	0.625	0.669	0.247	0.614	0.745	0.200	-0.046
		(0.324)	(0.285)	(0.642)	(0.340)	(0.253)	(0.611)	(0.942)
Migration index	"Training, no grant"	0.048	0.010	0.232	-0.376**	-0.403**	-0.348	-0.580
		(0.866)	(0.975)	(0.571)	(0.040)	(0.026)	(0.326)	(0.342)
	"Training and grant"	-0.181	-0.008	-0.082	0.835	0.846	0.324	0.405
		(0.693)	(0.986)	(0.836)	(0.126)	(0.106)	(0.426)	(0.478)
Observations		547	541	541	953	933	933	
F-stat		4.510	4.568	15.53	5.330	5.268	8.486	

*Notes:*All regression control for state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Moreover, women that failed to receive the cash grant show a large and statistically significant reduction in their risk index of more than 1 standard deviation. Here, the difference between genders is large at 1.44 standard deviations and highly significant at the 1 percent level. This finding could equally be driven by the fact that stakes were higher for female participants due to the high social costs of participating in the program.

For the other psychological and behavioral outcomes, we find no statistically significant effects across genders. The heterogenous effect on trust and risk aversion could be linked to the existing gender norms in South Sudan. Qualitative evidence from a focus group during the design stage of the program suggests negative connotations with female employment. Women that go against these norms face social costs. Consequently, the stakes of the program might have been higher for women. Failure to access the grant might not only have resulted in financial loss, but also created sunk costs related to the social costs. Therefore, the perception of being let down by the program might have been larger for women resulting in a loss of trust and higher risk aversion.

III.6.4 Robustness

Our study has to deal with a high degree of attrition (approximately 34 % of the baseline survey), due to the difficulties of tracking endline participants during the on-going conflict. Therefore, we test the robustness of our ITT estimates by calculating upper and lower bounds following Lee (2009). These correct for attrition by making extreme assumptions about missing information. We report the results in Appendix Tables C.6.1 and C.6.2. Even after extreme assumptions about attritors the ITT effect on the savings, investment and debt index remains statistically significant – overall, savings increased among those assigned to receive training and grants by about 0.26 – 0.27 standard deviations. The effect on the crime and violence index does not remain robust. The lee bounds derive an upper bound of -0.1 standard deviations that is statistically insignificant.

In addition, we address potential attrition bias by re-weighting observations based on their inverse probability to be in the endline survey (See Section III.5.6 for details on the methodology). Results are reported in Appendix Tables C.6.3 to C.6.8.

For the ITT estimates, the weighted regressions confirm a positive average effect on the savings index. The weakly negative effect on the crime and violence index loses statistical significance indicating that the main estimate might be partly due to attrition bias.

Turning to results that distinguish between "training and grant" and "training, but no grant," the re-weighting exercise confirms our previous results. The weighted regressions find a positive statistically significant effect on consumption and savings for participants that received training and grant. Moreover, the weighted regression shows a statistically significant negative impact on consumption for participants that received only the training, but not the grant. In the set of psychological and behavioral outcomes, we find a reduction in trust for participants that only received the training, but not the grant. In addition, the results confirm a weakly significant negative effect on general psychological well-being for this subgroup. The gender analysis confirms the heterogenous effect on risk aversion. Women that failed to access the grant show a reduction of the risk index of 1.2 standard deviations (Appendix Table C.6.8).

Finally, we test the robustness of our LATE estimates against alternative measures of conflict. Our primary measure of conflict exposure is based on geo-referenced data by UCDP (Sundberg & Melander 2013). It consists of the average number of fatalities within a 300 km radius weighted by geographic proximity to participants' baseline location. In addition, we generate an alternative measure based on the number of conflict events. We count every incidence of violence as a conflict event if it caused 5 or more fatalities. Then, we derive the average number of conflict events within a 300 km radius weighted by geographic proximity. We generate similar measures for fatalities and conflict events using the Armed Conflict Location and Event Data (ACLED) (Raleigh & Karlsen 2010).

The validity of the instrument requires that the difference in distance to the closest KCB bank branch is not correlated with another geographic variable that might drive the results. Controlling for conflict exposure is therefore essential for the identification of the LATE. In Tables C.6.9 – C.6.13 we replicate our preferred specification of Tables III.6 – III.9. In all tables, columns (1) and (2) are replications of columns (5) and (6) of the respective main results table using the number of fatalities from UCDP conflict data as conflict measure. In columns (3) and (4), we use the same data source, but calculate the number of conflict events in a location by counting every incidence of violence as a conflict event where 5 or more fatalities occurred. In columns (5) and (6) we use the number for fatalities based on ACLED data, in columns (7) and (8) we use the number of conflict events based on ACLED data.

Results on ITT estimates largely confirm our main findings. The positive effects on consumption and savings for the "training and grant" group is robust to the different conflict measures (Appendix Table C.6.9). For LATE estimates, the negative effects for "training, no grant" on consumption, psychological wellbeing and trust, as well as the positive savings effect of "training and grant" persist across specifications (Appendix Tables C.6.10 and C.6.11).

For LATE effects by gender, the results confirm the positive effect in the savings indicator for men that received the grant and the negative consumption effect for women that failed to receive the grant (Table C.6.12). Moreover, the positive effect on business skills for men that did not receive the grants remains robust. For psychological indicators, the negative effect on the risk index for women that failed to access their grants remains robust (Table C.6.13). In contrast, the negative impact on trust for this subgroup is only robust in one of the three alternative specifications. Since the loss in trust is robust when genders are pooled, this suggests that the loss in trust was not gender-specific.

III.7 Discussion and conclusion

Our study used the example of the unplanned cancellation of the South Sudan Youth Business Start-Up Grant Program to evaluate the impact of unintended operational problems on intended beneficiaries. Overall, our results suggest that the impact of a failed intervention is mixed and depends on the gender of participants and their *ex post* treatment status. In this instance, on average across all participants, the invention was largely ineffective. Most socio-economic or psychological and behavioral indicators neither worsened nor improved.

However, when considering *ex post* treatment groups by gender, some groups were detrimentally affected by the intervention. In particular, participants who failed to access the grant showed a reduction in their consumption index and their trust level. This is a clear indication that the program cancellation made some participants both economically and psychologically worse off than in the absence of the program. The negative effect on trust is no surprise given that the perception of government performance has been found to correlate strongly with both institutional and interpersonal trust (Murtin et al. 2018). Low levels of trust have been associated with inefficiently low levels of individual trade (Binzel & Fehr 2013, Kuran 2018). Thus, operational problems bear the risk to undermine development persistently by negative effects on trust. This underscores the need to build in mitigating features into development programs in risky environments.

What is more, female participants that failed to receive the grant showed a reduction in their risk tolerance. The gender-specific effect can be explained by higher social costs of participating in the program. Women's labor outside the home is still subject to negative social norms. Exposure to larger risk has been consistently associated with an increase in risk aversion (Callen et al. 2014, He & Hong 2018, Jakiela & Ozier 2019, Brown et al. 2019). Policymakers must be aware that these larger social cost can manifest in higher loss of risk tolerance for women when operational problems occur. Therefore, mitigating features must be sensitive to gender.

Nevertheless, the invention created some positive impacts among participants that received the originally planned treatment. In particular, savings and consumption increased for this *ex post* treatment group. Although the group that received the grant was smaller than the group that only received the training, the positive impacts on the savings indicator was large enough to lift the average effect above a statistically significant level, but not for the consumption indicator. Even for this subgroup, however, the intervention cannot be deemed fully effective, since we did not find any significant improvement in employment or business skills. When analyzing these outcomes by gender, it seems that the business skills training was more effective in men than in women. Future programs need to ensure to target training content better to female participants.

Our analysis is limited by the following factors. First, the study had to deal with a high degree of attrition due to resource constraints and the impossibility for enumerators to enter the center parts of the country. Despite our attempt to address sample selection, positive effects on the "training and grant" might not have been so large for participants residing in the most conflict afflicted territories. Second, we had to rely on an instrumental variable approach to estimate the effect of the two *ex post* treatments. While we deem our instrument valid, the effect is specific to participants that failed to access the grant due to high transaction costs (proxied by distance to the respective bank branch). This approach does not tell us the effect of the program cancellation on participants that failed to receive their grants, but lived close to a bank branch. While these might have had lower intrinsic motivation to use the grant, the estimated LATE is therefore not representative of the full sample of intended beneficiaries. More studies on implementation problems or cancellations are therefore warranted.

This paper is the first study that shows how failed intervention can have a negative impact on intended beneficiaries. Both the loss in consumption and trust as well as the increase in female risk aversion should warn policy-makers to pay more attention to unintended damage from failed interventions. Since we find suggestive evidence that these negative effect differed across genders, the external validity of the result should be confirmed by further research on failed inventions and heterogeneous effects across gender. Although most indicators showed no significant net improvements, participants that did receive the treatment as intended seemed to benefit economically. While it remains to be argued whether these positive impacts outweigh the negative impacts, our study emphasizes the importance of considering consequences of potential failures in the planning stages to mitigate potential detrimental impacts in the case of program failure.

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Appendix

A. Appendix of Democracy and Aid Donorship

A.1 Survey questions

Question 1

1a. Does your country (currently or in the past) provide development cooperation[1] to any other countries?

☐ yes ☐ no

1b. If yes, when did your country first provide development cooperation to another country?
Year:
Comments (if any):
—The following questions only apply if you replied yes to question 1a—

Question 2

2a. Does your country currently have (at least) one administrative body that is responsible for providing development cooperation to other countries? This could be a unit or division in the Ministry of Foreign Affairs, another ministry or government unit, or an independent agency.

☐ yes ☐ no

2b. If yes, please name the leading institution(s) and year(s) this responsibility was adopted:
Name(s):
Year(s):
Comments (if any):

Question 3

3a. In the history of your country, did the responsibility of providing development cooperation lay with another administrative body?

☐ yes ☐ no

3b. If yes, please name the leading institution(s) and year(s) this responsibility was adopted:
Name(s):

[1] "Development cooperation" should be broadly understood as including grants, concessional loans, technical assistance and in-kind assistance the main objective of which is the promotion of the economic development and welfare of another country.
This does NOT include: military equipment or services, anti-terrorism activities or humanitarian aid.

Year(s):
Comments (if any):

Question 4

4a. Does your country (currently or in the past) have legislation to govern its development co-operation?

☐ yes ☐ no

4b. If yes, what is/are the name(s) of the corresponding law(s) or regulation(s)?
Name(s):
4c. When did your country first introduce legislation to govern its development co-operation?
Year(s):
Comments (if any):

A.2 Additional tables and figures

Table A.2.1: Year of aid initiation by country

Country (ISO)	World region	First aid delivery	Year	Source	First aid institution	Year	Source
AFG	SA	No			No		
AGO	SSA	Yes	1976		Yes	2008	
ALB	ECA	No			No		
ARE	MENA	Yes	1970	Web/Literature	Yes	2008	Web/Literature
ARG	LAC	Yes	1992	Web/Literature	Yes	2003	Web/Literature
ARM	ECA	No			No		
AUS	EAP	Yes	1950	Web/Literature	Yes	1974	Web/Literature
AUT	ECA	Yes	1956		Yes	1974	
AZE	ECA	Yes	2011	Web/Literature	Yes	2011	Web/Literature
BEL	ECA	Yes	1962		Yes	1962	
BEN	SSA	Yes	1960		NA		
BFA	SSA	No			No		
BGR	ECA	Yes	1961	Web/Literature	Yes	2007	Web/Literature
BHR	MENA	Yes	NA	Web/Literature	NA		
BIH	ECA	No			No		
BLZ	LAC	No			No		
BRA	LAC	Yes	1969	Web/Literature	Yes	1969	Web/Literature
BRN	EAP	Yes	1985		NA		
BTN	SA	No			No		
CAN	NA	Yes	1950		Yes	1960	
CHE	ECA	Yes	1961		Yes	1961	
CHL	LAC	Yes	1993		Yes	1993	Web/Literature
CHN	EAP	Yes	1950	Web/Literature	Yes	1960	
CIV	SSA	No			No		
COG	SSA	Yes	1960		NA		Web/Literature
COL	LAC	Yes	1982	Web/Literature	Yes	1982	
CRI	LAC	Yes	1997		No		Web/Literature
CUB	LAC	Yes	1959	Web/Literature	NA		
CYP	ECA	Yes	2005	Web/Literature	Yes	2005	
CZE	ECA	Yes	1993	Web/Literature	Yes	2008	
DEU	ECA	Yes	1952		Yes	1961	
DJI	MENA	Yes	1977		Yes	1977	
DNK	ECA	Yes	1962		Yes	1962	
DZA	MENA	Yes	NA	Web/Literature	No		
ECU	LAC	Yes	2006		Yes	2007	
EGY	MENA	Yes	1980		Yes	1980	
ESP	ECA	Yes	1976		Yes	1985	
EST	ECA	Yes	1998		Yes	1998	
FIN	ECA	Yes	1965		Yes	1965	
FRA	ECA	Yes	1959	Web/Literature	Yes	1961	Web/Literature
GBR	ECA	Yes	1961		Yes	1964	
GHA	SSA	No			No		
GIN	SSA	No			No		
GNB	SSA	Yes	1976		No		
GRC	ECA	Yes	1997		Yes	2002	
GTM	LAC	No			No		
GUY	LAC	No			No		
HND	ECA	Yes	2011		Yes	2012	
HRV	ECA	Yes	1993		Yes	2008	
HUN	EAP	Yes	1956	Web/Literature	Yes	1962	Web/Literature

Table A.2 continued from previous page

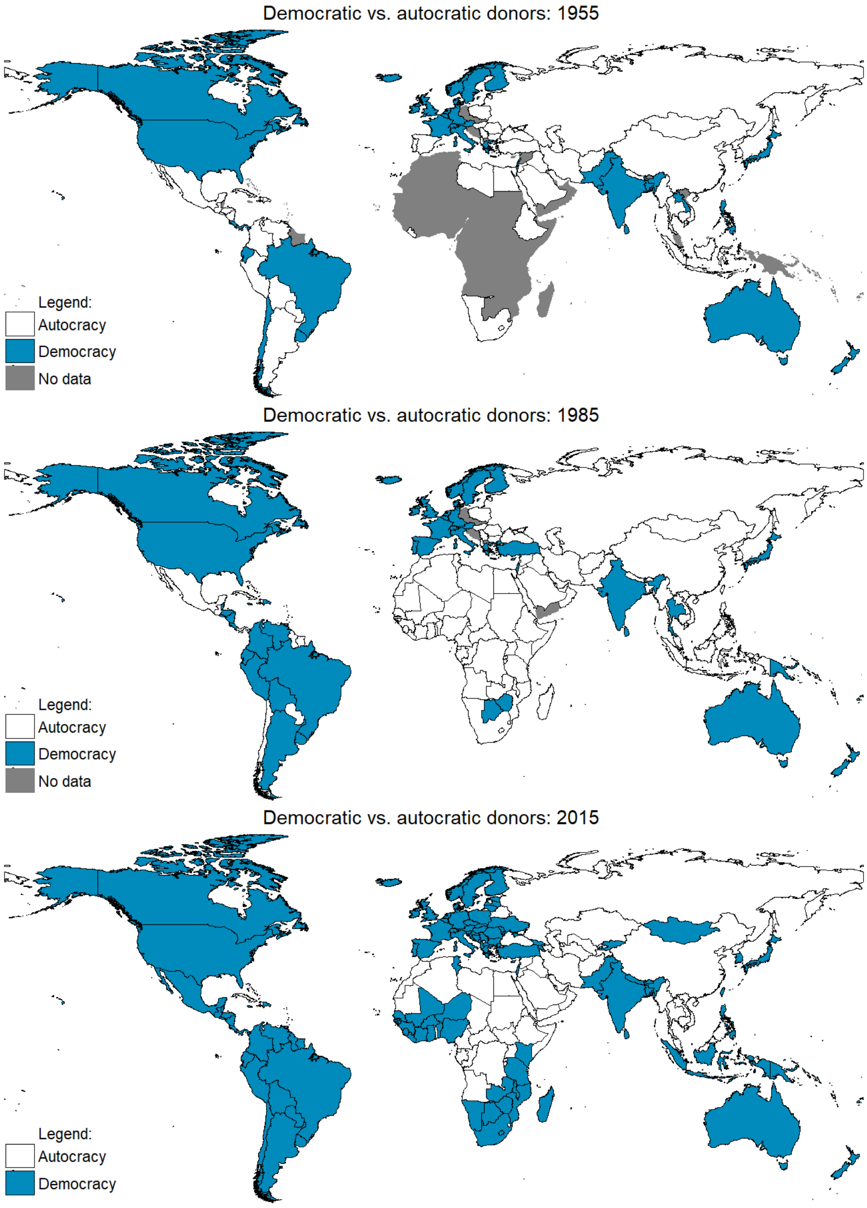
Country (ISO)	World region	First aid delivery	Year	Source	First aid institution	Year	Source
IDN	SA	Yes	1981	Web/Literature	No		Web/Literature
IND	ECA	Yes	1959	Web/Literature	Yes	1964	Web/Literature
IRL	MENA	Yes	1974		Yes	1974	
IRN	MENA	Yes	NA	Web/Literature	Yes	1975	Web/Literature
IRQ	ECA	Yes	1974		Yes	1974	
ISL	MENA	Yes	1980		Yes	1981	
ISR	ECA	Yes	1953	Web/Literature	Yes	1957	
ITA	LAC	Yes	1966		Yes	1987	
JAM	MENA	No			No		
JOR	EAP	No			No		
JPN	ECA	Yes	1954	Web/Literature	Yes	1954	Web/Literature
KAZ	EAP	Yes	2000		Yes	2014	
KOR	ECA	Yes	1963		Yes	1967	
KSV	MENA	No			No		
KWT	MENA	Yes	1962		Yes	1961	
LBY	SA	Yes	1968	Web/Literature	No		
LKA	SSA	Yes	2011		No		Web/Literature
LSO	ECA	No			No		
LTU	ECA	Yes	2001	Web/Literature	Yes	2004	
LUX	ECA	Yes	1982		Yes	1979	
LVA	MENA	Yes	1999		Yes	2003	
MAR	ECA	Yes	1986		Yes	1986	
MDA	SSA	No			No		
MDG	LAC	No			No		
MEX	ECA	Yes	1943		Yes	1971	
MKD	ECA	Yes	2012		No		Web/Literature
MLT	EAP	Yes	2008		Yes	2008	
MNG	SSA	Yes	2013		Yes	2013	
MWI	EAP	No			No		
MYS	SSA	Yes	1980		Yes	1980	Web/Literature
NAM	SSA	No			No		
NGA	ECA	Yes	1976	Web/Literature	Yes	1976	Web/Literature
NLD	ECA	Yes	1949		Yes	1965	
NOR	SA	Yes	1953		Yes	1952	
NPL	EAP	No			No		
NZL	SA	Yes	1951		Yes	2002	
PAK	LAC	Yes	NA	Web/Literature	Yes	NA	
PER	EAP	Yes	NA		Yes	2002	
PHL	ECA	Yes	1979		Yes	1979	
POL	ECA	Yes	1956	Web/Literature	Yes	2005	
PRT	LAC	Yes	1961		Yes	1974	
PRY	MENA	Yes	2014		No		Web/Literature
QAT	ECA	Yes	1974	Web/Literature	Yes	2008	Web/Literature
ROU	ECA	Yes	1956	Web/Literature	Yes	NA	Web/Literature
RUS	MENA	Yes	1953	Web/Literature	Yes	1957	Web/Literature
SAU	EAP	Yes	1966	Web/Literature	Yes	1974	Web/Literature
SGP	LAC	Yes	1992	Web/Literature	Yes	1992	Web/Literature
SLV	SSA	Yes	2013		Yes	2009	
SSD	ECA	Yes	2011		Yes	2011	
SVK	ECA	Yes	1999	Web/Literature	Yes	2003	
SVN	ECA	Yes	2004		Yes	2006	
SWE	SSA	Yes	1962		Yes	1965	
TCD	SSA	No			No		
TGO	EAP	No			No		

Table A.2 continued from previous page

Country (ISO)	World region	First aid delivery	Year	Source	First aid institution	Year	Source
THA	EAP	Yes	1992		Yes	2004	
TLS	MENA	Yes	2014	Web/Literature	NA		
TUN	ECA	Yes	1972		Yes	1972	
TUR	SSA	Yes	1985	Web/Literature	Yes	1992	
TZA	NA	No			No		
URY	LAC	Yes	2009	Web/Literature	Yes	2010	Web/Literature
USA	EAP	Yes	1950	Web/Literature	Yes	1955	
VEN	MENA	Yes	2005	Web/Literature	Yes	2015	
VNM	SSA	Yes	NA		No		
YEM	SA	No			No		
ZAF	SSA	Yes	1968	Web/Literature	Yes	1968	Web/Literature

Notes: See Section I.2 for a detailed description of the data-gathering process. The world regions are abbreviated as follows: Sub-Sahara Africa (SSA), East Asia and Pacific (EAP), Europe and Central Asia (ECA), Latin America and the Caribbean (LAC), the Middle East and North Africa (MENA), and South Asia (SA).

Figure A.2.1: World maps of democracy (1955, 1985 and 2015)



Note: See Section I.2 for a detailed definition of the democracy variable. Country boundaries were constructed using the Cshapes dataset (Weidmann, Kruse and Gleditsch 2010, 2016).

Table A.2.2: Correlation matrix

	Variable name	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
1	Aid donorship (broad def)	1																						
2	Aid donorship (narrow def)	0.57	1																					
3	Cold War	-0.01	-0.01	1																				
4	(log) Colony population	0.09	0.09	0.16	1																			
5	Democracy (baseline)	0.05	0.05	-0.24	0.07	1																		
6	Democracy (DD)	0.06	0.06	-0.16	0.07	0.86	1																	
7	Democracy (electoral)	0.08	0.09	-0.28	0.10	0.81	0.78	1																
8	Democracy (ethnic)	0.02	0.03	-0.06	0.10	0.27	0.24	0.35	1															
9	Democracy (instrument)	0.07	0.07	-0.35	0.10	0.61	0.61	0.61	0.26	1														
10	Democracy (Polity IV)	0.06	0.07	-0.27	0.10	0.89	0.80	0.88	0.28	0.62	1													
11	Democracy (winning coal.)	0.06	0.08	-0.12	0.12	0.76	0.70	0.78	0.29	0.50	0.80	1												
12	Duration	0.01	0.02	-0.55	-0.02	0.20	0.17	0.20	0.03	0.29	0.24	0.06	1											
13	(log) GDP per capita	0.10	0.11	-0.08	0.14	0.29	0.33	0.43	0.27	0.35	0.28	0.35	0.02	1										
14	Gov. share of GDP	0	0	-0.04	-0.09	-0.05	-0.11	-0.03	-0.05	-0.09	-0.08	-0.07	-0.12	0.08	1									
15	Intrastate terr. conflict	0.06	0.04	-0.03	0.03	-0.02	-0.01	-0.03	-0.02	-0.01	-0.02	-0.05	0.08	-0.09	-0.07	1								
16	Mil. interstate dispute	0.02	0.03	0.06	0.11	-0.01	0.02	-0.03	-0.04	0.02	-0.03	-0.01	-0.02	0.01	-0.00	0.07	1							
17	Openness	0	0.02	-0.26	-0.19	0.12	0.06	0.13	0.05	0.15	0.12	0.12	0.02	0.33	0.22	-0.10	-0.16	1						
18	Pol. distance to Russia	0.02	0.03	0.64	0.32	-0.05	0.03	-0.03	0.02	-0.09	-0.01	0.09	-0.47	0.08	-0.16	-0.02	0.00	-0.24	1					
19	Political distance to US	-0.06	-0.07	-0.50	-0.32	-0.15	-0.22	-0.15	-0.12	-0.11	-0.18	-0.25	0.48	-0.18	0.15	0.08	-0.02	0.15	-0.79	1				
20	(log) Population	0.04	0.05	-0.05	0.22	-0.04	-0.02	-0.04	-0.03	-0.04	0.02	0.01	0.24	-0.29	-0.23	0.27	0.16	-0.49	0.02	0.01	1			
21	Donor spatial lag (by geographic distance)	0.05	0.07	-0.60	-0.09	0.11	0.04	0.13	0.01	0.20	0.08	0	0.45	0.16	0.13	0.09	0	0.18	-0.59	0.43	0.17	1		
22	Donor spatial lag (by democ. peer group)	0.05	0.06	-0.31	-0.02	0.18	0.14	0.17	0	0.30	0.16	0.09	0.35	0.27	0.03	0.12	0.07	0.21	-0.31	0.25	0.03	0.53	1	
23	GDP spatial lag (by geographic distance)	0.03	0.05	-0.59	-0.09	0.07	0	0.08	0.01	0.15	0.01	-0.07	0.53	0.17	0.14	0.07	-0.01	0.13	-0.60	0.55	0.15	0.82	0.47	1
24	GDP spatial lag (by democ. peer group)	0.09	0.11	-0.16	0.17	0.29	0.32	0.29	0.14	0.50	0.22	0.23	0.13	0.62	0.02	-0.05	0.14	0.17	-0.03	-0.12	-0.09	0.40	0.46	0.37

Note: The correlation matrix is based on the estimation sample of Table I.2 , column 6.

Table A.2.3: List of variables

Variable	Definition	Source
Dependent variables		
Aid donorship (broad definition)	1 in the first year in which a country has provided development assistance to another country	Own construction (see Section I.2)
Aid donorship (narrow definition)	1 in the year during which the first administrative body has been established whose main responsibility is the management of outgoing development assistance	Own construction (see Section I.2)
(log) Aid disbursements (OECD)	Log of total annual aid disbursements at constant 2017 US$	Creditor Reporting System (OECD 2019)
(log) Aid commitments (AidData)	Log of total annual aid disbursements at constant 2011 US$	AidData Core Research Release version 3.1 (Tierney et al. 2011, AidData 2017)
Explanatory variables: baseline model		
Democracy (baseline)	1 if the country is coded as a democracy in a year	Polity IV Project (Marshall et al. 2016); Freedom House (2016); Cheibub et al. (2010), updated in Bjørnskov and Rode (2020); manual corrections as in Acemoglu et al. (2019)
(Log) GDP per capita	Log of real GDP per capita at constant 2005 US$	Penn World Tables 9.0 (Feenstra et al., 2015, 2019)
Government share of GDP	Ratio of government expenditure relative to total GDP	Penn World Tables 9.0 (Feenstra et al. 2015, 2019)
Political distance to US	Ideal point distance to the United States based on voting alignment in the United Nations General Assembly	Voeten et al. (2009), Bailey et al. (2015)
(Log) Population	Log of total population size (in millions)	Penn World Tables 9.0 (Feenstra et al. 2015, 2019)
Openness	Trade dependence of an economy measured as the sum of total exports and imports as a percentage of GDP at current national prices	Penn World Tables 9.0 (Feenstra et al. 2015, 2019)
Intrastate conflict over territory	1 if a country is involved in a territorial dispute as target or as challenger in a year	Gleditsch et al. (2002); Themnér and Wallensteen (2013)
Duration	Duration count measuring the years since entering the sample (i.e., since the end of the Second World War or since independence)	Correlates of War Database (Correlates of War Project 2017)

Explanatory variables: extensions		
Democracy (Polity IV)	Discrete ordinal score of a country's regime type on a democracy-autocracy scale based on an evaluation of that state's elections for competitiveness and openness, the nature of political participation in general, and the extent of checks on executive authority, normalized between 0 and 1	Polity IV Project (Marshall et al. 2016)
Democracy (DD)	1 if the country is coded as a democracy in a year	Cheibub et al. (2010), updated in Bjørnskov and Rode (2020)
Democracy (winning coalition)	Five-points measure based on scores for regime type, the competitiveness of executive recruitment, the openness of executive recruitment, and the competitiveness of participation, normalized between 0 and 1	Polity IV Project (Marshall et al. 2016); CNTS Data Archive (Banks and Wilson 2016)
Democracy (ethnic winning coalition)	Size of winning coalition based on ethnic groups with access to power, normalized between 0 and 1	Bormann et al. (2017)
Democracy (electoral democracy)	Index indicator that measures the value of making ruler responsive to citizens through the electoral system, normalized between 0 and 1	V-Dem (Coppedge et al. 2016)
Political distance to Russia	Ideal point distance to the Soviet Union/Russia based on voting alignment in the United Nations General Assembly	Voeten et al. (2009), Bailey et al. (2015)
Cold War	1 if year is prior to 1991	Own construction
Militarized interstate dispute	1 if a country is engaged in a militarized interstate dispute in a year	Correlates of War Militarized Interstate Disputes (v4.1) (Maoz et al. 2019; Palmer et al. 2020)
(Log) Colony population	Log of total population living in former colonies, computed based on data on colonial linkages, population data, and state independence (by state system membership)	CEPII (Mayer and Zignago 2011); Correlates of War Project (2011); Penn World Tables 9.0 (Feenstra et al. 2015, 2019)

Note: When calculating the natural logarithm of colony population, we added 1 to generate only non-negative values.

A.3 Robustness tables

Table A.3.1: Democracy, income, and aid donorship (1951-2015, first-stage regression results of Table I.2)

	(1) Model 2 Democracy	(2) Model 4 Democracy	(3) Model 4 Democracy #(log) GDP per capita	(4) Model 6 Democracy	(5) Model 6 Democracy #(log) GDP per capita	(6) Model 8 Democracy	(7) Model 8 Democracy #(log) GDP per capita
Democracy (instrument)	0.3543***	1.1246*	4.4047	1.3722**	6.5732	0.8321	1.6701
	(0.0746)	(0.6042)	(4.7293)	(0.5937)	(4.6600)	(0.6056)	(4.7006)
(log) GDP per capita	-0.0009	0.0464	0.5604	0.0435	0.5390	0.0010	0.1464
	(0.0502)	(0.0676)	(0.5552)	(0.0670)	(0.5478)	(0.0689)	(0.5558)
Democracy (instrument)		-0.0948	-0.1999	-0.1229*	-0.4521	-0.0549	0.1695
# (log) GDP per capita		(0.0708)	(0.5611)	(0.0695)	(0.5498)	(0.0716)	(0.5604)
Control variables	No	No	No	Yes	Yes	Yes	Yes
Country and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,377	5,377	5,377	5,193	5,193	4,658	4,658
Number of countries	145	145	145	140	140	126	126

Notes: The dependent variable of the first-stage regression is indicated in the column header. Results of control variables are not displayed. Standard errors are clustered at the country level and reported in parentheses.*** 1%, ** 5%, and * 10 % significance levels.

Table A.3.2: Democracy, income, and aid donorship (1951-2015, democracy measures)

	(1)	(2)	(3)	(4)	(5)	(6)
	Narrow definition			Broad definition		
	Baseline	Polity IV	DD	Baseline	Polity IV	DD
Panel A. Average effect of democracy						
Indicator	-0.0086	-0.0171	0.0045	0.0057	-0.0429	0.0212
	(0.0242)	(0.0311)	(0.0224)	(0.0249)	(0.0314)	(0.0210)
(log) GDP per capita	0.0102**	0.0098*	0.0128**	0.0124**	0.0097	0.0159***
	(0.0052)	(0.0057)	(0.0051)	(0.0058)	(0.0068)	(0.0056)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Country and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,193	4,953	5,139	4,658	4,449	4,604
Number of countries	140	134	139	126	121	125
Kleibergen-Paap F-stat	28.32	34.01	42.17	25.25	34.34	39.11
Panel B. Heterogeneous effect of democracy						
Indicator	-0.4580***	-0.3446**	-0.5751***	-0.3123***	-0.1822	-0.3033**
	(0.1274)	(0.1541)	(0.1654)	(0.1034)	(0.1123)	(0.1324)
Indicator	0.0580***	0.0422**	0.0699***	0.0408***	0.0180	0.0389**
# (log) GDP per capita	(0.0178)	(0.0212)	(0.0202)	(0.0142)	(0.0156)	(0.0161)
(log) GDP per capita	-0.0144	-0.0119	-0.0186*	-0.0047	0.0006	-0.0016
	(0.0089)	(0.0114)	(0.0103)	(0.0080)	(0.0098)	(0.0086)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Country and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,193	4,953	5,139	4,658	4,449	4,604
Number of cid	140	134	139	126	121	125
Kleibergen-Paap F-stat	11.76	11.05	17.64	13.10	15.14	19.40

Notes: The dependent variable in columns 1–3 follows our narrow definition of aid donorship. It is a binary variable that takes a value of one in the year a country establishes its first aid institution. The dependent variable in columns 4–6 follows our broad definition of aid donorship. It is a binary variable that takes a value of one in the year a country provides its first outgoing aid project. All specifications are 2SLS regressions with each democracy indicator instrumented by regional waves of democracy according to the respective indicator as described in the main text for our main specification. Peer groups for the Polity IV democracy indicator and the DD indicator are determined using values of that respective indicator at the beginning of the sample period. All regressions include all control variables as in column 5–8 of Table I.2. Results of control variables are not displayed. Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels.

Table A.3.3: Democracy, income, and aid donorship (1951-2015, robustness tests)

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline		Limited dataset		3-year averages	
	OLS FE	2SLS FE	OLS FE	2SLS FE	OLS FE	2SLS FE
Democracy	-0.0771**	-0.4580***	-0.1307*	-0.7495***	-0.2810**	-0.9165**
	(0.0350)	(0.1274)	(0.0677)	(0.2268)	(0.1145)	(0.3861)
(log) GDP per capita	0.0059	-0.0144	0.0167	-0.0178	0.0142	-0.0157
	(0.0051)	(0.0089)	(0.0122)	(0.0180)	(0.0150)	(0.0265)
Democracy # (log) GDP per capita	0.0088*	0.0580***	0.0147*	0.0945***	0.0326**	0.1087**
	(0.0045)	(0.0178)	(0.0083)	(0.0306)	(0.0147)	(0.0545)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Country FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,300	5,193	3,259	3,223	1,658	1,539
Number of countries	141	140	95	95	140	136
R-squared	0.0408		0.0597		0.0841	
Kleibergen-Paap		11.76		9.979		8.155

Note: The dependent variable is a binary variable that takes a value of one in the year a country establishes its first aid institution (narrow definition). All regressions include all control variables as in column 5–8 of Table I.2. Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels.

Table A.3.4: Democracy, income, and aid donorship (1951-2015, additional control variables)

	(1) OLS FE	(2) 2SLS FE	(3) OLS FE	(4) 2SLS FE	(5) OLS FE	(6) 2SLS FE	(7) OLS FE	(8) 2SLS FE
Democracy	-0.0752**	-0.4795***	-0.0752**	-0.4580***	-0.0750**	-0.4585***	-0.0779**	-0.4694***
	(0.0352)	(0.1398)	(0.0353)	(0.1274)	(0.0353)	(0.1280)	(0.0333)	(0.1148)
(log) GDP per capita	0.0060	-0.0156	0.0066	-0.0144	0.0066	-0.0144	0.0069	-0.0147*
	(0.0052)	(0.0097)	(0.0053)	(0.0089)	(0.0053)	(0.0089)	(0.0054)	(0.0085)
Democracy	0.0085*	0.0610***	0.0085*	0.0580***	0.0085*	0.0581***	0.0091**	0.0601***
# (log) GDP per capita	(0.0045)	(0.0198)	(0.0045)	(0.0178)	(0.0045)	(0.0179)	(0.0043)	(0.0160)
Political distance to Russia	-0.0017	0.0038						
	(0.0043)	(0.0057)						
Cold War dummy			0.1472	0.0831				
			(1.0784)	(0.4043)				
Militarized interstate dispute					0.0006	-0.0022		
					(0.0059)	(0.0067)		
(log) Colony population							0.0483**	0.0477**
							(0.0203)	(0.0192)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Duration dependence	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,300	5,193	5,193	5,193	5,193	5,193	4,941	4,941
Number of countries	141	140	140	140	140	140	134	134
R squared	0.0408		0.0415		0.0415		0.0436	
Kleibergen-Paap F-stat		11.39		11.76		11.66		12.68

Notes: The dependent variable is a binary variable that takes a value of one in the year a country establishes its first aid institution (narrow definition). All regressions include all control variables as in column 5–8 of Table I.2. Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels.

Table A.3.5: Democracy, income, and aid donorship (1951-2015, without EU accession countries)

	(1) OLS FE Narrow	(2) 2SLS FE Narrow	(3) OLS FE Narrow	(4) 2SLS FE Narrow	(5) OLS FE Narrow	(6) 2SLS FE Narrow	(7) OLS FE Broad	(8) 2SLS FE Broad
Democracy	-0.0057*	-0.0097	-0.0487	-0.3035***	-0.0543	-0.3669***	-0.0210	-0.2449**
	(0.0031)	(0.0276)	(0.0322)	(0.0988)	(0.0355)	(0.1280)	(0.0335)	(0.1073)
(log) GDP per capita	0.0116**	0.0120**	0.0094*	-0.0037	0.0062	-0.0099	0.0100	-0.0010
	(0.0047)	(0.0051)	(0.0048)	(0.0068)	(0.0052)	(0.0081)	(0.0062)	(0.0076)
Democracy # (log) GDP per capita			0.0054	0.0384***	0.0062	0.0471***	0.0016	0.0335**
			(0.0042)	(0.0143)	(0.0046)	(0.0180)	(0.0044)	(0.0151)
Government share of GDP					-0.0283	-0.0175	-0.0227	-0.0136
					(0.0190)	(0.0214)	(0.0164)	(0.0194)
Political distance to USA					-0.0114***	-0.0136***	-0.0087*	-0.0109**
					(0.0040)	(0.0044)	(0.0046)	(0.0053)
(log) Population					-0.0185	-0.0016	-0.0399***	-0.0311**
					(0.0221)	(0.0222)	(0.0147)	(0.0136)
Openness					0.0031	0.0097	0.0003	0.0037
					(0.0051)	(0.0063)	(0.0050)	(0.0062)
Intrastate conflict over territory					0.0179	0.0229	0.0056	0.0142
					(0.0162)	(0.0172)	(0.0175)	(0.0179)
Number of observations	5,103	4,991	5,103	4,991	4,920	4,824	4,435	4,339
R-squared	0.0380		0.0383		0.0390		0.0438	
Kleibergen-Paap		18.95		8.197		8.247		9.989

Notes: All regressions control for county and year fixed effects and duration dependence. The dependent variable in columns 1–6 is a binary variable that takes a value of one in the year a country establishes its first aid institution (narrow definition). The dependent variable in columns 7–8 is a binary variable that takes a value of one in the year of undertaking the very first activity of development aid (broad definition). Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels.

Table A.3.6: Democracy, income, and aid budgets (1971-2013/2015)

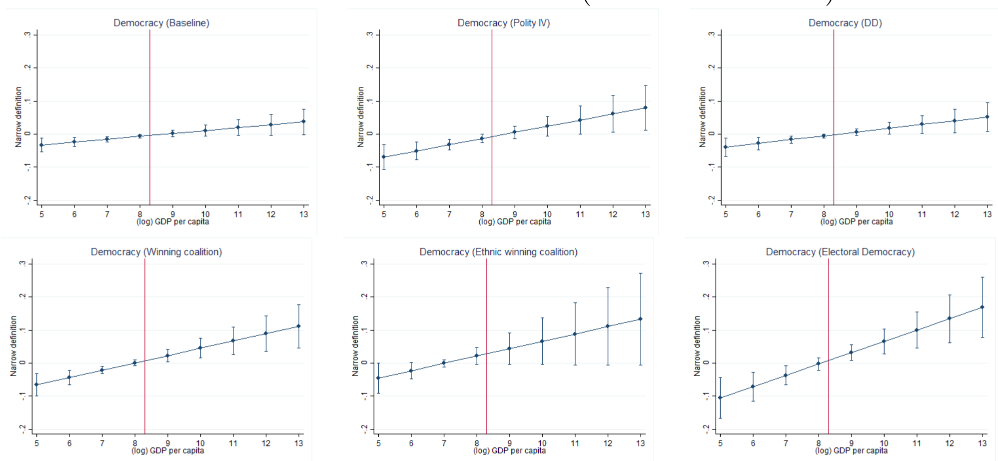
	(1) (log) Aid disbursements (OECD)	(2) (log) Aid commitments (AidData)
Democracy	-25.7117***	-28.3149***
	(9.9836)	(8.0594)
(log) GDP per capita	1.0001	0.4686
	(0.8158)	(0.8109)
Democracy	2.6357**	2.9780***
# (log) GDP per capita	(1.0505)	(0.8577)
Country and year FE	Yes	Yes
Control variables	Yes	Yes
Duration dependence	Yes	Yes
Sample period	1971–2015	1971–2013
Number of observations	1,344	977
Number of countries	46	44
R-squared	0.6002	0.4536

Notes: The dependent variable is one of two alternative measures of donor aid budgets. Column 1 uses annual aid disbursements reported in the OECD-DAC database, and column 2 uses AidData's Core Research Release database on annual aid commitments. Regressions include all control variables as in columns 5–8 of Table I.2. Results of control variables are not displayed. Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels. The sample in column 1 contains the following countries: Australia, Austria, Azerbaijan, Belgium, Bulgaria, Canada, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Kazakhstan, Kuwait, Latvia, Lithuania, Luxembourg, Malta, Netherlands, New Zealand, Norway, Poland, Republic of Korea, Romania, Russian Federation, Saudi Arabia, Slovakia, Slovenia, Spain, Sweden, Switzerland, Thailand, Turkey, United Arab Emirates, United Kingdom, and United States. The sample in column 2 contains the following countries: Australia, Austria, Belgium, Brazil, Canada, Chile, Colombia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, India, Ireland, Italy, Japan, Kuwait, Latvia, Lithuania, Luxembourg, Netherlands, New Zealand, Norway, Poland, Qatar, Republic of Korea, Romania, Saudi Arabia, Slovakia, Slovenia, South Africa, Spain, Sweden, Switzerland, Thailand, United Arab Emirates, United Kingdom, and United States.

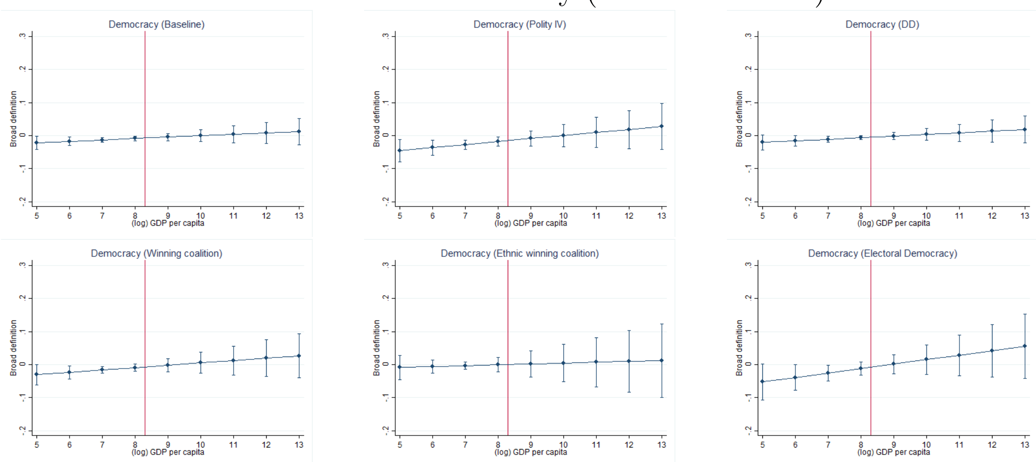
A.4 Additional results

Figure A.4.1: Marginal effect of alternative democracy measures on aid donorship across income levels

Panel A: First aid institution (narrow definition)

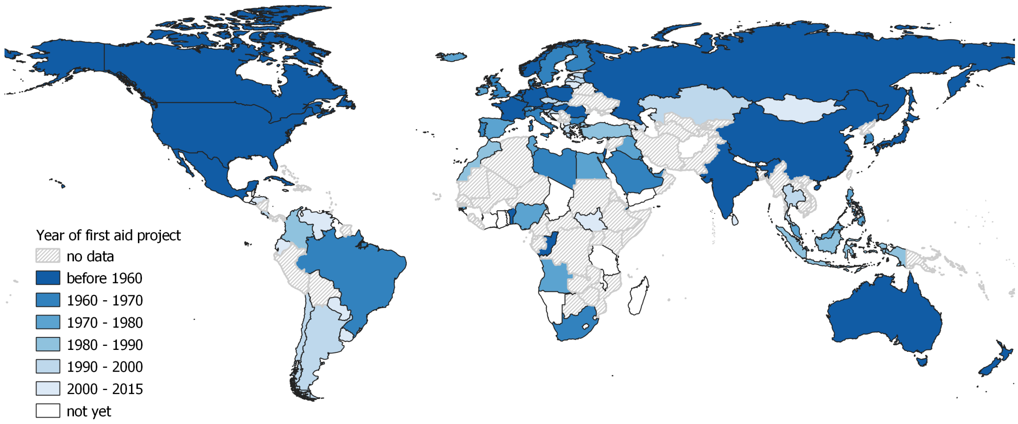


Panel B: First aid delivery (broad definition)



Notes: The figure displays the marginal effects of democracy on a country's likelihood to become an aid donor (upper panel using the narrow definition; lower panel using the broad definition) at different levels of per-capita income based on fixed-effects regressions. Each subfigure uses one of six measures of democracy, as indicated in its header. The figure also displays 90% confidence intervals. The vertical lines indicate the sample median of per-capita income.

Figure A.4.2: Year of first outgoing aid project by country (1950–2015)



Note: Authors' dataset (see text and appendix for details). Country boundaries were constructed using the Cshapes dataset (Weidmann, Kuse and Gleditsch 2010, Weidmann and Gleditsch 2016).

Table A.4.1: Democracy, income, and aid donorship (1951-2015, controlled for spatial lags, broad definition)

	(1) OLS FE	(2) 2SLS	(3) OLS FE	(4) 2SLS	(5) OLS FE	(6) 2SLS	(7) OLS FE	(8) 2SLS
Democracy	-0.0460	-0.3146***	-0.0477	-0.2844***	-0.0419	-0.3123***	-0.0346	-0.3085***
	(0.0332)	(0.1070)	(0.0340)	(0.1037)	(0.0333)	(0.1038)	(0.0350)	(0.1043)
(log) GDP per capita	0.0103	-0.0043	0.0116*	0.0008	0.0103	-0.0047	0.0064	-0.0086
	(0.0065)	(0.0081)	(0.0066)	(0.0074)	(0.0064)	(0.0080)	(0.0059)	(0.0079)
Democracy	0.0047	0.0412***	0.0048	0.0345**	0.0041	0.0408***	0.0033	0.0406***
# (log) GDP per capita	(0.0044)	(0.0145)	(0.0045)	(0.0135)	(0.0044)	(0.0143)	(0.0046)	(0.0143)
Donor spatial lag	0.2573***	0.2845***						
(by geographic distance)	(0.0832)	(0.0860)						
Donor spatial lag			0.0113	0.0041				
(by democracy peer group)			(0.0107)	(0.0107)				
GDP spatial lag					-0.0038	0.0001		
(by geographic distance)					(0.0182)	(0.0190)		
GDP spatial lag							0.0212*	0.0201**
(by democracy peer group)							(0.0111)	(0.0098)
Number of observations	4,660	4,658	4,545	4,543	4,660	4,658	4,596	4,594
Number of countries	128	126	124	122	128	126	126	124
R squared	0.0440		0.0414		0.0413		0.0421	
Kleibergen-Paap F-stat		13.55		10.83		12.77		15.94

Notes: Control variables, country and year fixed effects and duration dependence in all regressions. The dependent variable is a binary variable that takes a value of one in the year a country provides its first out-going aid project (broad definition). All regressions include all control variables as in columns 5–8 of Table I.2. Standard errors are clustered at the country level and reported in parentheses. *** 1%, ** 5%, and * 10 % significance levels.

B. Appendix of Move on up

B.1 Additional tables

Table B.1.1: List of variables

Variable	**Definition**	**Source**
	Dependent variables	
Electrified	1 if household has access to grid electricity	Nigeria National Bureau of Statistics (2017)
# of household members	number of all household members	Nigeria National Bureau of Statistics (2017)
# of elderly	number of all household members above age 65	Nigeria National Bureau of Statistics (2017)
# number of children	number of all household members below age 19	Nigeria National Bureau of Statistics (2017)
Migration all HH members	1 if HH members left household since last wave	Nigeria National Bureau of Statistics (2017)
Migration HH head	1 if HH head left household since last wave	Nigeria National Bureau of Statistics (2017)
Migration HH spouse	1 if spouse of HH head left household since last wave	Nigeria National Bureau of Statistics (2017)
Migration HH child	1 if child of HH head left household since last wave	Nigeria National Bureau of Statistics (2017)
Migration HH grandchild	1 if grandchild of HH head left household since last wave	Nigeria National Bureau of Statistics (2017)
(Log) production value	logarithmic value of all harvest produced by the household (self-reported)	Nigeria National Bureau of Statistics (2017)
(Log) labor costs	aggregate cost of agricultural workers hired by the household for this season (self-reported)	Nigeria National Bureau of Statistics (2017)
# of plots	number of plots owned by the household	Nigeria National Bureau of Statistics (2017)
(log) food consumption	logarithmic good consumption per HH member (in the past 7 days)	Nigeria National Bureau of Statistics (2017)
Non-farm work	1 if HH member worked self-employed or outside the home for wage (in the past 7 days)	Nigeria National Bureau of Statistics (2017)
Farm-work	1 if HH member worked on a family farm (in the past 7 days)	Nigeria National Bureau of Statistics (2017)
Working hours	total working hours in primary and secondary employment (in the past 7 days)	Nigeria National Bureau of Statistics (2017)
$log(m_{odt})$	logarithmic fraction of migrants from municipality o that moved to municipality d in wave t divided by total number of residents of o in wave t	Nigeria National Bureau of Statistics (2017)

Independent variables		
(Log) grid distance	negative logarithmic distance to the closest newly constructed transmission line	Nigeria National Bureau of Statistics (2017), Rural Electrification Agency (2020)
(Log) grid distance instrument	negative logarithmic distance to the least cost path of the closest newly constructed transmission line	Nigeria National Bureau of Statistics (2017), Rural Electrification Agency (2020)
Dummy grid	1 if household was within 15 km distance of any newly constructed transmission line	Nigeria National Bureau of Statistics (2017), Rural Electrification Agency (2020)
Dummy grid instrument	1 if household was within 15 km of the least cost path of any newly constructed transmission line	Nigeria National Bureau of Statistics (2017), Rural Electrification Agency (2020)
$Grid_{ot}$	1 if new transmission lines were constructed within the boundaries of municipality o in year t	GADM (2015), Rural Electrification Agency (2020)
Control variables: baseline model		
Substations	negative logarithmic distance to the closest substation	Nigeria National Bureau of Statistics (2017), Rural Electrification Agency (2020)
Capital	negative logarithmic distance to the state capital	GeoNames (2020)
Road distance	negative logarithmic distance to any primary or secondary 2009 road	Own construction based on OpenStreetMap (2020)
% Cropland	percentage of area covered in cropland within a 40 km buffer	European Space Agency (2019)
Population density	Population density within a 40 km buffer	WorldPop & Center for International Earth Science Information Network (CIESIN) (2018)
% Urban	percentage of area covered in urban land within a 40 km buffer	European Space Agency (2019)
3G mobile network	dummy variable indicating that the location is within reach of the 3G mobile network	Collins Bartholomew (2021)
Control variables: gravity model		
$log(dist_{od})$	logarithmic distance of municipality centroids	ADM2 boundaries from GADM (2015)
$\%Cropland_{dt}$	percentage of area covered in cropland within ADM2 boundaries	European Space Agency (2019), GADM (2015)
$\%Urban_{dt}$	percentage of area covered in urban land within ADM2 boundaries	European Space Agency (2019), GADM (2015)
$Grid_{dt}$	1 if new transmission lines were constructed within the boundaries of municipality d in year t	GADM (2015), Rural Electrification Agency (2020)

Table B.1.2: Effect on main lighting fuel

	(1)	(2)	(3)	(4)	(5)
	Baseline mean	-(Log) grid distance		Dummy grid	
		OLS	2SLS	OLS	2SLS
Collected firewood	0.088	0.002	0.005	-0.006	0.029
		(0.016)	(0.019)	(0.027)	(0.026)
Purchased firewood	0.038	0.006	0.015	0.008	0.007
		(0.009)	(0.011)	(0.015)	(0.013)
Kerosene	0.463	-0.051	-0.072*	-0.134	-0.310**
		(0.032)	(0.039)	(0.103)	(0.112)
Electricity	0.156	0.008	0.031	0.065	0.255***
		(0.028)	(0.028)	(0.078)	(0.083)
Generator	0.021	0.026	0.035	0.069	0.151
		(0.019)	(0.029)	(0.057)	(0.112)
Battery	0.202	-0.010	-0.029	-0.051	-0.171**
		(0.024)	(0.029)	(0.044)	(0.064)
Other	0.031	0.019**	0.014	0.049	0.039
		(0.009)	(0.012)	(0.031)	(0.035)
Observations		2,308	2,308	2,308	2,308
F-stat			331.727		55.568

Notes: All regressions control for wave-state fixed effects. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.1.3: The effect of new transmission lines on migration reasons

	(1) Mean	(2) All	(3) Male adults	(4) Female adults	(5) Children
Divorce/separation	0.0460	-0.0636**	-0.0016	-0.1321**	0.0188
		(0.0294)	(0.0053)	(0.0562)	(0.0168)
Studies/education	0.1057	-0.0194	-0.0991	0.0491	0.0655
		(0.0733)	(0.0979)	(0.0793)	(0.2134)
For work	0.1802	0.1088	0.3005**	-0.0482	0.1181**
		(0.1111)	(0.1203)	(0.1335)	(0.0529)
To find better land	0.0487	0.0152	-0.0286	0.0480	-0.0514
		(0.0517)	(0.0878)	(0.0466)	(0.0530)
Health reasons	0.0060	-0.0209	-0.0080	-0.0256	-0.0475
		(0.0139)	(0.0098)	(0.0215)	(0.0329)
Security reasons	0.0066	-0.0213	0.0001	-0.0133	-0.1068
		(0.0249)	(0.0037)	(0.0169)	(0.0908)
Marriage/cohabitation	0.2618	-0.0074	-0.1107	0.1172	-0.1119
		(0.0600)	(0.0948)	(0.1160)	(0.1035)
To join family	0.1840	-0.0083	-0.0962	-0.0388	0.3322**
		(0.0804)	(0.0758)	(0.0990)	(0.1546)
Moved with family	0.0268	-0.0058	0.0157	-0.0232	-0.0195
		(0.0302)	(0.0509)	(0.0300)	(0.0367)
To set up home	0.0690	-0.0103	-0.0040	-0.0030	0.0103
		(0.0428)	(0.0700)	(0.0363)	(0.0095)
Unable to stay due to conflict	0.0044	0.0044	0.0136	0.0014	0.0014
		(0.0043)	(0.0112)	(0.0017)	(0.0017)
Dispute with other HH member	0.0027	0.0015	0.0059	-0.0035	-0.0035
		(0.0034)	(0.0049)	(0.0046)	(0.0046)
Other	0.0581	0.0273	0.0124	0.0720	-0.2079
		(0.0706)	(0.1099)	(0.0619)	(0.1555)
Missing values	0.0083	0.0049	-0.0027	0.0188	-0.0009
		(0.0046)	(0.0059)	(0.0164)	(0.0012)
Observations		26,486	6,367	7,595	12,524
F-stat		113.1559	64.2769	59.6364	265.5221

Note: All regressions control for wave-state fixed effects and geographic controls. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.1.4: The effect of new transmission lines on main employment sector

	(1) Baseline	(2) All	(3) HH head	(4) HH spouse	(5) HH child	(6) HH grandchild
Agriculture	0.2511	-0.1043	-0.0911	-0.1130	-0.0762	-0.1102
		(0.0664)	(0.1313)	(0.1175)	(0.0538)	(0.0817)
Mining	0.0001	0.0005	0.0028	0.0000	0.0001	
		(0.0006)	(0.0036)	(0.0001)	(0.0001)	
Manufacturing	0.0221	0.0018	-0.0069	0.0362	0.0024	-0.0022
		(0.0089)	(0.0179)	(0.0348)	(0.0080)	(0.0045)
Technical Activities	0.0024	0.0015	0.0049	0.0040	-0.0009	0.0010
		(0.0019)	(0.0071)	(0.0037)	(0.0014)	(0.0018)
Electricity/Water/Gas/Waste	0.0007	0.0012	0.0024		0.0006	
		(0.0012)	(0.0029)		(0.0008)	
Construction	0.0066	-0.0058	-0.0287		0.0032	
		(0.0047)	(0.0287)		(0.0023)	
Transportation	0.0082	0.0090*	0.0281*	-0.0004	0.0056	
		(0.0045)	(0.0155)	(0.0007)	(0.0051)	
Buying and Selling	0.0595	0.0361*	0.1108*	0.2080***	-0.0148	-0.0090
		(0.0192)	(0.0585)	(0.0650)	(0.0106)	(0.0106)
Finance/Insurance/Real estate	0.0002	-0.0034	-0.0213	0.0026	-0.0001	
		(0.0029)	(0.0160)	(0.0033)	(0.0002)	
Personal Service	0.0224	0.0016	0.0568**	0.0009	-0.0100	-0.0927*
		(0.0078)	(0.0272)	(0.0095)	(0.0117)	(0.0537)
Education	0.0069	-0.0024	-0.0048	-0.0150	0.0008	
		(0.0045)	(0.0154)	(0.0294)	(0.0013)	
Health	0.0035	0.0023	0.0052	0.0068	0.0008	
		(0.0035)	(0.0101)	(0.0112)	(0.0009)	
Public Administration	0.0065	0.0025	0.0271	0.0009	-0.0028	-0.0020
		(0.0045)	(0.0180)	(0.0043)	(0.0040)	(0.0021)
Other	0.0035	0.0036	-0.0059	0.0056	0.0055	
		(0.0055)	(0.0195)	(0.0086)	(0.0044)	
Observations		15,993	2,716	2,536	9,338	564
F-stat		100.7010	68.2600	96.4335	102.0181	164.6119

Notes: All regressions control for wave-state fixed effects and the full set of geographic controls. Column (3) uses the sample of the household heads, column (4) uses the sample of the spouses of the household heads, column (5) uses the sample of the children of the household head, and column (6) uses the sample of grandchildren of the household head. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.1.5: Balancing between control and treatment municipalities

	Control	SD	treatment	SE	N
Primary road density	0.273	0.558	-0.001	0.162	316
(Log) population	11.985	0.453	0.162	0.132	322
% of cropland	0.364	0.306	0.035	0.086	322
% of urban land	0.049	0.170	0.045	0.049	322

Note: Balancing is tested using a regression with state fixed effects and standard errors clustered at village-level as in the main regressions. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$)

B.2 Robustness tables

Table B.2.1: Effect of new transmission lines on migration controlled for baseline differences

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
# of household members	5.963	-0.308**	-0.327**	-0.711***	-0.799***	2,310	58.444
		(0.147)	(0.159)	(0.201)	(0.230)		
# of elderly	0.071	-0.048	-0.048	-0.030	-0.036	2,310	58.444
		(0.031)	(0.032)	(0.033)	(0.036)		
# of children (total)	3.259	-0.279**	-0.310***	-0.512***	-0.586***	2,310	58.444
		(0.105)	(0.103)	(0.167)	(0.159)		
# of children (age 0-5)	1.176	-0.201**	-0.218**	-0.160	-0.247**	2,247	58.169
		(0.093)	(0.096)	(0.098)	(0.095)		
# of children (age 6-12)	1.301	0.087	0.076	-0.002	0.016	2,247	58.169
		(0.074)	(0.073)	(0.107)	(0.106)		
# of children (age 13-18)	0.802	-0.141	-0.144	-0.362**	-0.358**	2,247	58.169
		(0.089)	(0.089)	(0.137)	(0.140)		

Notes: All regressions control for wave-state fixed effects and the full set of geographic controls. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.2: The effect of new transmission lines on migration (individual level) controlled for baseline covariates

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
All HH members	0.019	0.015	0.015	0.060**	0.062**	15,729	100.476
		(0.017)	(0.018)	(0.027)	(0.029)		
HH head	0.003	0.006**	0.007**	0.010*	0.012**	2,682	69.652
		(0.003)	(0.003)	(0.005)	(0.005)		
HH spouse	0.035	-0.020	-0.019	-0.049*	-0.052**	2,495	97.169
		(0.018)	(0.017)	(0.025)	(0.021)		
HH child	0.091	0.031	0.031	0.109**	0.112**	9,180	101.136
		(0.024)	(0.026)	(0.043)	(0.044)		
HH grandchild	0.159	0.122	0.230**	0.060	0.286**	551	177.046
		(0.096)	(0.096)	(0.089)	(0.128)		
Other	0.180	0.060	0.047	0.088	0.128	810	38.440
		(0.080)	(0.082)	(0.239)	(0.241)		

Notes: All regressions control for wave-state fixed effects and the full set of geographic controls. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.3: Effect of new transmission lines on agricultural production controlled for baseline differences

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
(Log) production value	10.121	0.194	0.095	0.555	0.558	1,868	39.552
		(0.551)	(0.536)	(0.939)	(0.940)		
(Log) labor costs	1.436	0.161	0.146	1.117***	1.027**	1,877	43.519
		(0.448)	(0.448)	(0.411)	(0.385)		
(Log) # of paid workers	0.600	-0.095	-0.093	0.050	-0.015	1,877	43.519
		(0.098)	(0.094)	(0.175)	(0.175)		
# of plots	1.784	0.173	0.219	0.685*	0.778**	2,310	58.444
		(0.261)	(0.263)	(0.358)	(0.373)		
(Log) food consumption	4.011	0.086**	0.086**	0.269***	0.282***	2,239	58.431
		(0.042)	(0.041)	(0.074)	(0.085)		

Notes: All regressions control for wave-state fixed effects and the full set of geographic controls. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.4: Fixed effects regression on household composition

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
# of household members	5.963	-0.276*	-0.313*	-0.756***	-0.963***	3,524	22.125
		(0.158)	(0.181)	(0.254)	(0.294)		
# of elderly	0.071	-0.029	-0.037	-0.069*	-0.096**	3,524	22.125
		(0.032)	(0.034)	(0.037)	(0.043)		
# of children (total)	3.259	-0.207*	-0.247*	-0.411***	-0.555***	3,524	22.125
		(0.107)	(0.127)	(0.158)	(0.190)		
# of children (age 0-5)	1.176	-0.207**	-0.240**	-0.175**	-0.281***	3,459	21.732
		(0.096)	(0.103)	(0.077)	(0.097)		
# of children (age 6-12)	1.301	0.070	0.038	0.002	-0.050	3,459	21.732
		(0.065)	(0.069)	(0.063)	(0.085)		
# of children (age 13-18)	0.802	-0.086	-0.068	-0.243*	-0.232	3,459	21.732
		(0.084)	(0.097)	(0.127)	(0.163)		

Notes: All regressions control for household fixed effects. In addition, all regressions control for wave-state fixed effects and the interaction of wave and distance to the closest substation in order to provide a similar specification to the first-difference estimates of Table II.3. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.5: Fixed effects regression on migration (individual level)

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
All HH members	0.019	0.014	0.013	0.058*	0.059*	26,486	113.030
		(0.014)	(0.015)	(0.031)	(0.032)		
HH head	0.003	0.001	0.001	0.001	0.002	4,173	62.096
		(0.001)	(0.001)	(0.001)	(0.002)		
HH spouse	0.035	-0.015	-0.016	-0.052***	-0.054***	4,104	112.771
		(0.015)	(0.015)	(0.018)	(0.018)		
HH child	0.091	0.035*	0.034	0.113**	0.114**	15,661	124.926
		(0.021)	(0.022)	(0.046)	(0.048)		
HH grandchild	0.159	-0.018	0.002	-0.036	0.004	1,083	98.915
		(0.072)	(0.076)	(0.103)	(0.121)		
Other	0.180	0.006	-0.001	0.060	0.054	1,454	57.224
		(0.053)	(0.054)	(0.179)	(0.179)		

Notes: All regressions control for individual level fixed effects. In addition, all regressions control for wave-state fixed effects and the interaction of wave and distance to the closest substation in order to provide a similar specification to the first-difference estimates of Table II.4. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.6: Fixed effects regression on agricultural production

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
(Log) production value	10.121	-0.453	-0.241	-0.901	-0.685	3,029	19.136
		(0.715)	(0.509)	(1.350)	(1.080)		
(Log) labor costs	1.436	-0.055	-0.065	0.307	0.262	3,037	20.801
		(0.296)	(0.327)	(0.340)	(0.374)		
(Log) # of paid workers	0.600	0.052	-0.036	-0.014	-0.180	3,037	20.801
		(0.084)	(0.107)	(0.120)	(0.167)		
# of plots	1.784	0.048	-0.088	0.315	0.083	3,524	22.125
		(0.240)	(0.193)	(0.330)	(0.266)		
(Log) food consumption	4.011	0.063*	0.052	0.202***	0.225**	3,451	22.421
		(0.037)	(0.041)	(0.070)	(0.090)		

Notes: All regressions control for household fixed effects. In addition, all regressions control for wave-state fixed effects and the interaction of wave and distance to the closest substation in order to provide a similar specification to the first-difference estimates of Table II.3. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.7: Placebo test of future transmission lines on migration

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS	(5) 2SLS	(6) Obs	(7) F-stat
# of household members	5.963	-0.144	-0.122	-0.206	-0.181	2,323	113.196
		(0.107)	(0.102)	(0.145)	(0.142)		
# of elderly	0.071	-0.011	-0.008	-0.016	-0.014	2,323	113.196
		(0.021)	(0.020)	(0.026)	(0.025)		
# of children (total)	3.259	-0.063	-0.039	-0.099	-0.065	2,323	113.196
		(0.095)	(0.090)	(0.114)	(0.109)		
# of children (age 0-5)	1.176	-0.116*	-0.090	-0.139	-0.113	2,259	106.210
		(0.068)	(0.062)	(0.082)	(0.076)		
# of children (age 6-12)	1.301	0.100	0.105	0.083	0.100	2,259	106.210
		(0.080)	(0.078)	(0.101)	(0.101)		
# of children (age 13-18)	0.802	-0.046	-0.054	-0.006	-0.014	2,259	106.210
		(0.077)	(0.079)	(0.094)	(0.097)		

Note: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.8: Placebo test of future transmission lines on migration (individual level)

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
All HH members	0.019	0.004	0.004	0.011	0.005	15,993	110.885
		(0.015)	(0.015)	(0.021)	(0.021)		
HH head	0.003	0.014**	0.013**	0.011*	0.010*	2,716	192.178
		(0.006)	(0.006)	(0.006)	(0.006)		
HH spouse	0.035	0.000	0.001	-0.007	-0.016	2,536	72.799
		(0.014)	(0.014)	(0.021)	(0.019)		
HH child	0.091	-0.002	-0.002	0.017	0.009	9,338	100.710
		(0.022)	(0.021)	(0.030)	(0.030)		
HH grandchild	0.159	-0.037	-0.001	-0.011	0.014	564	81.082
		(0.086)	(0.084)	(0.118)	(0.108)		
Other	0.180	0.128*	0.140*	0.125	0.126	828	52.410
		(0.074)	(0.076)	(0.109)	(0.104)		

Note: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.9: Placebo test of future transmission lines on agricultural production

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
(Log) production value	10.121	-0.534*	-0.532	-0.360	-0.376	1,876	72.340
		(0.322)	(0.327)	(0.438)	(0.442)		
(Log) labor costs	1.436	0.160	0.062	0.216	0.067	1,885	74.849
		(0.374)	(0.386)	(0.455)	(0.489)		
(Log) # of paid workers	0.600	0.026	-0.003	-0.006	-0.032	1,885	74.849
		(0.096)	(0.102)	(0.138)	(0.140)		
# of plots	1.784	-0.191	-0.186	-0.012	0.001	2,323	113.196
		(0.196)	(0.201)	(0.156)	(0.166)		
(Log) food consumption	4.011	0.067***	0.063**	0.112***	0.106***	2,250	117.100
		(0.026)	(0.025)	(0.036)	(0.036)		

Notes: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.10: The effect of new transmission lines on migration controlling for new roads

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls
Panel A: New Grid Construction					
# of household members	5.963	-0.330**	-0.349**	-0.693***	-0.778***
		(0.140)	(0.150)	(0.197)	(0.227)
# of elderly	0.071	-0.061*	-0.061*	-0.038	-0.044
		(0.033)	(0.035)	(0.032)	(0.037)
# of children (total)	3.259	-0.300***	-0.325***	-0.506***	-0.582***
		(0.102)	(0.100)	(0.148)	(0.141)
# of children (age 0-5)	1.176	-0.206**	-0.223**	-0.174*	-0.255**
		(0.093)	(0.097)	(0.096)	(0.102)
# of children (age 6-12)	1.301	0.063	0.056	-0.020	-0.006
		(0.071)	(0.071)	(0.102)	(0.102)
# of children (age 13-18)	0.802	-0.136	-0.136	-0.335	-0.337
		(0.089)	(0.090)	(0.133)	(0.135)
Panel B: New Road Construction					
# of household members	5.963	0.148	0.146	0.144	0.127
		(0.163)	(0.173)	(0.162)	(0.171)
# of elderly	0.071	0.027	0.029	0.027	0.030
		(0.031)	(0.032)	(0.031)	(0.031)
# of children (total)	3.259	0.023	0.015	0.021	0.003
		(0.162)	(0.168)	(0.160)	(0.165)
# of children (age 0-5)	1.176	0.030	0.044	0.030	0.043
		(0.129)	(0.130)	(0.127)	(0.129)
# of children (age 6-12)	1.301	-0.083	-0.102	-0.084	-0.105
		(0.090)	(0.090)	(0.089)	(0.089)
# of children (age 13-18)	0.802	0.077	0.075	0.075	0.066
		(0.181)	(0.181)	(0.178)	(0.179)
Observations		2,259	2,259	2,259	2,259
F-stat				66.461	55.961

Notes: Panel A and Panel B show results from the same regressions, whereas Panel A reports point estimates for the dummy indicating new transmission grid construction, Panel B reports point estimates for the dummy indicating new road construction. All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.11: The effect of new transmission lines on migration (individual level)

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
			Panel A: New Grid Construction				
All HH members	0.019	0.015	0.014	0.053*	0.056*	15,993	100.312
		(0.016)	(0.017)	(0.027)	(0.028)		
HH head	0.003	0.005	0.005	0.011	0.011	2,716	68.572
		(0.003)	(0.003)	(0.007)	(0.007)		
HH spouse	0.035	-0.019	-0.019	-0.057**	-0.059***	2,536	94.928
		(0.017)	(0.016)	(0.024)	(0.021)		
HH child	0.091	0.029	0.029	0.098**	0.102**	9,338	101.025
		(0.023)	(0.024)	(0.042)	(0.043)		
HH grandchild	0.159	0.100	0.184**	0.020	0.198*	564	168.629
		(0.089)	(0.085)	(0.074)	(0.106)		
Other	0.180	0.058	0.045	0.130	0.164	828	44.469
		(0.082)	(0.083)	(0.234)	(0.239)		
			Panel B: New Road Construction				
All HH members	0.019	-0.045**	-0.044**	-0.043**	-0.041**	15,993	100.312
		(0.020)	(0.020)	(0.019)	(0.019)		
HH head	0.003	-0.024	-0.024	-0.024	-0.024	2,716	68.572
		(0.020)	(0.020)	(0.019)	(0.019)		
HH spouse	0.035	-0.035	-0.017	-0.038	-0.019	2,536	94.928
		(0.032)	(0.014)	(0.032)	(0.014)		
HH child	0.091	-0.044*	-0.044*	-0.040	-0.039	9,338	101.025
		(0.025)	(0.027)	(0.025)	(0.026)		
HH grandchild	0.159	-0.194	-0.150	-0.196	-0.150	564	168.629
		(0.120)	(0.125)	(0.118)	(0.120)		
Other	0.180	0.062	0.063	0.063	0.065	828	44.469
		(0.243)	(0.255)	(0.241)	(0.254)		

Note: Panel A and Panel B show results from the same regressions, whereas Panel A reports point estimates for the dummy indicating new transmission grid construction, Panel B reports point estimates for the dummy indicating new road construction. All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.12: The effect of new transmission lines on agricultural production

	(1) Baseline mean	(2) Dummy grid no controls	(3) Dummy grid controls	(4) Dummy least cost grid no controls	(5) Dummy least cost grid controls	(6) obs	(7) F-stat
Panel A: New Grid Construction							
(Log) production value	10.121	0.206	0.108	0.544	0.562	1,876	37.105
		(0.519)	(0.512)	(0.911)	(0.916)		
(Log) labor costs	1.436	0.047	0.003	0.935**	0.866**	1,885	41.024
		(0.458)	(0.465)	(0.385)	(0.358)		
(Log) # of paid workers	0.600	-0.092	-0.093	0.065	-0.003	1,885	41.024
		(0.094)	(0.087)	(0.149)	(0.151)		
# of plots	1.784	0.165	0.208	0.682*	0.769**	2,323	56.279
		(0.258)	(0.263)	(0.339)	(0.356)		
(Log) food consumption	4.011	0.081**	0.080**	0.258***	0.273***	2,250	55.914
		(0.039)	(0.038)	(0.074)	(0.085)		
Panel B: New Road Construction							
(Log) production value	10.121	-0.773**	-0.971**	-0.776*	-0.966**	1,876	37.105
		(0.384)	(0.427)	(0.382)	(0.424)		
(Log) labor costs	1.436	0.281	0.232	0.274	0.241	1,885	41.024
		(0.455)	(0.490)	(0.453)	(0.488)		
(Log) # of paid workers	0.600	0.048	0.084	0.047	0.085	1,885	41.024
		(0.305)	(0.317)	(0.302)	(0.314)		
# of plots	1.784	-0.260	-0.223	-0.254	-0.198	2,323	56.279
		(0.220)	(0.224)	(0.218)	(0.222)		
(Log) food consumption	4.011	-0.114*	-0.117*	-0.112*	-0.108*	2,250	55.914
		(0.064)	(0.063)	(0.064)	(0.063)		

Notes: All regressions control for wave-state fixed effects and distance to the closest substation. The table replicates Table II.5 while controlling for primary and secondary road construction. Panel A reports results on the grid dummy for comparison with table 6, Panel B reports results on the road dummy from the same regression as Panel A. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.13: The effect of new transmission lines on media device ownership

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) IV no controls	(5) IV controls	(6) Obs	(7) F-stat
Radio	0.553	-0.101	-0.092	-0.166	-0.161	2,300	55.424
		(0.067)	(0.065)	(0.114)	(0.105)		
TV set	0.205	0.031	0.038	0.191**	0.210**	2,300	55.424
		(0.051)	(0.053)	(0.077)	(0.089)		
Computer	0.009	0.010	0.010	-0.002	-0.002	2,300	55.424
		(0.010)	(0.010)	(0.016)	(0.017)		
Internet	0.000	-0.028	-0.027	-0.085*	-0.084*	2,312	55.693
		(0.024)	(0.024)	(0.044)	(0.045)		
Mobil	0.275	-0.021	-0.019	0.042	0.037	12,019	115.238
		(0.024)	(0.023)	(0.031)	(0.031)		

Notes: All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.14: The effect of new transmission lines on migration controlling for 3G mobile network

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls
Panel A: New Grid Construction					
# of household members	5.963	-0.324**	-0.347**	-0.666***	-0.755***
		(0.143)	(0.152)	(0.216)	(0.243)
# of elderly	0.071	-0.061*	-0.062*	-0.042	-0.047
		(0.033)	(0.035)	(0.032)	(0.037)
# of children (total)	3.259	-0.292***	-0.321***	-0.469***	-0.550***
		(0.101)	(0.100)	(0.157)	(0.149)
# of children (age 0-5)	1.176	-0.212**	-0.226**	-0.193**	-0.272**
		(0.092)	(0.096)	(0.090)	(0.099)
# of children (age 6-12)	1.301	0.074	0.063	0.019	0.027
		(0.070)	(0.071)	(0.101)	(0.105)
# of children (age 13-18)	0.802	-0.132	-0.134	-0.318	-0.321
		(0.088)	(0.089)	(0.136)	(0.139)
Panel B: New 3G Mobile Network Coverage					
# of household members	5.963	-0.208	-0.206	-0.195	-0.199
		(0.176)	(0.177)	(0.182)	(0.183)
# of elderly	0.071	0.027	0.029	0.027	0.028
		(0.021)	(0.021)	(0.021)	(0.021)
# of children (total)	3.259	-0.292**	-0.293**	-0.285**	-0.289**
		(0.119)	(0.122)	(0.121)	(0.123)
# of children (age 0-5)	1.176	0.139**	0.142**	0.138**	0.143**
		(0.065)	(0.065)	(0.065)	(0.065)
# of children (age 6-12)	1.301	-0.282***	-0.280***	-0.279**	-0.279***
		(0.101)	(0.100)	(0.102)	(0.100)
# of children (age 13-18)	0.802	-0.130	-0.136	-0.121	-0.131
		(0.112)	(0.113)	(0.111)	(0.111)
Observations		2,259	2,259	2,259	2,259
F-stat				62.647	53.359

Notes: Panel A and Panel B show the results from the same regressions, whereas Panel A reports point estimates for the dummy indicating new transmission grid construction, Panel B reports point estimates for the dummy indicating 3G mobile network coverage. All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.15: The effect of new transmission lines on migration (individual level) controlling for 3G mobile network

	(1) Baseline mean	(2) OLS no controls	(3) OLS controls	(4) 2SLS no controls	(5) 2SLS controls	(6) Obs	(7) F-stat
Panel A: New Grid Construction							
All HH members	0.019	0.016	0.016	0.058**	0.058**	15,993	96.735
		(0.017)	(0.017)	(0.026)	(0.028)		
HH head	0.003	0.005	0.005	0.012*	0.012*	2,716	66.511
		(0.003)	(0.003)	(0.007)	(0.007)		
HH spouse	0.035	-0.014	-0.016	-0.044**	-0.048**	2,536	90.614
		(0.016)	(0.016)	(0.020)	(0.019)		
HH child	0.091	0.030	0.029	0.100**	0.101**	9,338	96.849
		(0.023)	(0.025)	(0.043)	(0.044)		
HH grandchild	0.159	0.118	0.191**	0.049	0.210**	564	168.798
		(0.088)	(0.084)	(0.075)	(0.103)		
Other	0.180	0.057	0.041	0.142	0.175	828	43.174
		(0.083)	(0.084)	(0.232)	(0.236)		
Panel B: New 3G Mobile Network Coverage							
All HH members	0.019	-0.013	-0.006	-0.016	-0.008	15,993	96.735
		(0.019)	(0.019)	(0.020)	(0.020)		
HH head	0.003	-0.001	-0.000	-0.001	-0.000	2,716	66.511
		(0.010)	(0.011)	(0.010)	(0.011)		
HH spouse	0.035	-0.052***	-0.047**	-0.050**	-0.045**	2,536	90.614
		(0.019)	(0.019)	(0.018)	(0.019)		
HH child	0.091	0.004	0.012	-0.002	0.007	9,338	96.849
		(0.030)	(0.031)	(0.031)	(0.033)		
HH grandchild	0.159	-0.119	-0.058	-0.114	-0.059	564	168.798
		(0.090)	(0.095)	(0.087)	(0.091)		
Other	0.180	-0.072	-0.080	-0.071	-0.076	828	43.174
		(0.082)	(0.081)	(0.081)	(0.081)		

Note: Panel A and Panel B show the results from the same regressions, whereas Panel A reports point estimates for the dummy indicating new transmission grid construction, Panel B reports point estimates for the dummy indicating 3G mobile network coverage. All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

Table B.2.16: The effect of new transmission lines on agricultural production controlling for 3G mobile network

	(1) Baseline mean	(2) Dummy grid no controls	(3) Dummy grid controls	(4) Dummy least cost grid no controls	(5) Dummy least cost grid controls	(6) Obs	(7) F-stat
Panel A: New Grid Construction							
(Log) production value	10.121	0.185	0.098	0.508	0.522	1,876	36.837
		(0.520)	(0.509)	(0.911)	(0.912)		
(Log) labor costs	1.436	0.046	0.001	0.939**	0.871**	1,885	40.508
		(0.461)	(0.468)	(0.388)	(0.362)		
(Log) # of paid workers	0.600	-0.092	-0.095	0.065	-0.002	1,885	40.508
		(0.094)	(0.087)	(0.149)	(0.151)		
# of plots	1.784	0.177	0.216	0.734**	0.811**	2,323	53.876
		(0.256)	(0.263)	(0.299)	(0.334)		
(Log) food consumption	4.011	0.078**	0.080**	0.245***	0.263***	2,250	53.314
		(0.037)	(0.037)	(0.073)	(0.085)		
Panel B: New 3G Mobile Network Coverage							
(Log) production value	10.121	-0.401	-0.322	-0.378	-0.293	1,876	36.837
		(0.411)	(0.443)	(0.408)	(0.434)		
(Log) labor costs	1.436	-0.039	-0.041	0.005	-0.001	1,885	40.508
		(0.074)	(0.093)	(0.109)	(0.119)		
(Log) # of paid workers	0.600	-0.023	-0.048	-0.015	-0.044	1,885	40.508
		(0.157)	(0.152)	(0.154)	(0.150)		
# of plots	1.784	-0.397***	-0.385***	-0.419**	-0.395**	2,323	53.876
		(0.138)	(0.135)	(0.150)	(0.142)		
(Log) food consumption	4.011	0.091*	0.090*	0.083	0.085	2,250	53.314
		(0.051)	(0.052)	(0.048)	(0.051)		

Notes: Panel A and Panel B show the results from the same regressions, whereas Panel A reports point estimates for the dummy indicating new transmission grid construction, Panel B reports point estimates for the dummy indicating 3G mobile network coverage. All regressions control for wave-state fixed effects and distance to the closest substation. Standard errors are clustered at the level of the survey enumeration area and stated in parentheses below point estimates. *** 1%, ** 5%, and * 10 % significance levels.

B.3 Least cost approach

My approach draws heavily on Faber (2014). To construct a grid that assigns construction costs to each pixel, I use data on elevation from the 90-meter Shuttle Radar Topography Mission (SRTM) Global Digital Elevation Model (Farr & Kobrick 2000) and gridded data on landcover categories from the Climate Change Initiative Land Cover Maps (CCI-LC) by the European Space Agency (European Space Agency 2019). After converting elevation data into a gridded raster of terrain slope measured in degrees, I aggregate both datasets to a resolution of 900m x 900m for computational feasibility. Then, I generate a conductance raster that assigns construction costs to each grid cell, based on the following equation:

$$c_i = 1 + slope_i + 25 \times wetland_i + 25 \times urban_i + 25 \times water_i \tag{B.2.1}$$

where c_i represents the construction costs at grid cell i, $slope_i$ is the average land gradient at i ranging from 0 to approximately 32 degrees. The terms $wetland_i$, $urban_i$ and $water_i$ are dummy variables that mark the respective land cover categories that make infrastructure construction extremely costly. Finally, I determine the least cost path for each pair of substations based on this conductance raster.

B.4 Theoretical model

In this section we describe an extension for the static migration model from Bryan & Morten (2019) that distinguishes between urban and rural locations drawing on the two-sector Rosen-Roback model (Roback 1982). While the baseline model predicts an increase in population following a productivity shock, I show how the presence of credit constraints can alter this outcome.

B.4.1 Baseline model

The economy consists of N locations. Each location is the origin o of a number of L_o workers and can belong either to the rural sector, $r_o = 1$, or the urban sector, $r_o = 0$. Overall, there are N^r rural locations and N^u urban locations, with $N^r + N^u = N$. Following Bryan & Morten (2019), skill is location specific. For every possible destination location d workers i draw a skill level s_{id} from a Fréchet distribution (and respectively s_{io} for the origin location), such that

$$F(s_1, \ldots, s_N) = exp\left(-\left\{\sum_{d=1}^{N} s_d^{-[\tilde{\theta}/(1-\rho)]}\right\}^{1-\rho}\right), \tag{B.2.2}$$

where larger values of $\tilde{\theta}$ are associated with more evenly distributed skill level across locations and larger values of ρ are associated with a higher correlation of skill across locations. For simplicity, I will use $\theta = \tilde{\theta}/(1-\rho)$ for the remaining of the text. Following again the original model by Bryan & Morten (2019), innate skills are multiplied with the schooling quality at origin to form human capital.

$$h_{ido} = s_{id} q_o. \tag{B.2.3}$$

Wage of worker i from origin o working and living at destination d is then determined by:

$$wage_{ido} = w_d h_{ido} = w_d s_{id} q_o, \tag{B.2.4}$$

where w_d can be thought of as the wage per effective unit of labor in destination d or the productivity of location d. Bryan & Morten (2019) also include an error term in the wage equation to account for any factor that causes workers from origin o to increase their labor demand at a certain destination d. Since this is exactly the type of variation in labor demand that I am interested in, I omit the inclusion of the error term here. The term w_d is determined by the price level at destination d, p_d, and a technology term A_d, such that:

$$w_d = p_d A_d. \tag{B.2.5}$$

The indirect utility function of a worker i staying in her origin location depends on the amenities α_o this location offers and the consumption level determined by thereceived wage, such that:

$$U_{ioo} = \alpha_o w_o h_{ido}. \tag{B.2.6}$$

Again, the original model of Bryan & Morten (2019) includes an error term that describes random variation in amenities at d that depends on origin o. As I am only interested in qualitative predictions, and for the sake of simplicity, this error term is again omitted.

Moving to another location is costly and must be compensated by a higher income. So indirect utility for a worker i from origin o living and working in destination d becomes:

$$U_{ido} = \alpha_d w_d h_{ido}(1 - \tau_{do}), \tag{B.2.7}$$

where $\tau \ni [0, 1]$ is defined as movement costs. The proportion of persons i from origin o that decide to migrate to destination d is given by:

$$m_{od} = \frac{\tilde{w}_{do}}{\tilde{w}_{do} + \tilde{w}_o}, \tag{B.2.8}$$

with $\tilde{w}_{do} = \alpha_d w_d h_{ido}(1 - \tau_{do})$ and $\tilde{w}_o = \alpha_o w_o$. Here $\tilde{w}_{do}$ measures the attractiveness of location d for someone from o. This is the main sorting equation. In contrast to Bryan & Morten (2019) my sorting equation includes human capital. In my empirical analysis of section III.6 we only observe total out-migration from origin o. This can be written as:

$$M_o = \sum_{d=1}^{N} m_{od} = \sum_{d=1}^{N} \frac{\tilde{w}_{do}}{\tilde{w}_{do} + \tilde{w}_o}. \tag{B.2.9}$$

B.4.2 Productivity shock

Denote e as a measure of local electricity access. We assume technology of production A_o is a positive function of local electricity access, such that $A_o(e)' = t$, with $t \ni [0, 1]$. This implies that an increase in e increase local productivity:

$$\frac{\partial \tilde{w}_{oo}}{\partial e} = \alpha_o p_o t h_{ioo} \geq 0. \tag{B.2.10}$$

When productivity rises, wages at origin o increase and in turn the attractiveness of destination o for workers from all locations increases. As a net effect, we expect to see falling migration flows from origin o to all other destinations d:

$$\frac{\partial M_o}{\partial e} = -\alpha_o p_o t h_{ioo} \sum_{d=1}^{N} \frac{\tilde{w}_{do}}{(\tilde{w}_{do} + \tilde{w}_{oo})^2} \leq 0. \tag{B.2.11}$$

This simple prediction is in line with previous work. Lewis & Severnini (2020) use a Rosen-Roback style model to predict that productivity boosts from rural electrification lead to an increase in population locally. Bryan & Morten (2019) assume an exogenous technology term, but include a normally distributed error term to allow for any unmeasured characteristics that increase productivity which in turn raises migration to this location.

B.4.3 Credit constraints

Credit constraints are a common market failure in developing countries. In the context of migration, scholars have shown that a lack of credit is an important barrier to optimal migration, since it prevents households from being able to pay for movement costs upfront even if the expected return from migration is positive (Bryan et al. 2014). We remain in a static setting and model credit constraints such that movement costs cannot exceed wage at origin, i.e., $\tau_{do} \leq wage_{ioo}$. One can think of the restriction as a simplification of a dynamic setting, where movement costs have to be paid by earnings and saving of the previous period. Our dyadic mobility measure then becomes:

$$m_{od} = \begin{cases} \frac{\tilde{w}_{do}}{\tilde{w}_{do}+\tilde{w}_{oo}}, & \text{if } \tau_{do} \leq wage_{ioo} \\ 0, & \text{otherwise} . \end{cases} \tag{B.2.12}$$

This restriction reduces the aggregate measure of out-migration from origin o to:

$$M_o = \sum_{d=1}^{N} \frac{\tilde{w}_{do}}{\tilde{w}_{do} + \tilde{w}_{oo}} \times 1_{\tau_{do} \leq wage_{ioo}}, \tag{B.2.13}$$

where $1_{\tau_{do} \leq wage_{ioo}}$ is an indicator function turning 1 if $\tau_{do} \leq wage_{ioo}$. This means when credit constraints are present the migration flows from origin o are smaller than optimal. Now, if origin o receives an increase in electricity access, productivity and wages at o increase. This reduces the set of origin-destination pairs for which $\tau_{do} > wage_{ioo}$. At the same time, rising wages increase $\tilde{w}_{oo}$, i.e., the attractiveness of location o. Both effects have opposing implications for the aggregate out-migration M_o. The net effect depends on the number of origin-destination pairs for which $\tau_{do} \geq wage_{ioo}$ is satisfied before and after the productivity shock. Assume without electricity at the origin o there are m destinations where wage at origin is smaller than movement costs with electricity there are only n destinations for which this is the case, with $n < m < N$. Given these assumptions, an increase in electricity access at origin o increases out-migration if:

$$\left| -\alpha_o p_o th_{ioo} \sum_{d=1}^{N-m} \frac{\tilde{w}_{do}}{(\tilde{w}_{do} + \tilde{w}_{oo})^2} \right| < \sum_{d=N-m}^{N-m+n} \frac{\tilde{w}_{do}}{\tilde{w}_{do} + \tilde{w}_{oo}}. \tag{B.2.14}$$

Note that the productivity shock only affects the attractiveness of the origin $\tilde{w}_{oo}$ and the set of destinations that workers from o can afford to migrate to. It does not affect the relative attractiveness of potential destinations $\tilde{w}_{do}$. This means for any pair of destinations $d_1, d_2 \ni N$ if destination d_1 is preferred over destination d_2 before the technology shock at origin o, this is still the case after the technology shock:

$$U(\tilde{w}_{d_1o}|e = 0) > U(\tilde{w}_{d_2o}|e = 0) \iff U(\tilde{w}_{d_1o}|e = 1) > U(\tilde{w}_{d_2o}|e = 1). \tag{B.2.15}$$

C. Appendix of Broken Promises

C.1 Methodological details on lab-in-field experiments

Lotteries

This study uses choices over lotteries that vary in expected return and variance to extract risk preferences. In the endline, data collection respondents were asked to choose between two or three alternative lotteries. The design of this experiment involved eight rounds, building on the research design by Jakiela & Ozier (2015). After choosing one option, the chosen lottery was played as a flip of a fair coin (50 percent chance of each outcome). The game started with two practice rounds to make participants familiar with the rules. After that, the participants had to play six additional rounds. At the end of the game, one round was selected at random and the lottery chosen by the participants was played and paid out. Participants were informed about these rules at the beginning of the game. The lotteries are set up as described below in Table C.4.1.

The number of times respondents chose the riskiest lottery can be used as a proxy for their risk preferences. Given that respondents in these types of experiments often display choices that are inconsistent with constant relative risk aversion (CRRA), a non-parametric approach to measure risk aversion is more appropriate. Thus, following the approach put forward by Jakiela & Ozier (2015), the set of lottery choices can also be used to infer risk preferences in a less stringent and non-theoretic manner. One measure is created by counting how many times respondents choose the riskiest lotteries, i.e., lotteries with the largest spread, or the safest lotteries. In addition, the likelihood to choose the riskier lottery during each decision round was evaluated individually. The results are then compared to survey answers on risk preferences.

Test questions were included to detect biased answers that resulted from a lack of understanding. Due to the relatively low numeracy skills and the complexity of the lotteries, the study included 3 questions to test for monotonicity, i.e., if participants behaved like utility-maximizers (Andreoni & Sprenger 2010). If participants answered more than 1 of these test questions in a way inconsistent with utility maximization, it is likely that they simply did not understand the nature of the decision problem.

Table C.1.1: Pay-outs of lotteries, expected utility

	Lottery A		Lottery B		Lottery C	
	Heads	Tails	Heads	Tails	Heads	Tails
Practice						
Decision 1	100	100	150	150		
Decision 2	100	150	200	250		
Game						
Decision 3	100	100	100	120		
Decision 4	100	100	0	400		
Decision 5	30	340	100	100	0	400
Decision 6	100	100	55	240	30	340
Decision 7	30	230	60	170	90	110
Decision 8	10	200	70	160	90	110

Trust game

Trust attitudes towards the World Bank were assessed using a trust game. The basic structure of a trust game developed by Berg et al. (1995) involves Player A receiving an endowment of X and choosing how much of this endowment to send to Player B, $Y \ni [0, X]$. Player B receives $3Y$ – i.e., three times whatever A sent him – and must decide how much of this endowment to send back to A, $Z \ni [0, 3Y]$. A receives a payout of $X - Y + Z$ and B receives a payout of $3Y - Z$. $\frac{Y}{X}$ is used as a measure of trust. $\frac{Z}{3Y}$ is used as a measure of trustworthiness. The table below summarizes payouts for the two players:

Table C.1.2: Trust game payouts

Player A			**Player B**		
Endowment	Sends	Payout	Endowment	Sends	Payout
X	Y	$X - Y + Z$	$3Y$	Z	$3Y - Z$

In our study, participants were asked to play several rounds of a trust game. In the first game, Player B was framed as the World Bank to extract a measure of trust toward the World Bank or official institutions in general. Participants may hold the World Bank responsible for the (non-) payment of the business start-up grants. This framing of Player B as the World Bank allows for a direct measure of how willing participants are to partake in an interaction with the World Bank that could have financial consequences. Hence, it can act as a measure of how not receiving the promised grant had influenced their level of trust and their willingness to interact with the World Bank. The reciprocal behavior of Player B was modeled to mirror the probability of non-disbursement of the cash grant. In 34 percent of the cases documented by the phone survey, participants received the grant. This information was used to define the reciprocal behavior of Player B. Player B played fairly 34 percent of the time – that is, returns back exactly half of what they obtain from the study participant. Player B 66 percent of the time acted unfairly and kept all that is sent to them, regardless of what the respondent sent. In the end, the participant was paid out the budget of Player A.

To obtain a more general measure of the respondents' trust levels, and to accompany the first measure, a second game was played which pit the participants against each other. The survey respondents were equally and randomly selected as Players A and B, stratified by treatment groups and treatment strands. Regarding the implementation of the games and pairing of the players, a lab-in-the-field experimental setup was impossible to organize because respondents had to be interviewed individually. This was primarily due to the complicated logistical circumstances surrounding fieldwork in South Sudan, in no small part due to rapidly deteriorating security conditions, but also due to constraints on the respondents' time. Respondents were, therefore, playing the games against a pre-loaded hypothetical distribution of responses. Enumerators explained to the respondents that the other player would be another survey respondent elsewhere in South Sudan. The set of possible responses, in terms of the fraction of the endowment sent or returned, was equally distributed between $[0.1, 1]$ in increments of 0.1. In no cases was the fraction of endowment sent or returned equal to zero.

C.2 Index creation

Following Anderson (2008) index s_{ji} is defined as a weighted average of all standardized outcomes k within outcome group j.

$$s_{ij} = \frac{1}{W_{ij}} \sum_{k} w_{ijk} \frac{y_{ijk} - \bar{y}_{ijk}}{\sigma^{y}_{jk}}$$

Weight w_{jk} of each outcome k is derived from the inverted covariance matrix of all standardized outcomes k.

$$\sum_{j}^{-1} = \begin{bmatrix} c_{j11} & \dots & c_{j1K} \\ \vdots & \ddots & \vdots \\ c_{jK1} & \dots & c_{jKK} \end{bmatrix}$$

Weight w_{jk} then consists of the row sum of the inverted covariance matrix.

$$w_{jk} = \sum_{l=1}^{K_j} c_{jkl}$$

C.3 Balancing and validity of the research design

Table C.3.1: Balancing original control and treatment group at baseline

		(1)	(2)	(3)	(4)	(5)
		Control group		ITT group		
		Mean	SD	Coeff.	SE	N
Panel A: Individual and household characteristics						
Age		27.417	4.901	0.265	0.207	2,292
Gender		0.602	0.490	0.009	0.020	2,292
Married		0.666	0.472	-0.016	0.020	2,291
Employment status		0.612	0.487	0.012	0.020	2,292
Business ownership		0.642	0.480	0.017	0.020	2,292
Consumption food		5.330	1.212	0.070	0.051	2,292
Consumption nonfood		2.418	1.293	0.010	0.056	2,292
Formal bank account		0.373	0.484	-0.004	0.020	2,292
(Log) amount formal loans		-0.332	1.723	-0.036	0.075	2,280
(Log) amount informal loans		-1.329	3.290	0.104	0.136	2,258
Education level	No education	0.191	0.393	0.016	0.017	2,292
	Some Primary	0.315	0.465	0.015	0.020	2,292
	Some Secondary	0.404	0.491	-0.031	0.020	2,292
	Some University or Higher	0.090	0.286	0.000	0.012	2,292
Literacy	No English	0.247	0.432	0.016	0.018	2,292
	Some English	0.273	0.446	0.022	0.019	2,292
	Good English	0.480	0.500	-0.038*	0.021	2,292
Numeracy	Low	0.238	0.426	0.010	0.018	2,292
	Medium	0.160	0.367	0.037**	0.016	2,292
	High	0.602	0.490	-0.047**	0.021	2,292
Household size		7.310	3.377	-0.050	0.141	2,292
Number of children		3.107	2.282	0.134	0.096	2,292
Number of elderly		0.109	0.344	-0.021	0.014	2,292
Number of rooms		3.180	1.775	-0.093	0.071	2,292
Number of buildings		3.676	1.951	-0.138*	0.080	2,292
(Log) distance to KCB branch		2.395	1.938	0.001	0.082	2,256
Fatalities UCDP 2011-2014		0.013	0.046	0.004	0.004	2,292
Fatalities UCDP 2015-2017		-0.000	1.000	0.128*	0.077	2,292
Joint test of		F-stat		p-value		
orthogonality		1.08		0.362		
Panel B: State at baseline						
Central Equatoria		0.169	0.375	-0.002	0.016	2,292
Eastern Equatoria		0.160	0.367	-0.008	0.015	2,292
Lakes		0.158	0.365	0.001	0.015	2,292
Northern Bahr El Ghazal		0.170	0.376	0.006	0.016	2,292
Western Bahr El Ghazal		0.172	0.377	-0.000	0.016	2,292
Western Equatoria		0.172	0.377	0.003	0.016	2,292
Joint test of		F-stat		p-value		
orthogonality		0.13		0.984		

Notes: Columns (1) and (2) reports mean and standard deviation of baseline characteristics for control group participants. Columns (3) and (4) report on a t-test comparing difference in means in the control and treatment group. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.2: Balancing between "training, no grant" vs. "training and grant"

		(1)	(2)	(3)	(4)	(5)
		"training, no grant"		"training and grant"		
		Mean	SD	Coeff.	SE	N
Panel A: Individual and household characteristics						
Age		27.570	4.691	0.399	0.436	626
Gender		0.673	0.470	-0.086	0.059	626
Married		0.606	0.489	0.049	0.044	626
Employment status		0.656	0.476	-0.015	0.046	626
Business ownership		0.642	0.480	0.065*	0.039	626
Consumption food		5.390	1.150	-0.009	0.112	626
Consumption nonfood		2.398	1.322	0.283**	0.125	626
Formal bank account		0.421	0.494	0.107**	0.047	626
(Log) amount formal loans		-0.338	1.756	-0.099	0.161	625
(Log) amount informal loans		-0.972	2.892	-0.518*	0.298	614
Education level	No education	0.173	0.379	-0.099**	0.045	626
	Some Primary	0.308	0.462	-0.005	0.040	626
	Some Secondary	0.399	0.490	0.093**	0.046	626
	Some University or Higher	0.120	0.326	0.011	0.030	626
Literacy	No English	0.233	0.423	-0.124***	0.044	626
	Some English	0.269	0.444	0.083*	0.043	626
	Good English	0.498	0.501	0.041	0.051	626
Numeracy	Low	0.192	0.395	-0.052	0.039	626
	Medium	0.216	0.412	-0.033	0.043	626
	High	0.591	0.492	0.086*	0.051	626
Household size		7.058	3.215	0.222	0.382	626
Number of children		3.171	2.239	-0.080	0.253	626
Number of elderly		0.072	0.332	0.001	0.034	626
Number of rooms		3.240	1.698	0.005	0.161	626
Number of buildings		3.639	2.029	-0.023	0.197	626
(Log) distance to KCB branch		2.749	2.089	-0.401***	0.137	617
Fatalities UCDP 2011-2014		0.023	0.202	-0.008	0.011	626
Fatalities UCDP 2015-2017		0.136	3.728	-0.184	0.220	626
Joint test of		F-stat			p-value	
orthogonality		1.65			0.036	
Panel B: State at baseline						
Central Equatoria		0.188	0.391	-0.030	0.032	626
Eastern Equatoria		0.240	0.428	-0.202***	0.031	626
Lakes		0.063	0.242	0.218***	0.028	626
Northern Bahr El Ghazal		0.125	0.331	0.132***	0.031	626
Western Bahr El Ghazal		0.091	0.288	0.161***	0.029	626
Western Equatoria		0.293	0.456	-0.279***	0.032	626
Joint test of		F-stat			p-value	
orthogonality		48.71			0.000	

Notes: Columns (1) and (2) report mean values and standard deviation of baseline characteristics for participants that received "training but no grant". In Panel A column (2) reports OLS estimates on receiving "training and grant" and state fixed effect, in Panel B column (2) reports simple OLS estimates on receiving "training and grant". Standard errors are clustered at the boma level and reported below coefficients in parentheses. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.3: Balancing between "Distant" vs. "Close" to a KCB bank branch on main outcomes

	(1)	(2)	(3)	(4)	(5)
	Distance dummy = 0		Distance dummy = 1		
	Mean	SE	Coeff.	SD	N
Panel A: Main outcome indices					
Employment index	-0.006	1.012	-0.124	0.139	764
Consumption index	-0.009	1.055	-0.151	0.135	764
Savings index	-0.027	0.993	-0.018	0.143	764
Business skills index	-0.024	1.007	0.090	0.139	764
Psychological index	-0.088	0.944	0.166	0.147	764
Risk index	-0.018	0.999	-0.036	0.134	764
Trust index	-0.018	0.999	0.157	0.147	764
Crime and violence index	0.039	0.985	0.272	0.210	764
Migration index	0.059	1.003	0.071	0.170	764
Joint test of orthogonality	F-stat		p-value		
	1.05		0.3986		

Notes: Columns (1) and (2) report mean values and standard deviation of baseline covariates for participants that lived close to a KCB bank branch. Columns (3) and (4) reports OLS estimates on the "distant" dummy and strata fixed effect. Standard errors are clustered at boma level and reported below coefficients in parentheses.* (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.4: Balancing between "Distant" vs. "Close" to a KCB bank branch on baseline covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Distance dummy = 0		Distance dummy = 1				
	Mean	SE	Coeff.	SD	Coeff.	SD	N
Age	27.505	4.735	0.738	0.526	-0.658	0.824	1,523
Gender	0.635	0.482	-0.025	0.056	0.088	0.080	1,523
Married	0.674	0.469	0.041	0.035	-0.086	0.049	1,523
Employment status	0.586	0.493	0.055	0.049	-0.021	0.084	1,523
Business ownership	0.622	0.485	0.124***	0.040	0.086	0.063	1,523
Consumption food	5.512	1.087	-0.077	0.098	0.119	0.128	1,523
Consumption nonfood	2.504	1.368	-0.253*	0.136	0.087	0.151	1,523
Formal bank account	0.383	0.486	-0.021	0.043	-0.002	0.061	1,523
(Log) amount formal loans	-0.300	1.648	0.171	0.107	-0.030	0.102	1,515
(Log) amount informal loans	-1.554	3.516	-0.234	0.384	0.046	0.449	1,500
No education	0.228	0.420	-0.006	0.043	-0.134	0.054	1,523
Primary education	0.279	0.449	0.102*	0.061	0.136	0.075	1,523
Secondary education	0.372	0.483	-0.060	0.052	-0.018	0.060	1,523
University education	0.121	0.326	-0.036**	0.018	0.015	0.023	1,523
No English	0.270	0.444	0.071	0.045	0.015	0.049	1,523
Some English	0.283	0.450	-0.009	0.053	0.048	0.061	1,523
Good English	0.447	0.497	-0.062	0.049	-0.063	0.059	1,523
Low numeracy	0.270	0.444	0.039	0.045	-0.058	0.051	1,523
Medium numeracy	0.153	0.360	0.027	0.041	0.084	0.057	1,523
High numeracy	0.577	0.494	-0.065	0.049	-0.026	0.062	1,523
Household size	7.540	3.334	0.147	0.441	0.280	0.595	1,523
Number of children	3.260	2.262	0.286	0.256	0.038	0.350	1,523
Number of elderly	0.108	0.360	-0.046	0.031	-0.073	0.039	1,523
Number of rooms	3.044	1.618	0.113	0.184	0.321	0.258	1,523
Number of buildings	3.348	1.871	0.357	0.231	0.163	0.317	1,523
(Log) distance to any bank	1.436	1.103	1.073***	0.232	0.044	0.254	1,190
(Log) distance to city	1.434	1.047	2.251***	0.348			1,500
(Log) distance to road	0.513	0.559	1.489***	0.189			1,500
Land gradient	0.724	0.506	0.492*	0.278			1,500
Fatalities UCDP 2011-2014	0.022	0.130	-0.008***	0.002			1,523
Fatalities UCDP 2015-2017	0.253	2.466	-0.384***	0.075			1,523
Joint test of orthogonality			F-stat	p-value	F-stat	p-value	
			22.53	0.0000	1.09	0.3492	

Notes: Columns (1) and (2) report mean values and standard deviation of baseline covariates for participants that lived close to a KCB bank branch. Columns (3) and (4) report OLS estimates on the "distant" dummy and strata fixed effect. Standard errors are clustered at the boma level and reported below coefficients in parentheses. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.5: Placebo test of first stage results from LATE estimation

		(1) "Training, no grant"	(2) "Training and grant"	(3) "Training, no grant"	(4) "Training and grant"	(5) "Training, no grant"	(6) "Training and grant"
Instrument 1	Treatment	0.5572***	0.2581***	0.5553***	0.2651***	0.4622***	0.3188***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Instrument 2	Treatment x (log) Distance to any bank	0.0044	0.0061	0.0039	0.0052	0.0065	0.0020
		(0.820)	(0.712)	(0.843)	(0.751)	(0.761)	(0.912)
	(Log) distance to any bank	0.0236***	-0.0178**	0.0226***	-0.0170**	0.0140*	-0.0130*
		(0.005)	(0.011)	(0.009)	(0.020)	(0.071)	(0.074)
	Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
	Individual controls	No	No	Yes	Yes	Yes	Yes
	Geography controls	No	No	No	No	Yes	Yes
	K-stat	0.0132		0.0087		0.0033	
	Observations	1,190	1,167	1,167	1,151	1,151	

Notes: Columns (1) and (2) correspond to LATE estimates without control variables, columns (3) and (4) correspond to LATE estimates with baseline controls, and columns (5) and (6) to LATE estimates with baseline and geographic controls. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.6: Baseline difference between attritors and non-attritors

		Non-attritors		Attritors		
		Mean	SD	Coeff.	SE	N
Panel A: Individual and household characteristics						
Age		27.632	4.826	-0.251	0.233	2,292
Gender		0.636	0.481	-0.088	0.022***	2,292
Married		0.661	0.473	-0.020	0.028	2,291
Employment status		0.619	0.486	0.006	0.020	2,292
Business ownership		0.649	0.478	0.009	0.018	2,292
Consumption food		5.405	1.170	-0.141	0.054***	2,292
Consumption nonfood		2.432	1.325	-0.036	0.065	2,292
Formal bank account		0.397	0.489	-0.068	0.022***	2,292
(Log) amount formal loans		-0.290	1.626	-0.180	0.086**	2,280
(Log) amount informal loans		-1.360	3.323	0.290	0.127**	2,258
Education level	No education	0.210	0.408	-0.048	0.020**	2,292
	Some Primary	0.307	0.462	0.050	0.019***	2,292
	Some Secondary	0.379	0.485	0.035	0.022	2,292
	Some University or Higher	0.104	0.305	-0.037	0.011***	2,292
Literacy	No English	0.261	0.440	-0.030	0.020	2,292
	Some English	0.286	0.452	-0.001	0.020	2,292
	Good English	0.453	0.498	0.031	0.025	2,292
Numeracy	Low	0.252	0.434	-0.043	0.018**	2,292
	Medium	0.173	0.378	0.023	0.017	2,292
	High	0.575	0.494	0.020	0.021	2,292
Household size		7.384	3.342	-0.342	0.148**	2,292
Number of children		3.248	2.294	-0.254	0.110**	2,292
Number of elderly		0.098	0.344	-0.001	0.014	2,292
Number of rooms		3.179	1.691	-0.116	0.079	2,292
Number of buildings		3.611	1.989	-0.007	0.079	2,292
(Log) distance to KCB branch		2.338	1.938	0.251	0.139*	2,256
Fatalities UCDP 2011-2014		0.017	0.112	-0.004	0.002	2,292
Fatalities UCDP 2015-2017		0.083	2.139	-0.054	0.059	2,292
Joint test of		F-stat			p-value	
orthogonality		4.00			0.000	
Panel B: State at baseline						
Central Equatoria		0.171	0.376	-0.008	0.026	2,292
Eastern Equatoria		0.154	0.361	0.008	0.045	2,292
Lakes		0.147	0.354	0.034	0.026	2,292
Northern Bahr El Ghazal		0.175	0.380	-0.006	0.025	2,292
Western Bahr El Ghazal		0.171	0.376	0.002	0.037	2,292
Western Equatoria		0.183	0.387	-0.030	0.047	2,292
Joint test of		F-stat			p-value	
orthogonality		3.21			0.007	

Notes: Columns (1) and (2) report mean values and standard deviations of baseline characteristics for non-attritors. In Panel A columns (3) and (4) report results of OLS estimates on attrition conditional on state fixed effect, in Panel B column (2) reports simple OLS estimates on receiving "training and grant". Standard errors are clustered at the boma level and reported below coefficients in parentheses. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.7: Difference in attrition probability between original treatment and control group

	(1) Control mean (SD)	(2) Treatment	(3) N
Attrition	0.335	0.002	2,292
	(0.472)	(0.018)	

Notes: Difference in attrition probability between the original treatment vs. the control group, estimated with an OLS regression of the attrition dummy on the treatment dummy and strata fixed effects. The standard error of the treatment dummy is clustered at the boma level and reported in parentheses. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.8: Baseline difference between attritors from the original control vs. attritors from the original treatment group

		(1)	(2)	(3)	(4)	(5)
		Control group		ITT group		
		Mean	SD	Coeff.	SE	N
Individual and household characteristics						
Age		27.226	5.186	0.450	0.362	769
Gender		0.582	0.494	-0.073	0.040*	769
Married		0.670	0.471	-0.031	0.031	768
Employment status		0.644	0.479	-0.044	0.036	769
Business ownership		0.670	0.471	-0.022	0.030	769
Consumption food		5.223	1.334	0.108	0.092	769
Consumption nonfood		2.447	1.287	-0.071	0.104	769
Formal bank account		0.322	0.468	0.015	0.030	769
(Log) amount formal loans		-0.386	1.859	-0.178	0.142	765
(Log) amount informal loans		-1.017	2.913	-0.181	0.216	758
Education level	No education	0.190	0.393	-0.039	0.024	769
	Some Primary	0.340	0.474	0.029	0.032	769
	Some Secondary	0.410	0.493	0.001	0.031	769
	Some University or Higher	0.060	0.237	0.009	0.015	769
Literacy	No English	0.249	0.433	-0.031	0.033	769
	Some English	0.249	0.433	0.062	0.030**	769
	Good English	0.501	0.501	-0.031	0.036	769
Numeracy	Low	0.231	0.422	-0.032	0.027	769
	Medium	0.190	0.393	0.003	0.030	769
	High	0.579	0.494	0.029	0.037	769
Household size		7.182	3.463	-0.206	0.256	769
Number of children		3.026	2.301	0.012	0.161	769
Number of elderly		0.117	0.360	-0.038	0.024	769
Number of rooms		3.091	1.784	-0.068	0.098	769
Number of buildings		3.670	1.836	-0.103	0.109	769
(Log) distance to KCB branch		2.593	1.962	-0.096	0.104	756
Fatalities UCDP 2011-2014		0.014	0.054	-0.002	0.003	769
Fatalities UCDP 2015-2017		-0.004	1.130	0.012	0.068	769
Joint test of orthogonality		F-stat 1.39			p-value 0.0789	
State at baseline						
Central Equatoria		0.174	0.380	-0.011	0.011	769
Eastern Equatoria		0.169	0.375	0.000	0.000	769
Lakes		0.164	0.370	0.003	0.003	769
Northern Bahr El Ghazal		0.174	0.380	0.008	0.007	769
Western Bahr El Ghazal		0.148	0.356	0.001	0.004	769
Western Equatoria		0.171	0.377	0.000	0.004	769
Joint test of orthogonality		F-stat 1.33			p-value 0.2487	

Notes: Columns (1) and (2) report mean values and standard deviation of baseline characteristics for attritors from the control group. In Panel A, columns (3) and (4) report results from OLS estimates on being an attritor from the treatment group conditional on state fixed effect, in Panel B, column (3) and (4) report results from a simple OLS estimation on being an attritor from the treatment group. Standard errors are clustered at the boma level and reported below coefficients in parentheses. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.3.9: Attrition – Baseline difference between difficult-to-reach endline participants and attritors

		(1)	(2)	(3)	(4)	(5)
		Difficult-to-reach		Attritors		
		Mean	SD	Coeff.	SE	N
Individual and household characteristics						
Age		27.699	5.004	-0.216	0.292	1,248
Gender		0.666	0.472	-0.123***	0.027	1,248
Married		0.714	0.452	-0.054	0.033	1,247
Employment status		0.662	0.474	0.007	0.026	1,248
Business ownership		0.706	0.456	-0.001	0.027	1,248
Consumption food		5.290	1.238	-0.059	0.069	1,248
Consumption nonfood		2.326	1.277	-0.007	0.093	1,248
Formal bank account		0.336	0.473	-0.037	0.028	1,248
(Log) amount formal loans		-0.157	1.203	-0.256**	0.100	1,241
(Log) amount informal loans		-1.263	3.175	0.245	0.181	1,233
Education level	No education	0.319	0.467	-0.121***	0.028	1,248
	Some Primary	0.351	0.478	0.035	0.027	1,248
	Some Secondary	0.261	0.440	0.110***	0.026	1,248
	Some University or Higher	0.069	0.254	-0.024*	0.014	1,248
Literacy	No English	0.376	0.485	-0.112***	0.030	1,248
	Some English	0.267	0.443	0.019	0.027	1,248
	Good English	0.357	0.480	0.093***	0.035	1,248
Numeracy	Low	0.363	0.481	-0.122***	0.026	1,248
	Medium	0.163	0.370	0.031	0.022	1,248
	High	0.474	0.500	0.091***	0.029	1,248
Household size		7.376	3.302	-0.317*	0.187	1,248
Number of children		3.457	2.355	-0.352**	0.143	1,248
Number of elderly		0.100	0.334	-0.002	0.019	1,248
Number of rooms		3.046	1.552	-0.074	0.108	1,248
Number of buildings		3.777	2.013	-0.100	0.100	1,248
(Log) distance to KCB branch		2.790	1.918	-0.073	0.147	1,230
Events UCDP 2011-2014		0.038	0.073	0.005	0.006	1,248
Events UCDP 2015-2017		-0.062	1.193	0.230***	0.087	1,248
Fatalities UCDP 2011-2014		0.012	0.058	-0.004	0.003	1,248
Fatalities UCDP 2015-2017		0.029	1.367	-0.061	0.079	1,248
Events ACLED 2011-2014		0.020	0.026	0.001	0.001	1,248
Events ACLED 2015-2017		0.012	0.011	0.002*	0.001	1,248
Fatalities ACLED 2011-2014		0.014	0.033	-0.001	0.002	1,248
Fatalities ACLED 2015-2017		0.010	0.011	0.001*	0.001	1,248
Joint test of		F-stat		p-value		
orthogonality		3.14		0.000		
State at baseline						
Central Equatoria		0.208	0.406	-0.118***	0.029	1,523
Eastern Equatoria		0.184	0.388	-0.096**	0.049	1,523
Lakes		0.148	0.356	-0.004	0.024	1,523
Northern Bahr El Ghazal		0.126	0.333	0.153***	0.040	1,523
Western Bahr El Ghazal		0.149	0.357	0.068*	0.037	1,523
Western Equatoria		0.184	0.388	-0.002	0.035	1,523
Joint test of		F-stat		p-value		
orthogonality		1.79		0.111		

Notes: "Dificult-to-reach" is defined as not reached during the phone survey that preceded the endline. Columns (1) and (2) report mean values and standard deviations of baseline characteristics for difficult-to-reach participants. In Panel A, columns (3) and (4) report OLS estimates on attrition conditional on state fixed effect, in Panel B, columns (3) and (4) report simple OLS estimates on attrition. Standard errors are clustered at the boma level and reported below coefficients in parentheses. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

C.4 Treatment on the treated estimates

Table C.4.1: Treatment on the treated estimates of the "training and grant" vs. "training, but no grant" on main socio-economic outcomes

		(1) TOT (no controls)	(2) TOT (controls)	(3) TOT (controls + geography controls)
		Main outcomes – Socio-economic		
Employment index	"Training, no grant"	0.087	0.086	0.147*
		(0.149)	(0.134)	(0.094)
	"Training and grant"	0.057	0.062	0.115
		(0.580)	(0.554)	(0.351)
Consumption index	"Training, no grant"	0.046	0.037	-0.011
		(0.489)	(0.591)	(0.907)
	"Training and grant"	0.178**	0.157**	0.131
		(0.023)	(0.048)	(0.217)
Savings index	"Training no grant"	0.221***	0.205***	0.177**
		(0.000)	(0.000)	(0.022)
	"Training and grant"	0.434***	0.420***	0.379***
		(0.000)	(0.000)	(0.000)
Business skills index	"Training, no grant"	-0.031	-0.022	-0.011
		(0.594)	(0.727)	(0.906)
	"Training and grant"	0.240***	0.220***	0.263**
		(0.004)	(0.010)	(0.019)
Observations		1,523	1,495	1,474

Notes: All regression control for gender-state fixed effects. P-values are in parenthesis displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.4.2: Treatment on the treated estimates of the "training and grant" vs. "training, but no grant" on main psychological and behavioral outcomes

		(1) TOT (no controls)	(2) TOT (controls)	(3) TOT (controls + geography controls)
	Main outcomes – Psychological and behavioral			
Psychological wellbeing index	"Training, no grant"	0.029	0.035	0.054
		(0.585)	(0.490)	(0.444)
	"Training and grant"	0.027	-0.014	-0.015
		(0.716)	(0.847)	(0.862)
Risk index	"Training, no grant"	0.016	0.000	0.089
		(0.839)	(0.998)	(0.404)
	"Training and grant"	-0.068	-0.076	0.003
		(0.365)	(0.327)	(0.973)
Trust index	"Training, no grant"	-0.144**	-0.169***	-0.162*
		(0.012)	(0.002)	(0.081)
	"Training and grant"	0.057	0.072	0.104
		(0.430)	(0.329)	(0.301)
Crime and violence index	"Training, no grant"	-0.051	-0.061	0.033
		(0.414)	(0.331)	(0.700)
	"Training and grant"	-0.104	-0.103	-0.066
		(0.170)	(0.190)	(0.415)
Migration index	"Training, no grant"	-0.080	-0.078	-0.102
		(0.150)	(0.167)	(0.212)
	"Training and grant"	0.029	0.018	-0.002
		(0.738)	(0.823)	(0.984)
Observations		1,523	1,495	1,474

Notes: All regression control for gender-state fixed effects. P-values are in parenthesis. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

C.5 *Ex post* minimum detectable effect sizes

Table C.5.1: *Ex post* minimum detectable effect size: ITT estimates

	(1) No controls	(2) Controls
Employment index	0.164	0.160
Consumption index	0.169	0.169
Savings index	0.155	0.153
Business skills index	0.139	0.148
Psychological wellbeing index	0.135	0.126
Risk index	0.177	0.167
Trust index	0.137	0.140
Crime and violence index	0.143	0.147
Migration index	0.137	0.144

Table C.5.2: *Ex post* minimum detectable effect size: LATE estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	no controls		controls		(controls + (geography controls)	
	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"
Employment index	0.649	1.185	0.614	1.102	0.648	0.850
Consumption index	0.464	1.321	0.449	1.183	0.446	0.914
Savings index	0.427	1.035	0.441	1.033	0.526	0.889
Business skills index	0.490	0.974	0.528	1.052	0.423	0.872
Psychological wellbeing index	0.465	0.925	0.441	0.865	0.333	0.706
Risk index	0.757	1.515	0.703	1.414	0.893	1.174
Trust index	0.571	1.042	0.591	1.094	0.400	0.767
Crime and violence index	0.799	1.405	0.833	1.461	0.643	0.954
Migration index	0.464	1.030	0.505	1.074	0.543	0.848

Table C.5.3: *Ex post* minimum detectable effect size: LATE estimates for "training, no grant" and "training and grant" by gender

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		LATE for males			**LATE for females**			Coeff
		(no controls)	(controls)	(controls + geo controls)	(no controls)	(controls)	(controls + geo controls)	equality (3) vs (6)
Employment index	grant	1.836	1.681	1.269	1.269	1.194	0.976	1.269
	no grant	1.062	1.073	1.361	1.361	0.569	0.783	1.361
Consumption index	grant	1.672	1.597	1.198	1.198	1.431	0.988	1.198
	no grant	0.886	0.912	0.718	0.718	0.565	1.122	0.718
Savings, investment & debt index	grant	1.815	1.680	1.286	1.286	1.073	0.955	1.286
	no grant	0.943	0.929	0.724	0.724	0.454	0.762	0.724
Business skills index	grant	1.595	1.726	1.342	1.342	1.303	1.025	1.342
	no grant	0.978	1.108	0.749	0.749	0.617	1.093	0.749
Psychological wellbeing index	grant	1.442	1.451	1.136	1.136	1.177	0.932	1.136
	no grant	0.944	0.987	0.950	0.950	0.452	0.716	0.950
Risk index	grant	1.667	1.689	1.159	1.159	1.615	1.180	1.159
	no grant	1.015	1.039	0.952	0.952	0.707	1.443	0.952
Trust index	grant	1.421	1.461	1.167	1.167	1.287	0.991	1.167
	no grant	1.007	1.073	1.145	1.145	0.498	0.723	1.145
Crime/violence index	grant	1.772	1.754	1.485	1.485	1.827	1.104	1.485
	no grant	1.073	1.188	0.994	0.994	0.893	1.124	0.994
Migration index	grant	1.288	1.307	1.103	1.103	1.468	1.139	1.103
	no grant	0.790	0.907	1.147	1.147	0.508	0.993	1.147

C.6 Robustness tables

Table C.6.1: Lee bounds for the intention-to-treat effects on main socio-economic outcomes

	(1) Lower bound	(2) Upper bound
Main outcomes – Socio-economic		
Employment index	0.045	0.047
	(0.610)	(0.810)
Consumption index	0.093	0.098
	(0.173)	(0.538)
Savings, investment and debt	0.261**	0.268**
index	(0.031)	(0.047)
Business skills index	0.007	0.009
	(0.942)	(0.926)
Observations	2,292	

Notes: P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.2: Lee bounds for the intention-to-treat effects on main psychological and behavioral outcomes

	(1) Lower bound	(2) Upper bound
Main outcomes – Psychological and behavioral		
Psychological wellbeing index	-0.005	-0.002
	(0.961)	(0.989)
Risk index	-0.052	-0.049
	(0.595)	(0.645)
Trust index	-0.055	-0.050
	(0.590)	(0.641)
Crime and violence index	-0.253***	-0.105
	(0.000)	(0.553)
Migration index	-0.027	-0.027
	(0.641)	(0.826)
Observations	2,292	

Notes: P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.3: Weighted intention-to-treat effects of the original intervention on main socio-economic outcomes.

	(1) ITT (no controls)	(2) ITT (controls)
Main outcomes – Socio-economic		
Employment index	0.069	0.074
	(0.257)	(0.223)
Consumption index	0.103	0.094
	(0.103)	(0.128)
Savings, investment and debt index	0.283***	0.281***
	(0.000)	(0.000)
Business skills index	0.024	0.023
	(0.633)	(0.668)
Observations	1,495	1,507

Notes: All regressions control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.4: Weighted intention-to-treat effects of the original intervention on main psychological and behavioral outcomes.

	(1) ITT (no controls)	(2) ITT (controls)
Main outcomes – Psychological and behavioral		
Psychological wellbeing index	-0.002	0.004
	(0.961)	(0.929)
Risk index	-0.048	-0.062
	(0.408)	(0.287)
Trust index	-0.033	-0.045
	(0.513)	(0.385)
Crime and violence index	-0.077	-0.086
	(0.145)	(0.105)
Migration index	-0.038	-0.026
	(0.442)	(0.613)
Observations	1,495	1,495

Notes: All regressions control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.5: Weighted local average treatment effects of the "training and grant" vs. "training, but no grant" on main socioeconomic outcomes

	(1) "Training, no grant"	(2) "Training and grant"	(3) "Training, no grant"	(4) "Training and grant"	(5) "Training, no grant"	(6) "Training and grant"	(7) (5) - (6)
	(no controls)		(controls)		(controls + geography controls)		
Main outcomes – Socioeconomic							
Employment index	-0.050	0.351	-0.041	0.335	0.245	0.290	-0.046
	(0.838)	(0.450)	(0.864)	(0.434)	(0.289)	(0.364)	(0.920)
Consumption index	-0.429*	1.130*	-0.352*	0.937**	-0.390**	0.701**	-1.091***
	(0.052)	(0.058)	(0.060)	(0.048)	(0.017)	(0.044)	(0.005)
Savings index	-0.208	1.367***	-0.198	1.326***	-0.150	1.128***	-1.279***
	(0.278)	(0.007)	(0.287)	(0.003)	(0.467)	(0.002)	(0.001)
Business skills index	0.006	0.065	0.045	-0.015	0.078	-0.075	0.152
	(0.976)	(0.873)	(0.826)	(0.970)	(0.592)	(0.820)	(0.694)
Observations	1,474		1,474		1,474		
F-stat	4.041		4.165		14.084		

Notes: All regressions control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.6: Weighted local average treatment effects of the "training and grant" vs. "training, but no grant" on main psychological and behavioral outcomes

	(1) "Training, no grant"	(2) "Training and grant"	(3) "Training, no grant"	(4) "Training and grant"	(5) "Training, no grant"	(6) "Training and grant"	(7) (5) - (6)
	(no controls)		(controls)		(controls + geography controls)		
Main outcomes – Psychological and behavioral							
Psychological wellbeing index	-0.156	0.268	-0.083	0.140	-0.215*	0.229	-0.444
	(0.366)	(0.439)	(0.632)	(0.673)	(0.089)	(0.392)	(0.155)
Risk index	-0.470	0.715	-0.435	0.599	-0.223	0.467	-0.690
	(0.096)	(0.218)	(0.110)	(0.263)	(0.487)	(0.273)	(0.307)
Trust index	0.015	-0.151	0.004	-0.161	-0.383**	0.112	-0.495
	(0.951)	(0.732)	(0.988)	(0.712)	(0.027)	(0.709)	(0.207)
Crime and violence index	-0.526	0.677	-0.574*	0.716	-0.172	0.274	-0.446
	(0.132)	(0.260)	(0.088)	(0.213)	(0.468)	(0.444)	(0.389)
Migration index	-0.302	0.474	-0.329	0.556	-0.141	0.190	-0.331
	(0.162)	(0.278)	(0.132)	(0.196)	(0.490)	(0.551)	(0.449)
Observations	1,474		1,474		1,474		
F-stat	4.041		4.165		14.084		

Notes: All regressions control for gender-state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.7: Weighted local average treatment effects of the "training and grant" vs. "training, but no grant" on main socio-economic outcomes by gender

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		LATE for males			**LATE for females**			Coeff equality
		(no controls)	(controls)	(controls + geo controls)	(no controls)	(controls)	(controls + geo controls)	(3) vs. (6)
		Main outcomes – Socio-economic						
Employment index	"training, no grant"	0.228	0.106	0.293	-0.189	-0.091	0.447	0.154
		(0.587)	(0.795)	(0.519)	(0.472)	(0.682)	(0.137)	(0.729)
	"training and grant"	-0.136	-0.050	0.122	0.716	0.536	0.427	0.305
		(0.856)	(0.940)	(0.797)	(0.210)	(0.249)	(0.253)	(0.597)
Consumption index	"training, no grant"	-0.289	-0.276	-0.109	-0.458*	-0.335	-0.960**	-0.851
		(0.454)	(0.465)	(0.708)	(0.089)	(0.138)	(0.030)	(0.159)
	"training and grant"	0.654	0.504	0.276	1.386*	1.117*	0.483	0.206
		(0.363)	(0.435)	(0.551)	(0.066)	(0.053)	(0.187)	(0.728)
Savings index	"training, no grant"	-0.520	-0.493	-0.394	-0.049	-0.020	-0.297	0.098
		(0.225)	(0.198)	(0.160)	(0.802)	(0.910)	(0.303)	(0.790)
	"training and grant"	1.901**	1.693**	1.219**	0.968*	0.908**	0.666*	-0.553
		(0.034)	(0.017)	(0.014)	(0.060)	(0.033)	(0.072)	(0.341)
Business skills	"training, no grant"	0.049	-0.001	0.727***	-0.065	0.007	-0.141	-0.868*
		(0.909)	(0.998)	(0.007)	(0.780)	(0.975)	(0.727)	(0.092)
	"training and grant"	0.239	0.279	0.408	0.007	-0.134	-0.211	-0.619
		(0.741)	(0.703)	(0.437)	(0.989)	(0.778)	(0.568)	(0.385)
Observations		541	541	541	933	933	933	1,474
F-stat		3.384	3.516	13.759	4.293	4.726	7.648	

Notes: All regressions control for state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.8: Weighted local average treatment effects of the "training and grant" vs. "training, but no grant" on main psychological and behavioral outcomes by gender

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		LATE for males			**LATE for females**			Coeff
		(no controls)	(controls)	(controls + geo controls)	(no controls)	(controls)	(controls + geo controls)	equality (3) vs. (6)
Main outcomes – psychological and behavioral								
Psychological wellbeing index	"training, no grant"	0.020	0.089	-0.314	-0.298	-0.172	-0.207	0.107
		(0.958)	(0.817)	(0.363)	(0.137)	(0.327)	(0.443)	(0.837)
	"training and grant"	0.220	0.059	0.151	0.369	0.172	0.250	0.098
		(0.713)	(0.919)	(0.724)	(0.475)	(0.699)	(0.482)	(0.870)
Risk index	"training, no grant"	-0.449	-0.460	0.286	-0.466	-0.392	-1.230**	-1.517***
		(0.266)	(0.263)	(0.401)	(0.141)	(0.137)	(0.025)	(0.002)
	"training and grant"	0.608	0.645	0.046	0.779	0.614	0.631	0.585
		(0.381)	(0.339)	(0.916)	(0.274)	(0.298)	(0.151)	(0.241)
Trust index	"training, no grant"	-0.171	-0.299	-0.247	-0.197	-0.208	-0.936***	-0.689
		(0.645)	(0.419)	(0.415)	(0.315)	(0.230)	(0.002)	(0.149)
	"training and grant"	0.226	0.361	0.562	-0.143	-0.205	-0.010	-0.571
		(0.704)	(0.522)	(0.158)	(0.734)	(0.595)	(0.977)	(0.289)
Crime and violence index	"training, no grant"	-0.384	-0.448	-0.072	-0.647	-0.695*	-0.529	-0.457
		(0.407)	(0.361)	(0.843)	(0.121)	(0.054)	(0.246)	(0.390)
	"training and grant"	0.699	0.713	0.216	0.760	0.790	0.208	-0.008
		(0.361)	(0.325)	(0.704)	(0.338)	(0.264)	(0.622)	(0.990)
Migration index	"training, no grant"	0.033	-0.006	0.200	-0.460*	-0.452**	-0.405	-0.604
		(0.920)	(0.988)	(0.634)	(0.081)	(0.038)	(0.280)	(0.337)
	"training and grant"	-0.180	-0.022	-0.053	0.919	0.880	0.282	0.335
		(0.738)	(0.967)	(0.899)	(0.177)	(0.129)	(0.508)	(0.579)
Observations		541	541	541	933	933	933	1,474
F-stat		3.384	3.516	13.759	4.293	4.726	7.648	

Notes: All regressions control for state fixed effects. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.9: Robustness of the intention to treat effects of Table III.4 and III.5 to different conflict measures

	(1) UCPD fatalities	(2) UCPD events	(3) ACLED fatalities	(4) ACLED events
Employment index	0.070	0.067	0.068	0.068
	(0.221)	(0.245)	(0.231)	(0.236)
Consumption index	0.074	0.079	0.075	0.075
	(0.214)	(0.184)	(0.204)	(0.206)
Savings index	0.268***	0.269***	0.267***	0.268***
	(0.000)	(0.000)	(0.000)	(0.000)
Business skills index	0.014	0.012	0.016	0.017
	(0.796)	(0.816)	(0.758)	(0.748)
Psychological wellbeing index	-0.007	-0.008	-0.005	-0.006
	(0.882)	(0.867)	(0.904)	(0.902)
Risk index	-0.047	-0.051	-0.049	-0.048
	(0.416)	(0.378)	(0.403)	(0.410)
Trust index	-0.119	-0.120	-0.119	-0.118
	(0.016)	(0.015)	(0.016)	(0.017)
Crime and violence index	-0.105	-0.103	-0.104	-0.104
	(0.045)	(0.046)	(0.045)	(0.045)
Migration index	-0.006	-0.007	-0.005	-0.007
	(0.911)	(0.898)	(0.921)	(0.889)
Observations	1,474	1,474	1,474	1,474

Notes: All regressions control for gender-state fixed effects, baseline controls and geographic controls. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.10: Robustness of local average treatment effects of Table III.6 to different conflict measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	UCPD fatalities		UCPD events		ACLED fatalities		ACLED events	
	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"
				Main outcomes – Socio-economic				
Employment	0.231	0.286	0.186	0.475	0.202	0.296	0.182	0.373
index	(0.325)	(0.351)	(0.403)	(0.277)	(0.359)	(0.321)	(0.416)	(0.229)
Consumption	-0.411**	0.708**	-0.375**	1.142**	-0.372**	0.620*	-0.388**	0.721*
index	(0.014)	(0.036)	(0.026)	(0.015)	(0.015)	(0.058)	(0.014)	(0.064)
Savings	-0.133	1.094***	-0.178	1.344***	-0.160	1.100***	-0.180	1.175***
index	(0.483)	(0.001)	(0.359)	(0.003)	(0.385)	(0.001)	(0.343)	(0.001)
Business	0.038	-0.029	0.058	-0.048	0.073	-0.167	0.066	-0.236
skills index	(0.804)	(0.927)	(0.693)	(0.911)	(0.620)	(0.566)	(0.665)	(0.449)
Observations	1,474		1,474		1,474		1,474	
F-stat	14.844		10.510		19.111		16.996	

Notes: All regressions control for gender-state fixed effects, baseline controls and geographic controls. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.11: Robustness of local average treatment effects of Table III.7 to different conflict measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	UCPD fatalities		UCPD events		ACLED fatalities		ACLED events	
	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"	"Training, no grant"	"Training and grant"
	Main outcomes – Socio-economic							
Psychological wellbeing index	-0.217*	0.225	-0.229*	0.432	-0.199*	0.076	-0.215*	0.008
	(0.076)	(0.377)	(0.067)	(0.232)	(0.073)	(0.738)	(0.059)	(0.975)
Risk index	-0.256	0.507	-0.248	0.913	-0.260	0.513	-0.277	0.675
	(0.426)	(0.233)	(0.428)	(0.141)	(0.401)	(0.182)	(0.376)	(0.123)
Trust index	-0.412***	0.115	-0.321**	0.271	-0.355**	0.020	-0.327**	-0.097
	(0.006)	(0.677)	(0.027)	(0.463)	(0.014)	(0.941)	(0.028)	(0.722)
Crime and violence index	-0.167	0.282	-0.255	0.527	-0.182	0.109	-0.216	-0.003
	(0.472)	(0.412)	(0.275)	(0.260)	(0.388)	(0.723)	(0.323)	(0.993)
Migration index	-0.126	0.188	-0.182	0.124	-0.137	0.134	-0.176	0.229
	(0.518)	(0.539)	(0.337)	(0.770)	(0.455)	(0.628)	(0.348)	(0.464)
Observations	1,474		1,474		1,474		1,474	
F-stat	14.844		10.510		19.111		16.996	

Notes: All regressions control for gender-state fixed effects, baseline controls and geographic controls. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.12: Robustness of local average treatment effects of Table III.8 by gender to different conflict measures

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		UCPD fatalities		UCPD events		ACLED fatalities		ACLED events	
		male	**female**	**male**	**female**	**male**	**female**	**male**	**female**
		Main outcomes – Socio-economic							
Employment	no grant	0.173	0.488*	0.167	0.289	0.248	0.288	0.198	0.279
index		(0.723)	(0.081)	(0.718)	(0.238)	(0.578)	(0.232)	(0.654)	(0.249)
	grant	0.221	0.369	0.916	0.072	-0.063	0.530*	-0.190	0.549*
		(0.625)	(0.289)	(0.134)	(0.863)	(0.902)	(0.099)	(0.728)	(0.089)
Consumption	no grant	-0.139	-0.899**	-0.076	-0.856**	-0.056	-0.839**	-0.061	-0.884**
index		(0.587)	(0.025)	(0.757)	(0.019)	(0.811)	(0.013)	(0.796)	(0.012)
	grant	0.337	0.460	0.623	0.964**	0.128	0.447	0.103	0.620*
		(0.431)	(0.192)	(0.246)	(0.044)	(0.781)	(0.165)	(0.852)	(0.076)
Savings	no grant	-0.407	-0.228	-0.392	-0.399*	-0.343	-0.440**	-0.341	-0.479**
index		(0.116)	(0.402)	(0.122)	(0.075)	(0.144)	(0.040)	(0.148)	(0.035)
	grant	1.212***	0.632*	1.532**	0.654	1.006**	0.871***	0.769	1.016***
		(0.008)	(0.064)	(0.012)	(0.169)	(0.022)	(0.008)	(0.110)	(0.002)
Business	no grant	0.681**	-0.109	0.700***	-0.128	0.762***	-0.080	0.749***	-0.080
skills		(0.011)	(0.780)	(0.005)	(0.713)	(0.004)	(0.814)	(0.005)	(0.818)
index	grant	0.427	-0.171	0.200	0.085	0.198	-0.243	0.082	-0.369
		(0.373)	(0.641)	(0.734)	(0.872)	(0.708)	(0.434)	(0.890)	(0.269)
Observations		541	933	541	933	541	933	541	933
F-stat		15.53	8.486	13.29	7.165	14.54	13.21	12.10	13.70

Notes: All regressions control for state fixed effects, baseline controls and geographic controls. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

Table C.6.13: Robustness of local average treatment effects of Table III.9 by gender to different conflict measures

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		UCPD fatalities		UCPD events		ACLED fatalities		ACLED events	
		male	**female**	**male**	**female**	**male**	**female**	**male**	**female**
Main outcomes – Socio-economic									
Psychological	no grant	-0.356	-0.182	-0.384	-0.169	-0.348	-0.073	-0.355	-0.095
wellbeing		(0.294)	(0.478)	(0.233)	(0.481)	(0.288)	(0.752)	(0.274)	(0.682)
index	grant	0.201	0.194	0.351	0.355	0.134	0.015	0.055	-0.073
		(0.620)	(0.560)	(0.498)	(0.420)	(0.752)	(0.960)	(0.910)	(0.813)
Risk	no grant	0.286	-1.156**	0.184	-0.822*	0.232	-0.902**	0.201	-0.931**
index		(0.401)	(0.025)	(0.555)	(0.075)	(0.479)	(0.039)	(0.540)	(0.035)
	grant	0.129	0.601	0.378	0.917	0.176	0.465	0.223	0.654*
		(0.755)	(0.154)	(0.475)	(0.142)	(0.699)	(0.181)	(0.676)	(0.091)
Trust	no grant	-0.132	-0.521**	-0.061	-0.376**	-0.220	-0.321	-0.167	-0.292
		(0.747)	(0.044)	(0.876)	(0.039)	(0.574)	(0.134)	(0.670)	(0.193)
	grant	0.424	0.028	0.117	0.731	0.792*	-0.222	1.035**	-0.390
		(0.309)	(0.937)	(0.817)	(0.110)	(0.063)	(0.508)	(0.027)	(0.265)
Crime and	no grant	-0.107	-0.419	-0.236	-0.508	-0.175	-0.323	-0.155	-0.396
violence		(0.763)	(0.297)	(0.479)	(0.158)	(0.610)	(0.313)	(0.647)	(0.231)
index	grant	0.247	0.200	0.392	0.446	0.323	-0.019	0.032	-0.115
		(0.642)	(0.611)	(0.501)	(0.438)	(0.574)	(0.950)	(0.956)	(0.706)
Migration	no grant	0.232	-0.348	0.148	-0.390	0.260	-0.275	0.218	-0.371
index		(0.571)	(0.326)	(0.702)	(0.244)	(0.509)	(0.385)	(0.580)	(0.256)
	grant	-0.082	0.324	-0.374	0.492	-0.244	0.261	-0.278	0.409
		(0.836)	(0.426)	(0.469)	(0.390)	(0.564)	(0.451)	(0.572)	(0.282)
Observation		541	933	541	933	541	933	541	933
F-stat		15.53	8.486	13.29	7.165	14.54	13.21	12.10	13.70

Notes: All regressions control for state fixed effects, baseline controls and geographic controls. P-values are in parentheses displayed below the estimated coefficients. * (**, ***) indicates statistical significance at the ten-percent (five-percent, one-percent) level.

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