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The

Impact and Implementation

of

International Development Finance

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To my dear parents

Heidi & Henning

Contents

List of Figures

List of Tables

Introduction

2023 marks the halfway point in the 2030 Agenda and the Sustainable Development Goals (SDGs). The SDGs constitute a guiding framework for coordinated efforts towards global development cooperation. Crucially, the signature countries recognize that ending poverty depends on several development outcomes, such as improving health and education, reducing inequality, and promoting economic growth in the developing world while coping with the adverse effects of climate change and using the world's scarce resources in a more sustainable manner.

Recent developments have put previous progress towards the 2030 Agenda under threat. The Covid-19 pandemic, Russia's 2022 invasion of Ukraine, and accelerating climate change are threatening to jeopardize decades of economic development in key areas, including eradicating poverty, zero hunger, and fighting global inequality. Rising geopolitical tensions—in particular between the United States and China—make it increasingly difficult to find coordinated policy responses. However, failure to find coordinated policy responses pushes individuals in distress to find alternative coping mechanisms, such as migration, often through informal channels.[1] The urgency to address these global economic, social, and environmental challenges has magnified the importance of effective resource allocation and utilization within and across states.

Global challenges require collective action. In consequence, the United Nations have identified international development finance as a key instrument to address the global development challenges and to achieve the 2030 Agenda (United Nations 2023). At the SDG Summit in September 2023, global policy makers called for the mobilization of additional financial resources to reach the agreed development goals. In particular, the summit stressed the importance for developed countries to commit 0.7 percent of their GNI on official development aid.

International development finance comprises of a vast variety of financial resources, instruments, and mechanisms implemented by a diverse group of actors. These actors include international institutions such as the World Bank or the International Monetary

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[1]International migration preferences are at a high level, with one in four individuals in the developing world indicating a preference to migrate (Gallup 2018), and climate change is expected to further increase the demand for migration in the coming decades (World Bank 2023).

Fund (IMF), multilateral organizations, national governments from developed and developing countries, and private actors. Broadly, international development finance aims at promoting economic growth, human development, and poverty reduction in the less developed regions of the world, but the precise goals are as diverse as the actors involved. International development finance encompasses financial instruments reaching from official development assistance to foreign direct investment and can be of concessional, semi-concessional, or commercial character.

This dissertation analyzes how international development finance is used by various actors to achieve their strategic goals. For this, it focuses on two central features of the early 21st century: the rise of China and its attempt to gain popular support across the globe and the efforts of the international community to tackle rising migration pressures in the developing world. Given the importance of this policy instrument, studying the impact of international development finance is essential to learn from the past and formulate better policies in the future. Evaluating policy, however, depends on precise measurement, and only granular data on the implementation of international development finance enables us to get a better understanding of the "true" effects of this policy instrument. By conjoining the two aspects, this thesis offers a comprehensive analysis of the impact and implementation of international development finance.

This dissertation provides new, fine-grained data on the implementation of international development finance across space and time. It uses worldwide data to systematically understand the impact and implementation of international development finance across different contexts. It uses geocoded data to trace the local impact of international development finance. It uses fine-grained temporal data to estimate the short-term effects of international development finance. Finally, it uses individual-level data to trace the agency of individuals in the implementation of international development finance, and the impact of international development finance on perceptions and livelihoods of individuals in recipient countries.

Separately, each chapter shades light on one important aspect of international development finance. The first chapter analyzes donor *interests* in international development finance, testing the effectiveness of international development finance as soft power instrument. The second chapter studies the role of *ideas* in the implementation of international development finance, tracing variation in policy output to the political ideology of individual bureaucrats. The third chapter examines the impact of international development finance implemented by international *institutions* on migration and development.

Interests. First, this thesis examines the role of national interest in international development finance. The objectives to provide international development finance are as manifold as the actors involved. Global challenges require collective action and development finance is more effective when coordinated (Milner and Tingley 2010). Yet, a

large share of development finance is still provided bilaterally.[2] The attractiveness of the bilateral approach also stems from the absence of a need to coordinate, as states use the greater policy autonomy to pursue their strategic goals. They channel aid strategically to former colonies (Alesina and Dollar 2000), friendly governments and allies in international organizations (Dreher et al. 2009, Faye and Niehaus 2012, Dippel 2015, Marx 2018), to gain access to relevant export markets (Rose 2019), and to prevent and fight terrorism or migration (Fleck and Kilby 2010, Czaika and Mayer 2011, Bermeo and Leblang 2015).

What is more, donor governments use aid to make strategic soft power gains among recipient countries and governments. With increasing geopolitical tensions, soft power has reemerged as central policy tool. As Goldsmith et al. (2014: 88) point out, "competition between major powers such as the United States and China for favorable perceptions in global public opinion is increasingly evident today and likely to be a pivotal feature of the emerging international order." Popular support facilitates trade relationships, fosters investments, and secures military support (Guiso et al. 2009, Goldsmith and Horiuchi 2012, Rose 2016, 2019). While states undertake considerable efforts to promote their development finance activities in recipient countries, the effect of international development finance on popular support for the donor government is a priori unclear. Successful cooperation might bolster popular support for donor governments, yet failures to deliver on development finance promises might also decrease donor approval in recipient countries. The first chapter of this dissertation provides a comprehensive analysis of the effect of international development finance on soft power, examining Chinese development finance implementation and its effects on popular support in project locations, recipient countries, and the entire developing world.

Ideas. Second, this thesis looks at the role of ideas in international development finance. A large share of international development finance is implemented by international organizations, such as the World Bank or the IMF. These institutions are set up as technocratic entities to coordinate collective action between states (Keohane 1984). They consist of large bureaucratic apparatuses that guarantee the day-to-day functions of the institution (Abbott and Snidal 1998, Barnett and Finnemore 1999, 2004). And while their mandate is designed to be objective and their policies promoted as technocratic, international organizations are often depicted as tools in the hands of powerful states (Strange 1988). Indeed, shareholder influence is documented in nearly all major international organizations (e.g., Dreher et al. 2009, 2022, Kilby 2009, Schneider and Tobin 2013, Carter and Stone 2015, Kersting and Kilby 2016).

Absent the interest of powerful states, large bureaucratic bodies are meant to guarantee the neutrality and technocratic decision-making of these institutions (Stone 2013). The bureaucracy itself is, however, also prone to institutional culture and incentives. Bureaucrats seek promotion, maximize budgets, and influence policy output (Vaubel 1986,

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[2]59% of official development aid, for instance, was implemented bilaterally in 2020 (OECD 2022).

Willett 2002, Dreher and Vaubel 2004, Dreher and Lang 2019). Yet, the heterogeneity among these individual preferences has only recently emerged in the literature on international bureaucrats (Copelovitch and Rickard 2021, Clark and Zucker 2023). The second chapter builds on this literature and argues that bureaucrats have heterogeneous political ideologies that manifest in the policy making of international institutions.

Institutions. Third, this thesis examines the impact of international development finance implemented by international institutions. Recent crises threaten to jeopardize decades of economic development and increase global inequalities. The international community has recognized this challenge and is multiplying its efforts to promote sustainable development and opportunity in developing countries—also to address rising migration pressures in the developing world. Indeed, development finance is one central instrument of Western governments to fight the "root causes" of irregular migration and to provide local opportunities in the developing world. The effectiveness of such development programs, however, has been contested for decades (Morgenthau 1962, Easterly 2003, Rajan and Subramanian 2008, Qian 2015, Clemens and Postel 2018).

Proponents argue that foreign aid might provide a "big push" to lead developing countries on a trajectory of sustained growth (Sachs et al. 2004), yet critics perceive aid as flawed and potentially counterproductive (Easterly 2003). Entangled with the general debate on the effectiveness of foreign aid, whether or not international development finance reduces migration remains an open question. Even if aid increases incomes, the effect on migration is unclear, as higher incomes might as well lead to *more* migration in developing countries where migration is costly and credit constraints are binding (Clemens 2014, Angelucci 2015, Bazzi 2017). The third chapter of this dissertation examines the impact of international development finance implemented by the World Bank—the world's largest international development finance institution—on local migration aspirations, livelihoods, and capabilities, as well as the relevant heterogeneities across space and time.

New Measures of the Implementation of International Development Finance

Chapter I collects new data on the implementation of Chinese development finance. For this, we revisit the initial data collection process implemented by AidData (Dreher et al. 2021b, Dreher et al. 2022). Based on the "Tracking Underreported Financial Flows" methodology, the original data set is based on official and unofficial sources that are publicly accessible and contain information on individual Chinese development projects (Strange et al. 2017b). For each project, we go back to the underlying source articles to identify precise commitment, start, and end dates of Chinese development finance projects. Where no dates are mentioned, we use excessive online search in English and Chinese language to identify the relevant dates. Commitment dates represent grant and loan agreement signings or official project announcements. Start dates are marked by foundation stone-laying and other forms of groundbreaking ceremonies. End dates mark

the end of construction, the point at which new infrastructure can be used, and/or the date on which a project passed final inspection. This study is the first to provide exact commitment dates for Chinese development projects implemented between 2000 and 2014 and substantially increased the coverage of start and end dates compared to existing data.

As part of Chapter I, we also map the data from the Gallup World Poll to the global province level using the available geographic identifiers of the data set. We again use this data set in Chapter III of this dissertation.

Chapter II collects data on the deployment and personal characteristics of IMF mission chiefs (MCs)—top IMF bureaucrats that are responsible for the negotiation of IMF programs with member countries. Building on an existing data set from Beaudry and Willems (2022), this chapter uses a mix of official information on article IV missions, publicly available data, and social media websites to identify MCs and follow their country assignment over time. In addition, we directly contacted IMF country offices and obtained detailed information on the MC deployment in the respective country to identify 835 IMF MCs over the time period from 1980 to 2016. In addition to these deployment patterns, we collect data on the biographies of MCs combining data from Nelson (2014) with biographical information that we gather from official IMF sources and on LinkedIn. Where we can not derive the data through these means, we use extensive web search to fill up missing data points. We code MC's year of birth, gender, nationality, education (including highest academic degree and alma mater), and year of entry into the Fund.

Chapter II also provides a new coding for IMF conditionality: the ideological leaning of individual policy conditions. To construct these measures, we extract the original text of 15,790 conditions and code their content along three dimensions: spending limits, tax increases, and pro-market conditions.

Chapter III assembles a novel data set on World Bank projects, combining data on geocoded World Bank projects from two sources: AidData Research and Evaluation Unit (2017) and a novel data set of World Bank projects from the World Bank's Application Programming Interface. In combination with month-specific project-level disbursement data extending previous data from Kersting and Kilby (2016), this gives us an unprecedented data set on the precise implementation of World Bank projects from 1995 to 2021 to estimate the effect of international development finance over space and time.

Identifying the Impact of International Development Finance

This dissertation uses quasi-experimental methods to address the challenges to identify causal effects prevalent in each chapter. The scarcity of credible counterfactual scenarios in the evaluation of policy decisions have led to the reliance on quasi-natural experiments to understand "the causes of things." As part of the "credibility revolution," economists have vastly extended the toolkit of credible research approaches to evaluate policies (Angrist and Pischke 2010). In particular, this thesis makes use of event study designs and

instrumental-variable approaches to identify causal effects.

Chapters I and ***III*** use an event-study design that identifies the short-term effects of international development finance events.[3] The identification strategy exploits the staggered rollout of Gallup World Poll survey waves over a time period of on average 26 days. The core idea of this approach is simple: it compares individuals interviewed by Gallup just before a specific event to individuals interviewed just after the event. Given that the rollout of the poll is plausibly exogenous to international development finance project events, and controlling for individual and survey characteristics, any difference between treatment and control group are likely to stem from the project-related event. We use this approach in chapter I to examine the effect of Chinese development project events on donor government perceptions and in chapter III to test the effects of international development finance project announcements made by the World Bank on migration preferences and outlooks on the future among recipient country populations.

In addition, all three chapters make use of different instrumental variable approaches.

Chapter I relies on a variation of a shift-share instrumental variable for Chinese development projects introduced in Bluhm et al. (2020). Relative to domestic demand, China overproduces certain construction inputs. Bluhm et al. show that these excess supplies of physical project inputs in China lead to increases in Chinese development projects abroad. To generate local variation, the instrument exploits that locations that receive Chinese development projects more frequently will be more impacted by changes in China's overall supply of project inputs. Following a growing body of shift-share instruments, the instrument thus interacts the measure for China's project inputs with a province's probability of hosting a completed Chinese development project, resulting in an instrumental variable that varies over space and time. With this instrument, chapter I examines the effect of international development finance on soft power.

Chapter II follows the so-called "judge-IV-approach." The approach uses differences in judge leniency to explain some of the variation in incarceration lengths. Following Beaudry and Willems (2022), we apply the judge fixed effects approach in the context of the IMF, where we test whether different preferences of IMF staff can explain variation in the conditionality of IMF programs. Just like judges are assigned to multiple cases over the course of their careers, IMF MCs are assigned to multiple countries over the course of their careers. We exploit this institutional setup and link MCs to the conditionality of their assigned IMF programs to estimate the impact of individual bureaucrats on the policy output of international organizations.

Chapter III uses a variant of an instrumental variable approach introduced by Kraay

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[3]I am extremely grateful to Bradley Parks, who hosted me at the research lab AidData at William & Mary in Williamsburg, VA, USA. We regularly met in the backyard of the local coffee shop "Aroma's" to discuss project progress over lunch. During one of these discussions, we came up with the idea of exploiting the within survey-wave variation of the poll and to collect specific event dates for Chinese development finance projects based on the source newspaper data.

(2012) for the implementation of World Bank projects. The instrument exploits the time lag between the commitment and the disbursement of World Bank projects. More precisely, it uses the *pre-determined* component of World Bank project disbursements to instrument *actual* World Bank disbursements. While *current* and *past* economic shocks in the recipient country are likely to be featured in these planned disbursement schedules, *future* shocks unknown at the time of project approval cannot be included. As loan disbursements follow the initial disbursement plans, fluctuations in pre-defined disbursements on projects approved in previous years are not correlated with contemporaneous macroeconomic shocks, which allows us to estimate causal effects of the implementation of World Bank-orchestrated international development finance on migration preferences in the developing world.

Conclusion

This dissertation analyzes how international development finance is implemented by various actors to achieve their respective strategic goals. In this dissertation, I aim to provide a holistic view on international development finance by documenting carefully how it is implemented and in which ways it shapes perceptions and livelihoods in recipient countries. Chapter I provides evidence that the Chinese government uses development finance to acquire soft power across the globe, particularly in countries that are strategically important. Chapter II shows that individuals matter for the implementation of international development finance and that international bureaucrats shape global governance according to their individual political ideologies. Finally, chapter III demonstrates that international development finance applied by an international institution alleviates migration pressures across individuals in the developing world in the short term, yet leads to higher incomes and subsequently more regular migration in the longer term.

There are several important takeaways from these findings. First, donor interests continue to play an important role in the provision of international development finance, in particular when provided bilaterally. Despite of the need to find coordinated responses to global crises and evidence of higher effectiveness of multilateral development finance, established and emerging donor governments alike continue to rely on development finance to pursue their strategic interests. While this is likely to reduce the efficiency of the instrument to achieve positive development outcomes, it can pay off for donor governments in other dimensions. Chapter I finds international development finance to be effective in promoting popular support for the Chinese government, an autocratic government of an emerging economy that seeks to gain influence at the international stage. In the past decade, China has multiplied its efforts in international development finance, raising concerns about economic and political dependencies among Western policymakers. With regard to soft power, this chapter provides evidence that these concerns are well-founded.

Second, individual ideas play a substantial role in the implementation of international

development finance. International organizations are often assumed to be controlled by powerful states where their interests are involved or, absent these interests, by a unitary and impartial international bureaucracy. The findings from chapter II challenge this perspective based on three interrelated arguments. First, in organizational activities that are of lesser interest for powerful member-states, the IO bureaucracy enjoys substantial agency. Second, despite supposed tight hierarchical structures and homogeneous worldviews of IO staff, substantial heterogeneity exists in how IO staff perform their duties. Finally, some of this heterogeneity can be traced down to the ideological biases to top staff, casting doubts on international organizations' claims of apolitical, technocratic governance. These findings raise important question on the limits of accountability within global governance.

Third, when coordinated by international institutions, international development finance can be successful in addressing the "root causes" of migration in the developing world. Political narratives of the effect of foreign aid on migration thus seem to be warranted to a certain extent. Yet, these effects are short term and conditional on the continued flow of international development finance. In the longer term, positive development effects increase migration capabilities and lead to higher regular migration into developed countries. When discussing the effectiveness of the instrument, it is important to consider the alternatives to the "root causes" strategy. Reducing migration through more restrictive immigration policies reduces welfare, increases irregular migration, and leads to higher death rates. Hence, donor governments should consider ways in which aid *and* migration can be combined effectively to improve the medium- and long-term development of origin countries such that root causes decrease over time. That should involve legal pathways for low- and medium-skilled migration, which can benefit labor markets in destination countries and, through the transfers of money, skills, and values by migrants back to their origin countries, reduce root causes of migration.

Finally, it is important to continue to study the policy instrument international development finance. The 21st century has confronted the global community with a set of cumulative crisis unprecedented in recent history. The dimension of such crises—climate change, but also other yet unknown crises—will if anything only increase in the future. Coordinated policy responses are and will continue to be needed, and the financial burden must be larger for those who can afford more. Hence, international development finance will continue to play a central role in the global struggle for development. Yet, given the limited and often scarce (financial) resources available, better and more efficient policy design is needed. For this, we need to evaluate and learn from past policies, and continue to study the implementation and impact of international development finance.

Chapter Summaries

Chapter I. *This chapter is co-authored with Axel Dreher, Andreas Fuchs, Bradley Parks, & Austin Strange.*

The pursuit of soft power is an important objective for powerful countries like the U.S. and China. As Goldsmith et al. (2014: 88) point out, "competition between major powers such as the United States and China for favorable perceptions in global public opinion is increasingly evident today and likely to be a pivotal feature of the emerging international order." Today, increasing geostrategic tension between China and the U.S. and Russia's 2022 invasion of Ukraine has further increased the demand for soft power—the "ability to achieve goals through attraction rather than coercion" (Nye 2004: p. x).

The pursuit of favorable public perceptions in recipient countries is a central motive for the provision of development finance. Indeed, "brand management" is one of the most important reasons why donor governments extend foreign aid bilaterally, rather than multilaterally. What is more, higher levels of soft power bear several potential benefits, including improvements in a country's geopolitical position through higher military support by foreign countries, and increased trade, and investment linkages (Guiso et al. 2009, Goldsmith and Horiuchi 2012, Rose 2016, 2019). Yet, there is no systematic evidence on whether and to what extent development finance affects overall levels of approval for donor governments in recipient countries.

In recent years, the Chinese government has become the world's largest bilateral source of international development finance, outspending the United States on a more than 2-to-1 basis. As emerging power, China increasingly aims at expanding its economic and political influence around the world. Its government understands that development finance is an important tool for achieving this. In 2014, Chinese President Xi Jinping acknowledged that the Belt and Road Initiative is part of a broader effort to "increase China's soft power, give a good Chinese narrative, and better communicate China's message to the world" (People's Daily 2014).

In this chapter, we test the effectiveness of Chinese international development finance as soft power instrument. We use data from almost 1 million individuals across 126 countries interviewed by the Gallup World Poll over the 2006-2017 period on attitudes toward China's government. Our approach follows earlier literature that measures soft power using recipient country public opinion toward governments (Nye 2004, Goldsmith and Horiuchi 2012, Rose 2016). We distinguish between short-term effects of specific project events and potential longer-term effects of aid projects.

In the short-term analysis, we examine the effect of project-specific events—project announcements, ground breakings, and project closures—on public approval of the Chinese government. For this, we created a new dataset including 3,998 commitment, start, and end dates of 2,214 Chinese development projects found in AidData's Global Chinese

Official Finance Dataset (Custer et al. 2021, Dreher et al. 2022). We match these events with Gallup World Poll survey waves to compare individuals interviewed just before a project event to those interviewed just after. Our findings show that individuals interviewed after project completion are around 3 percentage points more likely to approve of the Chinese government. This effect becomes stronger in the case of more concessional and larger projects.

We then aggregate China's overseas development projects to the province and country level to examine longer-term effects of project completion on Chinese government approval. Our instrumental-variable approach follows Bluhm et al. (2020) and Dreher et al. (2021b) and makes use of a supply shock—the yearly production volumes of physical construction materials produced in China—to proxy the over-time availability of Chinese projects. Chinese government-financed development projects are often tied to goods and services provided by Chinese companies, and as such they also heavily rely on surplus input materials produced in China. Therefore, larger production volumes of construction materials in China should increase the supply of overseas development projects. We interact this measure with the share of years over the sample period in which a region received a development project from China to proxy which regions are likely to receive larger or smaller shares of additional projects that result from these supply shocks.

Our results confirm the positive effect of project completion on popular support for the Chinese government. One additional Chinese development project increases public approval for the Chinese government in the recipient country by 0.2 percentage points. Our results are driven by higher incomes and satisfaction with public amenities in recipient countries. While the country-level increase in soft power does not translate in an overall increase in global perceptions of the Chinese government, China makes substantial soft power gains in important subsets of recipient countries. Specifically, we find that China's provision of development projects raised approval of the Chinese government among strategically important countries, such as countries on the African continent and swing states in the United Nations General Assembly.

Our results suggest that Chinese development projects can positively impact public approval of the Chinese government, particularly in strategically important countries. The US and partner countries are increasingly anxious about China's pursuit of global influence, including its efforts to win hearts and minds in the developing world through the provision of international development finance. Our findings document that these concerns are well-founded.

Chapter II. *This chapter is co-authored with Valentin Lang & Alexandros Kentikelenis.*
The influence of powerful states into the policy outputs of all major international or-
ganizations (IOs), including international development finance, is well-established (e.g.,
Dreher et al. 2009, 2022, Kilby 2009, Schneider and Tobin 2013, Carter and Stone 2015,
Kersting and Kilby 2016). But, in addition to the geopolitical dimension of this in-
fluence, there is also a technocratic one. IOs comprise of career staff who are highly
trained in their respective work areas and often enjoy a considerable degree of autonomy
in conducting the day-to-day business (Abbott and Snidal 1998, Barnett and Finnemore
1999, 2004). Bureaucratic power rests on the purportedly dispassionate, evenhanded, and
rules-based application of expert knowledge by staff who have shared beliefs due to their
broadly similar academic training and professional socialization (Chwieroth 2010, Nelson
2014). This allows IO bureaucracies to present themselves as well-oiled machines based
on hierarchical structures, technical knowledge, and institutional oversight that shields
them from the geopolitical interference of major shareholders and producing streamlined
and impartial policy output.

This chapter examines the implementation of international development finance and
argues that international bureaucracies are neither unitary actors nor politically neutral
in implementing IO policy. Rather, international institutions are made up of individuals
with ideological biases of their own, and these biases in turn manifest in organizational
output. In doing so, we contribute to the international-relations scholarship, scrutinize
the "micro-foundations" of intra-IO operations, and uncover the diverse ideological pref-
erences at the level of the individual that shape the policy output of IOs. We focus on
the IMF—arguably one of the world's most powerful IOs—to empirically study the im-
plications of ideological biases among IO bureaucrats. The IMF is notorious for its firm
hierarchical structures and tight control over the types of knowledge and expertise that
inform policy. Staff themselves are highly trained experts holding advanced economics
degrees from a handful of global elite universities (Chwieroth 2008, 2010, 2013, Nelson
2014, Chwieroth 2015, Nelson 2017). This makes the IMF a particularly hard case for
our argument.

To test the argument, we collect individual-level data on IMF mission chiefs (MCs).
Each MCs has the primary authority for designing the IMF's policy advice vis-à-vis the
assigned member-state, whether in the context of lending programs or when carrying out
periodic economic surveillance missions. Our final data set covers nearly the universe of
individuals holding such a post over the 1980-2016 period by following the career and
country deployment of 835 officials within the IMF over time and space. In addition,
we collect curricular information on these staff members, including their education, na-
tionality, and year of entry into the IMF. We combine these resources with data on the
policy conditions that the IMF attached to all lending programs of the same period. To
determine the ideological leaning of IMF conditionality, we coded 15,790 of these condi-

tions along the dimensions tax increase, spending limits, and pro-market, based on their original text available in official IMF documents.

Our empirical strategy builds on the "judge fixed effect" approach. Introduced by Kling (2006), this method uses the repetitive assignment of judges and variation in judge leniency to explain sentence lengths in court cases. We apply this approach to the IMF, exploiting the repetitive assignment of MCs to multiple member countries over the course of their career (Beaudry and Willems 2022). Interrogating the possibility that MC assignment could be endogenous such that the ideological leaning of mission chiefs could influence their placement, we find neither quantitative nor qualitative evidence for the strategic assignment of IMF MCs to specific member countries. Rather, IMF assignment seems to be largely determined by routine rotation procedures, seniority, availability, and managerial skills, as well as organizational diversity policies, rather than on a given staffers' perceived ideological preferences.

Using a "jackknife" (Angrist et al. 1999) logic, we construct an MC-specific bias measure based on all past and future country appointments of the individual MC, while *excluding* the MC's current country of responsibility. This ensures that the bias measure is independent of the circumstances under which the program is designed in the current member country. We then use the bias measure to explain IMF conditionality. We find that a one-standard-deviation increase in the bias of an MC translates into 6% or 2.2 additional conditions assigned to the program country.

Further, using the new data set on the ideology of IMF conditionality, we find that IMF programs led by MCs with pro-market views are more likely to demand market-liberalizing reforms and that MCs who have revealed preferences for fiscal adjustment via tax increases in other programs are more likely to demand tax increases. We also provide evidence that mission chiefs are more likely to demand cuts to public spending and advocate for market-liberalizing policies when they received their economics training at universities whose faculties have a reputation of strong faith in free markets and being sceptical of government intervention. Finally, and in line with the argument of conditional delegation, we find that the ideological leaning of IMF staff matters most in IMF programs that are less relevant for its major shareholders.

These results suggest that the ideological biases of IO staff add to the geopolitical biases that powerful governments introduce. More generally, they cast doubt on prominent images of powerful international organizations as cohesive and impartial bureaucracies, instead highlighting the role of individual staffers in shaping their policy output. Finally, our findings raise important question on the current limits of accountability within global governance.

Chapter III. *This chapter is co-authored with Andreas Fuchs, Tobias Heidland, & André Gröger.*

In response to surging migration from low- and middle-income countries (Hanson and McIntosh 2016), Western governments promote foreign aid also as a means to fight the "root causes" of irregular migration from the developing world. In contrast to this apparent political consensus regarding the use of foreign aid to curb migration, the scientific evidence about the effect of foreign aid on migration and development is incomplete and inconclusive (Qian 2015, Clemens and Postel 2018).

A long-standing theoretical and empirical debate discusses the effectiveness of foreign aid. Optimists argue that foreign aid provides a "big push" to help poorer countries overcome poverty traps and reach a path of sustained economic development (Sachs et al. 2004). Sceptics of foreign aid have argued that aid is flawed in several ways, preventing it from creating positive long-term effects, and generally doing more harm than good (Easterly 2003). While some positive effects are documented (Clemens et al. 2012, Galiani et al. 2017), there is ample evidence on negative side effects, for example on conflict escalation and deteriorating political institutions (Kersting and Kilby 2014, Nunn and Qian 2014, Bluhm et al. 2021). Entangled with the general debate on the effectiveness of foreign aid, whether or not aid reduces migration remains an open question. Even if aid increases incomes, the effect on migration is unclear, as higher incomes might as well lead to *more* migration in developing countries where migration is costly and credit constraints are binding (Clemens 2014, Angelucci 2015, Bazzi 2017).

This paper is the first to investigate the effects of foreign aid on migration and development in a causal manner, on the global level using individual-level data, and to carefully document systematically changes in migration aspirations, individual welfare outcomes, and realized migration flows in the short and longer term. We do so by means of the *aspiration-capability* framework (Sen 1999, Carling and Schewel 2018, La Ferrara 2019). The key idea is that individual migration aspirations can only translate into realized emigration if they are met with the respective capabilities. We follow this approach by investigating the effect of aid on both migration *preferences* and *flows* and document systematically *where* aid translates into changes in migration aspirations and capabilities and *how* it affects migration flows.

First, we document a novel aid effectiveness channel, with World Bank project announcements reducing migration preferences by improving individual expectations of the future. With the announcement, the World Bank board approves projects and the implementation of projects becomes certainty. Events are broadcasted in recipient country news and are—if perceived—likely to change individual outlooks of the future and thereby reduce migration preferences in recipient countries. To test this, we exploit the staggered rollout of the GWP and match these survey windows with World Bank project announcements to compare individuals interviewed just before to those interviewed just after an

announcement. The results show that the mere announcement of an aid project reduces migration preferences, especially among well-informed and young individuals, and that the effect coincides with a significant improvement in perceptions of the future.

The second part of the empirical analysis examines the implementation of aid, and the longer-term effects. To this end, we exploit geolocated project-level disbursement schedules over time to measure the amount of aid a province receives over time. To tackle potential endogeneity concerns, we follow Kraay (2012 and 2014) and Andersen et al. (2022) and construct an instrumental variable for disbursement flows of World Bank projects. While project design at approval is likely to be subject to *current* and *past* economic (and migration) shocks in the recipient country, they are unlikely to be correlated with *future* (migration) shocks unknown at the time of project approval. To the extent that loan disbursements follow precisely pre-determined disbursement plans, fluctuations in current disbursements in a given year stem from aid decisions made in previous years.

Consistent with the short-term announcement effects, we find that larger aid disbursements lead to lower migration preferences in aid recipient provinces in the short term. This effect is again more pronounced for younger and highly skilled individuals, driven mainly by low-income countries and aid projects targeting the production sector, associated with an improved perception of government institutions. This short-term effect also translates into a reduction in the number of asylum-seekers into OECD countries. In the longer term, these short-term effects vanish, and as local livelihoods improve through reductions in poverty and higher incomes, we observe an increase in migration flows from aid recipient countries.

Foreign aid can be successful in addressing the "root causes" of migration in the developing world. Political narratives of the effect of foreign aid on migration thus seem to be warranted to a certain extent. Yet, these effects are short term and conditional on the continued flow of foreign aid. In the longer term, positive development effects increase migration capabilities and lead to higher regular migration into the developed world. With regard to the effectiveness of the instrument, it is important to consider the alternatives to the "root causes" strategy. Reducing migration through more restrictive immigration policies reduces welfare, increases irregular migration and leads to higher death rates. Hence, donor governments should consider ways in which aid *and* migration can be combined effectively to improve the medium- and long-term development of origin countries such that root causes vanish over time. That should involve legal pathways, for low- and medium-skilled migration, which can benefit labor markets in destination countries and, through the transfers of money, skills, and values by migrants back to their origin countries, reduce the root causes of migration.

This chapter is co-authored with Axel Dreher,
Andreas Fuchs, Bradley Parks, & Austin Strange.

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Can Aid Buy Foreign Public Support?

1.1 Introduction

A large literature examines the effects of foreign aid.[1] Most of this research focuses on whether or not aid affects tangible outcomes in recipient countries, such as economic growth, health, education, and corruption.[2] These outcomes are certainly important to those living in developing countries and to development finance institutions. However, recipient country welfare is only one objective that motivates bilateral donors to provide foreign aid (Thiele et al. 2007).[3] Donor countries also use bilateral aid to pursue their own geostrategic goals, such as buying votes in international organizations, supporting friendly governments before elections, securing market access for exporters, deterring asylum seekers, and fighting terrorism (e.g., Kuziemko and Werker 2006, Fleck and Kilby 2010, Faye and Niehaus 2012, Dippel 2015, Rommel and Schaudt 2020). Another key motivation for bilateral aid is the acquisition of soft power—e.g., to influence international public opinion about the donor government.[4]

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[1]For ease of exposition, we will use the term "aid" in this paper to refer broadly to any types of official sector financial flows from a donor (or lender) to a recipient (or borrower). In cases when we wish to reference the narrower (OECD-DAC) definition of aid, we use the term Official Development Assistance (ODA). In cases when we wish to reference concessional and non-concessional official financing that does not qualify as ODA, we use the term Other Official Flows (OOF).

[2]See Werker et al. (2009), Dreher et al. (2018), and Doucouliagos (2019) for literature surveys.

[3]Also, this objective is probably better addressed via multilateral institutions (Milner and Tingley 2010).

[4]Soft power is "the ability to achieve goals through attraction rather than coercion" (Nye 2004: p. x). Public opinion is a commonly used proxy for soft power (e.g., Nye 2004, Goldsmith and Horiuchi 2012,

To this end, donor governments spend a considerable amount of time and money disseminating positive messages about their generosity to members of the public in developing countries. They attach their logos to aid shipments. They place signage at project sites to inform the public of their activities. They organize public ceremonies to mark the start of new projects and the completion of existing ones. Some broadcast their own messages through social media channels and cultivate journalists to encourage media coverage of their accomplishments. Others are more aggressive, forging content-sharing partnerships with radio stations, television channels, and newspapers or building telecommunication systems that make it easier to transmit information to the general public. In short, aid is used to shape perceptions on the ground in developing countries, and 'brand management' is one of the most important reasons why donor governments extend foreign aid bilaterally rather than multilaterally.[5]

Economists and political scientists have estimated the impacts of development finance on vote buying, migrant deterrence, the fight against terror, and public opinion in recipient provinces or countries (e.g., Bandyopadhyay et al. 2014, Lanati and Thiele 2018b, Dreher et al. 2019, Eichenauer et al. 2021). Yet, no study has comprehensively tested whether and to what extent development finance affects overall levels of approval for donor governments. This is a surprising omission since soft power is an important first-order outcome for a number of other strategic goals. Goldsmith and Horiuchi (2012) suggest that foreign public opinion affects military support by foreign countries. Guiso et al. (2009) and Rose (2016, 2019) show that soft power and higher levels of trust between countries also bring material economic gains, such as higher exports for countries with greater global influence. Disdier and Mayer (2007) find stronger trade ties between countries whose populations have higher levels of affinity for each other. What is more, to the extent that soft power affects economic outcomes in the donor country (via increased trade, for example), it may increase support for aid giving in the donor country and as such also lead to higher volumes of future foreign aid flows to recipient countries.

Whether and how aid improves foreign public perceptions of governments is of growing importance for many of the largest bilateral donors. As Goldsmith et al. (2014: 88) point out, "[c]ompetition between major powers such as the United States (U.S.) and China for favorable perceptions in global public opinion is increasingly evident today and likely to be a pivotal feature of the emerging international order." However, whether aid expands or erodes support for donor governments abroad remains an open question.

Instead of bolstering support for donor governments, development projects could easily become reputational liabilities if they are not carefully designed and implemented. Projects that involve large-scale construction activities often create noise, traffic, and

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Rose 2016).

[5]They do so in spite of well-documented concerns related to aid proliferation and fragmentation (Knack and Rahman 2007, Gehring et al. 2017).

pollution. They can lead to labor strikes, public protests, lawsuits, and allegations of political favoritism and corruption.[6] Additionally, development projects can backfire—from a 'brand management' perspective—if they fail to reach completion or experience major cost overruns that are borne by local and national governments.[7]

Potential public opinion effects are not limited to the localities where aid projects occur. Aid branding and publicity can affect attitudes in farther-flung places too, and attitudinal effects in some places might offset effects in others. Gauging the effects of foreign aid projects on a donor's soft power therefore requires an estimate of aggregate effects in addition to partial, localized estimates that are specific to project sites.

We investigate the effects of development projects on a donor government's popular support across three different target audiences: (i) people living in the province(s) where a project takes place, (ii) people living in other areas of the country with less direct exposure to the project, and, (iii) international audiences without direct exposure to projects. These three levels of analysis allow us to distinguish between direct and indirect public opinion effects. Provinces that host development projects have the highest levels of 'treatment exposure,' and the people living within these jurisdictions are most directly affected by positive and negative project outcomes. Those who live in close proximity to development projects will be more likely to make judgments about the donor government based on their own firsthand experiences and observations—or those of the people whom they know. However, indirect public opinion effects can also occur when people inside or outside the province where the project is located learn about it via television, radio, print media, online media, word of mouth, or travel. Such effects are, of course, not restricted to residents of the host province or even country, and different audiences around the world can react differently to information about development projects. The construction of Hambantota Port in Sri Lanka is a case in point. This project, which was financed by China Eximbank, has been cited in thousands of media reports in virtually every corner of the globe as evidence that the Chinese government is engaging in 'debt-trap diplomacy'

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[6]For example, during the middle of winter in 2018, a China Eximbank-financed thermal power plant in the Kyrgyz Republic failed, and local residents were left with no heating. When civil society organizations followed the paper trail, evidence of embezzlement emerged. This resulted in the dismissal of Prime Minister Sapar Isakov. 30 government officials were charged with corruption and using their positions to lobby for the selection of a Chinese company (TBEA) as the contractor for the project. Prosecutors estimate that bid-rigging and the inflated cost of the sole-source contract issued to TBEA cost the Government of the Kyrgyz Republic as much as US$ 111 million (Malik et al. 2021).

[7]The Astana Light Rail Construction Project is a case in point. China Development Bank issued a US$ 1.5 billion loan to Astana LRT LLP—a project company that is owned by the City of Astana—for this project and the Government of Kazakhstan provided a sovereign guarantee in support of the loan. However, in October 2019, the President of Kazakhstan ordered an investigation into the officials who initiated the project. The chief executive of Astana LRT LLP was accused of embezzling project funds and fled the country. The local authorities suspended the construction of the railway and the half-finished project became a source of public discontent (Malik et al. 2021). The four-meter-high concrete trestles upon which the railway was supposed to run are now referred to by local residents of Astana as "monuments of corruption."

(e.g., Brautigam 2020).[8] Nor do development projects necessarily produce consistent public opinion effects across these different audiences. For example, a development project that is considered to be useful and appropriate by residents of the recipient country might elicit a very different response from observers in a rival country to the donor or recipient, who may view the donor's or recipient's gains as coming at their expense.

We focus our empirical analysis on Chinese government-financed development projects. Beijing's overseas development program is a useful application for several reasons: China has become the world's largest bilateral source of international development finance. It now outspends the United States on a more than 2-to-1 basis.[9] Like other major powers, China is increasingly seeking to expand its economic and political influence around the world. Development finance is an important tool that the Chinese government uses to burnish its popular image in the Global South (Kurlantzick 2007, Hanauer and Morris 2014a,b, Fuchs and Rudyak 2019). In 2014, Chinese President Xi Jinping acknowledged that the Belt and Road Initiative (BRI) is part of a broader effort to "increase China's soft power, give a good Chinese narrative, and better communicate China's message to the world" (People's Daily 2014). Echoing this point, a senior Chinese government official announced that "the work of foreign aid relates to China's image. We cannot tolerate any negligence or projects of poor quality" (MOFCOM 2014). The Chinese government is also attractive from an inferential leverage perspective because it is the only bilateral donor for which all development projects have been subnationally geocoded for a substantial number of years across all major world regions. These data—in conjunction with public opinion data that have broad spatio-temporal coverage—provide a strong empirical foundation for the identification of potential causal effects.

We measure the causal effects of Chinese development projects in the short and longer run. First, we use time-stamped, respondent-level data for more than 1.5 million people interviewed by the Gallup World Poll across 126 countries between 2006–2017 in an event study to analyze the short-term effects. This approach exploits the staggered roll-out of the poll and the availability of precise interview dates. The precise dates allow us to compare respondents who were interviewed within 30 days before versus 30 days after the occurrence of a Chinese project event. We create a new database coding such events that

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[8]Another prominent example is the China Eximbank-financed Entebbe Airport Upgrading and Expansion Project in Uganda, which became a major source of international controversy when various media outlets reported (incorrectly) that the airport was a source of collateral the lender could seize in the event of default. Beijing tried to put the issue to rest by issuing the following public statement: "Not a single project in Africa has ever been confiscated by China because of failing to pay Chinese loans." But Beijing was lampooned by Trevor Noah—the host of a satirical television news program called *The Daily Show*—for the careful wording of its statement. In a video clip that has now been viewed nearly 4 million times on YouTube, Noah said: "I don't know, maybe it's just me, but that statement was not the most reassuring thing I've ever heard because 'We've never confiscated an airport' is very different from 'We're never going to confiscate an airport'" (Parks et al. 2022).

[9]Whereas average annual development finance commitments from China amounted to US$ 85.4 billion between 2013 and 2017, average annual development finance commitments from the U.S. amounted to US$ 37 billion during the same five-year period (Malik et al. 2021, Dreher et al. 2022).

includes 3,998 commitment, start, and end dates of 2,214 Chinese development projects in 126 countries and 2,025 first-order subnational administrative (ADM1) regions around the world.[10] Controlling for province-year fixed effects, as well as a range of individual and survey characteristics, the timing of an interview can be considered random relative to a Chinese project event. This research design enables us to rigorously analyze the immediate effects of development projects on public attitudes.

While the event study enables estimation of public opinion effects at the discontinuity and thus facilitates the identification of causal effects, it comes at the cost of neglecting a large share of available data. This is because it relies only on projects with event dates that occur during Gallup survey windows. What is more, these estimates relate to the specific timing of project events and are thus short term in nature. This approach does not capture the potential longer-term attitudinal effects of Chinese government-financed projects.

The second component of our empirical strategy uses an instrumental-variables approach to test longer term effects and to more comprehensively assess the public opinion effects of China's overseas development projects. We again make use of georeferenced and temporally disaggregated Chinese development project data and subnational public opinion data from the Gallup World Poll. We not only estimate the effects of Chinese development projects on public approval of Beijing within recipient provinces and countries, but also across the Global South as a whole.

Our instrumental-variables strategy follows Bluhm et al. (2020) and Dreher et al. (2021b) and makes use of a supply shock—the yearly production volumes of physical construction materials produced in China—to proxy the over-time availability of Chinese projects. China overproduces materials, such as steel, relative to its domestic demand (Dreher et al. 2021a). Chinese government-financed development projects are often tied to goods and services provided by Chinese companies and, as such, they also heavily rely on input materials produced in China. Therefore, larger production volumes of construction materials in China should increase the supply of overseas development projects. Bluhm et al. (2020) and Dreher et al. (2021b) use the share of years over the sample period in which a region received a development project from China to proxy which regions are likely to receive larger or smaller shares of additional projects that result from these supply shocks. The instrumental variable is the interaction of the supply-shock measure with this probability of receiving aid.

This identification strategy is based on an intuition similar to that of a difference-in-differences design. We investigate a differential effect of Chinese project input surpluses on public opinion in provinces with different exposure levels to Chinese development projects.

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[10]ADM1 regions are one layer below the national level and correspond, for example, to provinces, states, oblasts, governorates, and emirates, depending on the administrative divisions in place in a given country.

The identifying assumption is that, apart from the direct effect of Chinese development projects on public opinion, public opinion in provinces with differing probabilities of receiving development finance from the Chinese government will not be differentially affected by changes in China's production of physical project inputs, after controlling for province- and country-year-fixed effects and the other variables in the model. Below we provide tests of several underlying assumptions needed to make this approach plausible.

Our results show that the completion of Chinese development projects increases popular support for the Chinese government in recipient countries. This finding is consistent across the event study that captures effects of exposure to Chinese development projects within narrow, 30-day windows at the discontinuity, as well as the annual, macro-level analysis that includes a larger sample of projects. On average, we estimate that the completion of one additional Chinese development project increases public approval for the Chinese government at the recipient country level by more than 3 percentage points in the short run and 0.2 percentage points in the longer run.

We test the mechanisms behind the longer-term findings by looking at the potentially positive and negative side effects of Chinese development projects. The results suggest that Chinese development projects tend to foster development in recipient countries through higher reported and perceived incomes, improvements in living standards, and higher levels of satisfaction with public goods provision.[11]

Beyond these country-level impacts, a donor country's ability to amass soft power depends on *global* perceptions of its development projects. Analyzing reactions across all developing countries included in our sample, we find that China's provision of development projects increases public support for the Chinese government among countries in Africa, potential "swing states" in the United Nations General Assembly, and countries with higher baseline (ex ante) levels of public support for the Chinese government. All of these countries are arguably "high-value" targets for Beijing. We also find that Chinese development finance increases support for the Chinese government in third countries (i.e., neither the donor country nor recipient country) that are politically aligned with the respective recipient country. These indirect gains make development finance an especially attractive instrument for the accumulation of soft power. On the African continent, for example, we estimate that Beijing's global project portfolio increases public approval of the Chinese government by more than 2.2 percentage points per year, on average. Finally, our results show that Chinese development finance boosts public approval ratings for the governing authorities in recipient countries, which, from a political perspective, is consistent with the "win-win cooperation" principle that is commonly used by the Chinese

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[11]China's public opinion gains are less rather than more pronounced among people who live in close proximity to completed Chinese development projects. Relative to the country level, more citizens in project provinces report a deterioration of their living standards and a drop in their perceived incomes after the completion of Chinese development projects. This may be the result of lower-than-expected project quality, which is likely experienced to a greater degree among the residents of project provinces.

government to describe its foreign aid program.

Our findings contribute to the aid effectiveness literature in general and the literature on aid and public opinion in particular (e.g., Milner and Tingley 2013, Dietrich et al. 2018). They also add to a growing strand of research that measures the intended and unintended effects of Chinese development finance. Recent work has shown that Chinese development projects increase economic growth and reduce the spatial concentration of economic activity, but they also fuel local corruption, stoke ethnic tensions, weaken trade union participation, instigate public protests, and degrade the natural environment (Isaksson and Kotsadam 2018a,b, Bluhm et al. 2020, Isaksson 2020, Dreher et al. 2021a, Dreher et al. 2021b, Iacoella et al. 2021, Marchesi et al. 2021, Baehr et al. 2022, Gehring et al. 2022).[12] Likewise, recipient governments seem to benefit from these projects as they can steer funds to politically consequential jurisdictions in order to advance their electoral interests (Dreher et al. 2019, Anaxagorou et al. 2020).

Our work is most directly related to a growing set of studies that investigate the effects of aid on China's image within recipient countries. These studies typically analyze local public opinion effects around project sites—with mixed results. Using geocoded data on Chinese development projects and Afrobarometer survey data, Blair et al. (2022) leverage a spatial difference-in-differences strategy to estimate the public opinion impacts of local exposure to Chinese development finance in 38 African countries. They find that Chinese development projects are disproportionately announced in places that hold positive views of China, but individuals who live near *completed* projects report less favorable views of China and more favorable views of the United States and other Western powers. In contrast, a recent study finds that the general public in Africa mostly provides positive evaluations of China's foreign aid, particularly within host countries which are more developed and democratic (Han and Huang 2016). Similarly, a separate study using Afrobarometer data decomposes African perceptions toward Chinese trade, investment, and aid, and finds that the latter two are often viewed more positively by African citizens (Morgan 2019). Finally, Chen and Han (2022) find that individuals more supportive of incumbent political parties are most supportive of aid from China as well.

Our paper is unique in several ways. We study public opinion gains and losses that accrue locally as well as in other areas of recipient countries and in other countries. As such, we offer a comprehensive assessment of the effects of aid on foreign perceptions of donors. In addition, previous work also focuses almost exclusively on attitudes toward Chinese projects in African countries. One study finds no evidence at national and subnational scales that China's trade, aid, and investment in 18 Latin American countries affect public attitudes toward Beijing, on average (Eichenauer et al. 2021).[13] But

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[12] Dreher et al. (2022) provide an overview of this literature.

[13] They do, however, find evidence of a polarization effect, with 'treated' individuals being more likely than 'untreated' individuals to express very positive or very negative views of China—consistent with the view that Chinese development projects create winners and losers.

China's development finance is a global phenomenon and Asia is the largest recipient of Chinese development finance (Dreher et al. 2022). Our study analyzes a substantially larger sample of development projects that covers all developing regions, with an identification strategy that enables estimation of causal effects in regressions at the level of individuals, provinces, and countries.[14] As such, we can analyze the effect of development finance on soft power at various levels. We use individual-level data to estimate short-term effects of aid events on donor approval. Then, we use province- and country-level aggregates to examine the longer-term effect of aid on Chinese government approval in project provinces, recipient countries, and at the global level. Relying on the staggered roll-out of the Gallup World Poll at the micro level and an instrumental variable for aggregate-level analyses allows us to report causal effects rather than just conditional correlations. In addition, the analysis on the global level also takes into account that China might value popular support in some developing countries more than in others. Ultimately, this nuanced view of the overall level of soft power gains is needed when it comes to evaluating the effectiveness of aid as a tool to enhance global soft power. This makes our study the first comprehensive analysis of the soft-power effects of foreign aid via public opinion.

We proceed as follows. Section 1.2 introduces our new data on Chinese project events and presents the survey data on the approval of China's government. In Section 1.3, we analyze short-run effects with the individual-level event study. In Section 1.4, we proceed with the analysis of longer-run effects at the province and country level that relies on the instrumental-variables strategy. Section 1.5 studies effects at the global level. Section 1.6 analyzes whether Chinese development finance is "win-win" cooperation in the sense that it also raises citizens' approval of the national government of recipient countries. Section 1.7 concludes.

1.2   Data

1.2.1   New Data on the Timing of Chinese Development Projects

Analyzing the effects of China's development projects on public approval of the Chinese government requires comprehensive project-level information with high spatial and tem-

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[14]Jones (2018), Xu and Zhang (2020), and Blair et al. (2022) leverage variation in the timing of project implementation, comparing individuals interviewed in locations before the project implementation phase to those interviewed in locations where projects started the implementation phase. To the extent that pledges and commitments create expectations about outcomes that implemented projects do not meet, this approach biases the estimated coefficients downwards because it cannot disentangle the effects of announcement and delivery. What is more, the assumption that the timing when projects are committed relative to when they start being implemented is random relies on the absence of time-varying effects on projects and opinion, which might or might not hold true. Eichenauer et al. (2021) use a plausibly exogenous instrument at the country level, but do not report causal estimates at finer scales.

poral precision. Such data are unavailable via official channels as the Chinese government does not release project-level information of this nature. The Ministry of Commerce—China's lead institution responsible for foreign aid during the past several decades—ranks last among the 47 international donors evaluated in the 2020 Aid Transparency Index (Publish What You Fund 2020). China's State Council publishes official white papers on foreign aid, but the information in these publications is largely limited to aggregate statistics by world regions and decades (State Council 2011, 2014, 2021).[15] In response to this lack of official data, several open-source research initiatives have created project-level datasets that combine and refine information contained across government documents, media reports, and registrars maintained by recipient governments and international organizations (e.g., Strange et al. 2017a, Ray et al. 2021). AidData's Global Chinese Official Finance Dataset, created using the Tracking Underreported Financial Flows (TUFF) methodology, is to date the most comprehensive data-gathering effort as it covers the 'known universe' of China's development projects in developing countries (Dreher et al. 2021b, Dreher et al. 2022).[16] In addition to dozens of other variables, it provides detailed project descriptions, classifications according to sectors and flow types, and—in 56% of the project cases—information on monetary commitment amounts.

We draw on the 1.1.1 version of this dataset, in which Bluhm et al. (2020) geocoded all implemented and completed projects included in AidData's Global Chinese Official Finance Dataset, in order to estimate localized effects using precise geographic information on development projects.[17] Bluhm et al. assigned longitude and latitude to each project site where possible. In sum, the data contain 3,485 Chinese development projects worth US$ 273.6 billion (in terms of committed finance) implemented in 6,190 project locations across 134 countries. These locational data allow us to attribute 2,183 projects to the level of first subnational administrative (ADM1) regions, which typically correspond to provinces or states, and are the most fine-grained level of analysis that we require for our study design.[18] 1,519 of these projects can be classified as Official Development Assistance (ODA) according to OECD definitions.[19] The remaining 664 projects are categorized as

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[15]Fuchs et al. (2022) introduce a method to track Chinese aid exports with official customs trade data.

[16]This dataset is a widely used data source in analyses that examine the nature, allocation, and effects of Chinese development finance. Contributions in economics and political science include Hsiang and Sekar (2016), Hernandez (2017), Isaksson and Kotsadam (2018a,b), Anaxagorou et al. (2020), Isaksson (2020), Martorano et al. (2020), Eichenauer et al. (2021), Horn et al. (2021), Iacoella et al. (2021), Zeitz (2021), Gehring et al. (2022), Cervellati et al. (2022), and Watkins (2022), among others.

[17]In September 2021, AidData presented an updated version of this dataset, which also covers the years 2015–2017 (Custer et al. 2021, Dreher et al. 2022). However, the lack of precise geographic information prevents us from including these data in our analysis.

[18]We thus use project locations with precision codes 1–4. Precision code 1 corresponds to an exact location; precision code 2 corresponds to locations within 25 kilometers of the exact project site; precision code 3 corresponds to a second-order administrative (ADM2) region; and precision code 4 corresponds to an ADM1 region.

[19]The dataset codes all Chinese government-financed projects as ODA if they are financed by Chinese government institutions, have development intent, and a minimum level of concessionality with a 25-

Other Official Flows (OOF), meaning they lack concessionality or development intent and are thus more commercially oriented.[20] The world map in Figure 1.1 visualizes the number of projects completed over the sample period at the province level.

These data enable researchers to analyze Chinese government-financed development projects with a level of spatial precision that is not possible for projects financed by any other bilateral donor over such a long period of time.[21] However, AidData's Geocoded Global Chinese Official Finance Dataset lacks the precise temporal data needed to carefully analyze whether Chinese projects affect public opinion. While all of the project records include information on the commitment year, information on the exact start and end dates are missing for about three quarters of the projects, and no calendar day-level information is available on commitment dates. As such, existing studies have relied on annual-level tests of project allocation and effects. This is a particularly consequential limitation for studies focused on the effects of development projects on outcomes such as public opinion, since such outcomes can be confounded by a host of factors. Causal inference is easier when it is possible to identify narrow windows of time during which other factors should not vary much if at all. What is more, in order to study the effects of completed projects, previous studies had assumed average duration times from commitment to completion (e.g., Bluhm et al. 2020, Dreher et al. 2021b) or only used a small fraction (approximately 25%) of the projects with known start and end dates (Isaksson and Kotsadam 2018a,b, Isaksson 2020). While the former approach assumes no systematic variation in the speed of implementation across different types of projects, the latter assumes that those projects with known start and end dates are representative of the larger sample of Chinese government-financed development projects. Neither of these assumptions are likely to hold true.

To address this limitation, we revisited the initial data collection process implemented by AidData (Dreher et al. 2021b, Dreher et al. 2022). Our objective was to identify precise commitment, start, and end dates. Commitment dates represent grant and loan agreement signings or official project announcements. Start dates are typically marked by foundation stone-laying and other forms of groundbreaking ceremonies. End dates typically mark the end of construction, the point at which new infrastructure can be used, and/or the date on which a project passed final inspection. The TUFF methodology that was implemented to create the original dataset is based on systematic screening of vari-

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percent grant element or larger.

[20]We include projects that are classified as "Vague Official Finance" in this category. These project records contain insufficient information about concessionality and/or development intent to classify whether they are ODA or OOF.

[21]Other global, geocoded project-level databases for bilateral donors we are aware of cover Indian development finance for a period of eight years (Asmus et al. 2022) and French development cooperation for seven years (AFD 2021). Coverage of the variable capturing attitudes towards these donor governments is with 126,927 and 273,601 observations substantially lower than for the Chinese government (580,484, excluding "don't know" and "refused" answers). For the United States and Japan geocoded data are also available for the subset of humanitarian aid, but not for overall flows (Bommer et al. 2022).

ous official and unofficial sources that are publicly accessible and contain information on individual Chinese development projects (Strange et al. 2017b). For each project record, we reviewed these underlying sources to double-check existing dates and fill in missing information. In addition, a team of research assistants performed additional English- and Chinese-language internet searches that targeted individual projects with missing information on commitment, start, and end dates. This was done using Chinese project titles and the China-based search engine Baidu. To do so, we first translated existing project names into the Chinese language, and then searched for the recipient country in combination with a basic project description, name, and year. If the results of this searching were insufficient to find exact project events, we used a more fine-grained approach based on project-specific keywords or the names of the actors potentially involved in the project, such as the respective Chinese ambassador or the relevant construction contractor. Appendix A provides the full codebook that we used to create these data.

This data-gathering effort significantly improved the coverage and quality of project event dates: We uncovered a total of 3,998 dates. Our study is the first to provide exact commitment dates for Chinese development projects and substantially increased the coverage of start and end dates compared to previous work (see Figure 1.B.1 in Appendix B for a comparison). Of the 3,485 projects included in Dreher et al. (2021b) and Dreher et al. (2022), 429 now contain a commitment date (12%), 1,771 a start date (51%), and 1,798 an end date (52%).[22]

These project event dates allow us to estimate short-term effects of Chinese development projects on public approval of the Chinese government. They also enable us to improve temporal precision in estimates of the more longer-term effects as we can rely on exact project end dates rather than rough estimates based on their commitment year. These data can also benefit future research by enabling researchers to carry out subannual analyses of Chinese development finance. Indeed, AidData's ongoing efforts to track China's global development finance now make use of similar project event coding protocol we developed for this study.

Our event study utilizes these newly collected data by focusing on individuals interviewed just before and after a project-related event. We rely on binary variables indicating these events. In our study of longer-term effects, we rely on the completion date variable exclusively and use the number of Chinese development projects completed in a year as our main variable of interest. We prefer this variable over the (logged) monetary amount of development funding (in US$) to a particular subnational region as 44% of

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[22]Of the 2,183 geocoded projects, 286 projects now contain information on commitment dates (13%), 1,123 projects on start dates (51%), and 1,136 projects on end dates (52%). Commitment dates were considerably more difficult to identify using our open-source search method. This was in part because online sources appeared less likely to explicitly publish such dates, whereas start and end dates were more common. Similarly, sources did not refer to commitment dates in uniform ways or language (relative to start and end dates, which were more clearly identifiable in these sources).

Figure 1.1 – Chinese development projects completed at the province level, 2006–2017
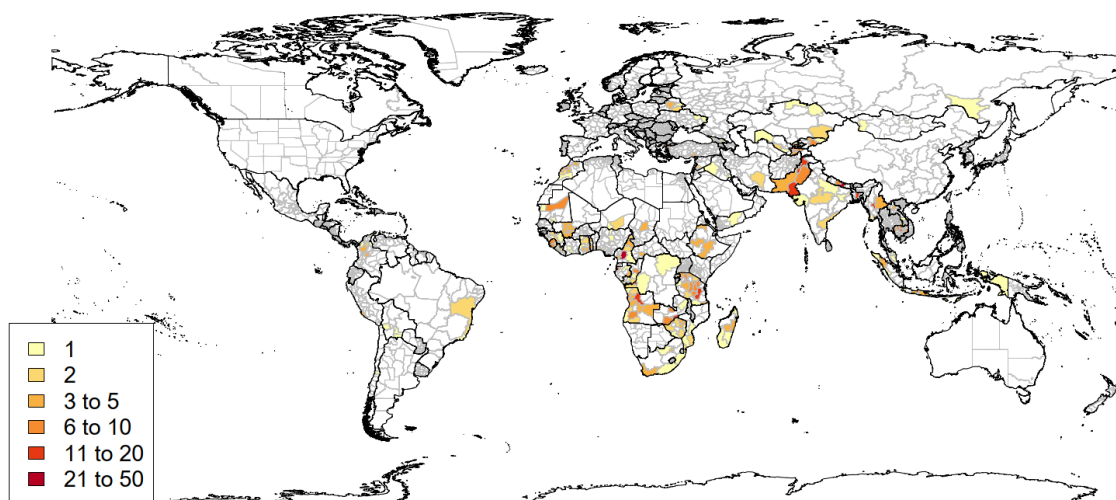
Notes: This figure shows the number of China's development projects by province (ADM1 region) that contain information on their completion date. Source: Bluhm et al. (2020), Dreher et al. (2022), and Authors' data.

the projects lack information on the monetary values (see Dreher et al. 2021b). The use of project counts circumvents this problem; however, we repeat the analysis using monetary amounts for comparison. In addition, we make use of monetary amounts to investigate the effects of large projects (which we define as those of US$ 1 million or above) in separate regressions.

1.2.2 Public Approval of the Chinese Government

We pair our fine-grained Chinese project information with respondent-level data on public opinion from the Gallup World Poll (GWP, Gallup 2018). To the best of our knowledge, the GWP is the most systematic collection of worldwide public opinion data. It covers worldwide annual data since 2006 and includes repeated cross-sectional data for more than 1.5 million individuals—on average 115,000 individuals per interview-year.[23] Each country-wave of the GWP is tethered to a specific period of time when the survey was completed. Individuals are tagged with location variables, which we matched to ADM1 regions.[24] This allows us to examine respondent-level data for a maximum of 126 countries

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[23]Figure 1.B.2 in Appendix B shows the number of individuals included per year, as well as the months and days of interview.

[24]GWP provides within-country geographic variables indicating the subnational region the respondent lives in (named *REGION_xxx*). We matched this variable with ADM1 shapefile names from the Database of Global Administrative Area (GADM). We use GADM version 2.8 to map the administrative areas of all countries. We successfully mapped provinces from 126 out of the total 140 developing countries in the GWP. To be precise, we include all GWP countries apart from those that belong to the OECD's Development Assistance Committee, which is the group of established donor countries. From these countries, we mapped 2,025 out of a total of 2,280 provinces. If countries or provinces could not be mapped, this was either because the GWP used a spatial identifier that was above the unit of ADM1

and 2,025 ADM1 regions.

GWP data are probability-based and nationally representative of the resident population of 15 years and older—with only a few exceptions due to staff safety concerns and scarcely populated or poorly accessible areas. Questions are standardized around the world for all respondents and asked in the respective national language. GWP interviewers conduct surveys by telephone where telephone coverage exceeds 80 percent using random digit dialing or nationally representative telephone number lists. In all other regions, they conduct face-to-face interviews based on random routes procedures at different times of the day.[25] A typical survey collects data from 1,000 individuals, varying with country population size.[26] GWP data are widely used in economic research (Deaton 2008, Bjørnskov 2010, Kahneman and Deaton 2010, Stevenson and Wolfers 2013, Bertoli and Ruyssen 2018, Deaton 2018, Guriev et al. 2022).

While GWP contains several questions that potentially relate to China's soft power, only the following question clearly relates to popular attitudes toward China's government and has significant coverage (with 890,000 recorded responses) over time and space: "Do you approve or disapprove of the job performance of the leadership of China?"[27] For our empirical analysis below, we recode this variable as a binary indicator that takes the value of one if the respondent approves of China's leadership.[28] 63% of the respondents approve of China's leadership. For comparison, the fraction of respondents who approve of their national government is 54%. The world map in Figure 1.2 visualizes the mean approval rates for the Chinese government over the sample period by ADM1 region. The country with the highest average approval rate is Mali (91%), followed by the Central African Republic (91%) and Guinea (87%). Kosovo (12%), Puerto Rico (16%), and Croatia (25%) have the lowest average approval rates. As can be seen in Figure 1.3, approval rates peaked in 2006 and bottomed out in 2013, but also show considerable differences across world regions. Overall, average approval rates across countries show a decreasing trend over time in our study period.[29]

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regions (but below the country level), or because names of GADM28 units and the spatial identifier did not match. See Table 3.A.1 for the full list of countries included in our analysis.

[25]Face-to-face interviews usually take about one hour; telephone interviews take 30 minutes.

[26]For more information on the GWP Survey Method, see https://news.gallup.com/poll/105226/world-poll-methodology.aspx.

[27]The question with the second-best coverage is the following: "In general, what opinion do you have of the following nations? China." However, the coverage of this question is poor with less than 54,000 observations. The correlation between those individuals expressing a positive opinion on the job performance of the Chinese government (our baseline question) and those expressing a positive opinion on China as a nation is 0.36. We use the latter below in robustness tests.

[28]We code responses indicating "Don't know" as missing.

[29]We give all countries equal weight when calculating the average.

Figure 1.2 – Average support of the Chinese government by province, 2006–2017
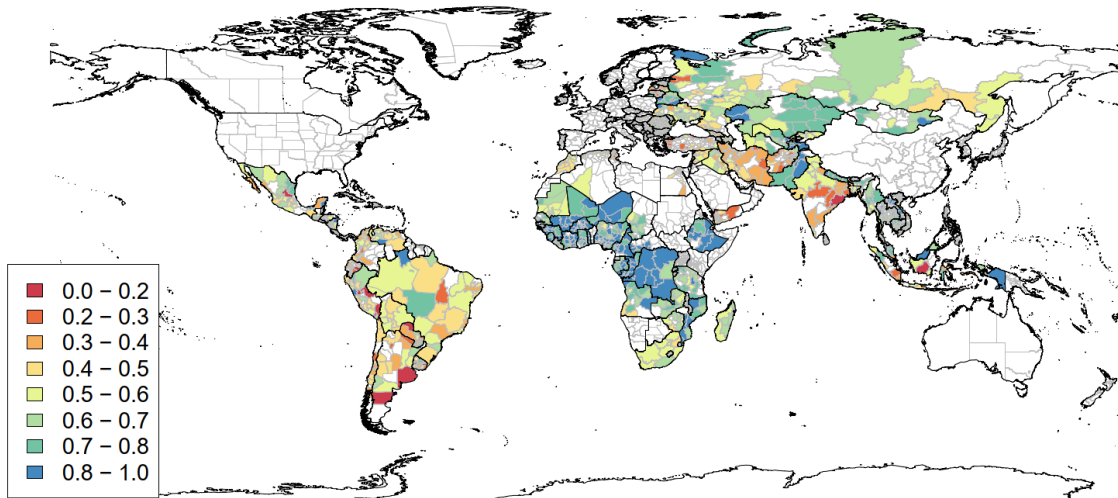
![red]	0.0 – 0.2
	0.2 – 0.3
	0.3 – 0.4
	0.4 – 0.5
	0.5 – 0.6
	0.6 – 0.7
	0.7 – 0.8
	0.8 – 1.0

Notes: This figure shows the average share of interviewed individuals that approve of the Chinese government by province (ADM1 region) over the sample period (2006-2017). Data from Gallup (2018).

Figure 1.3 – Support for the Chinese government by world region over time, 2006–2017
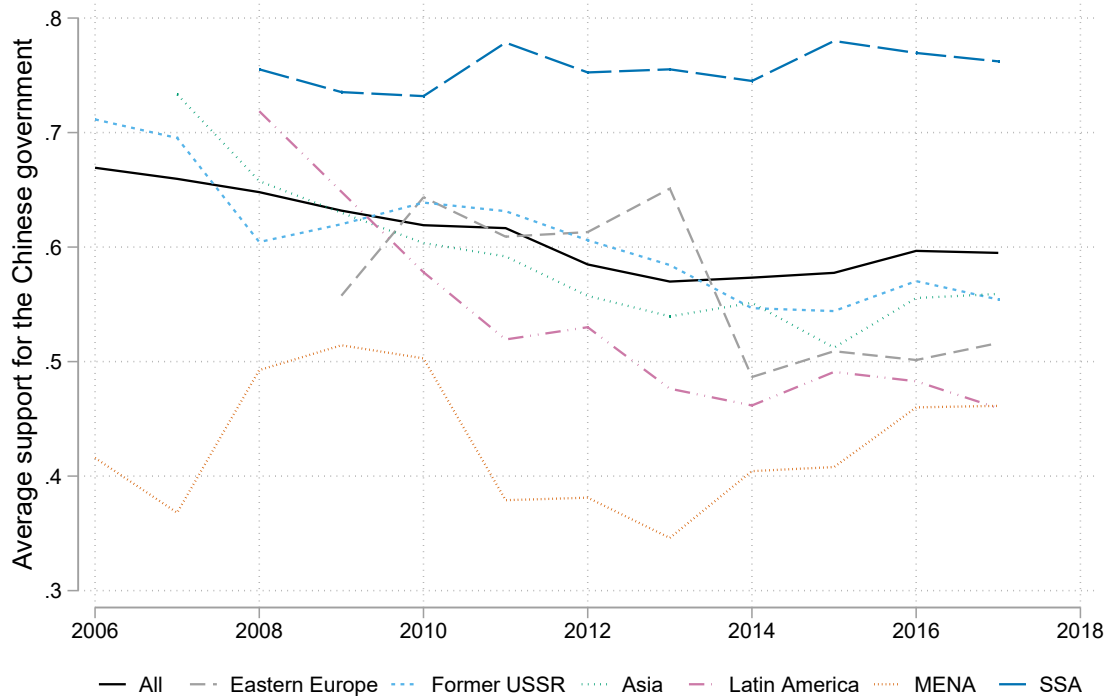
Notes: The figure displays the average approval of the Chinese government by world region over the 2006–2017 period. We first create country means and then world region averages. Abbreviations: USSR–Union of Soviet Socialist Republics; MENA: Middle East and North Africa; SSA: Sub-Saharan Africa. Data from Gallup (2018).

1.3 The Short Run: Individual-level Event Study

Our goal is to estimate the effects of Chinese development projects on public support for the Chinese government. However, isolating these potential effects is difficult since individuals are exposed to many potentially confounding factors. For example, any other interactions with Chinese state or non-state actors—whether direct or indirect—could affect individuals' opinion toward the Chinese government. Many individuals in developing countries regularly observe and interact with Chinese markets, shops, goods and services, Chinese state-owned and private businesses and investors, managers and workers of Chinese companies, and news coverage provided by local outlets, international media agencies, and Chinese state-run media (Fung et al. 2021).

Given that public opinion towards China and the Chinese government is likely based on composite assessments of these diverse observations and experiences, isolating the effects of Chinese government-financed development projects is key for our empirical strategy. We address this challenge in two ways. In this section, we use narrow time windows around project commitment, start, and end dates to isolate potential short-term effects of Chinese development projects on approval of the Chinese government. In the next section, we use an instrumental-variables approach that relies on exogenous variation in the supply of Chinese development projects to estimate longer-term effects.

1.3.1 Empirical Strategy

We carry out an individual-level analysis of short-term effects of Chinese project events. This approach exploits the staggered roll-out of the GWP, which specifies the exact calendar days on which respondents are interviewed. All individuals within a country wave are interviewed on days within a time window of about four weeks.[30] Our new data on project-level event dates allow us to identify GWP country survey waves that fall in windows around these events. Project events include the three types introduced in the previous section: (1) the commitment of new projects, which are typically announced at bilateral meetings that attract public attention; (2) the start of project implementation, which is visible on the ground and often accompanied by a groundbreaking or foundation stone-laying ceremony; and (3) project completion, which is often covered by the media and accompanied with a ribbon-cutting ceremony, particularly for larger infrastructure projects. In total, 29,331 individuals in 35 countries and 420 provinces were exposed to 45 projects totalling 53 project events that fall into the respective survey windows over the 2006–2017 period.[31]

These 53 project events are covered at random, based on whether or not they fall within

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[30]The mean number is 26 days with a standard deviation of 19 days.

[31]The number of events exceeds the number of projects because eight events are included twice, with both their start and end dates falling within an interview period.

the windows around GWP interview dates. Both project events and GWP interviews occur throughout the year and cover all weeks of the month and all days of the week (see again Figures 1.B.1 and 1.B.2). Table 1.B.2 in the Appendix tests whether these 45 projects differ in their main characteristics from (i) other projects where information on event dates is also available, and (ii) the entire sample of projects (independent of whether information on dates is available). It demonstrates that the 41 projects are by and large representative of all projects in the dataset with information on commitment, start, and end dates, respectively (columns 1–3), as well as the entire sample of projects (columns 4–6). These projects do not substantially differ from those without dates across a wide range of project characteristics.[32] The subsample of projects covers a wide range of activities, including the construction and expansion of airports, roads, and railways, the dispatching of peacekeeping missions and medical teams, and the construction of primary schools, Confucius Institutes, and government buildings (see Tables 1.B.3–1.B.5).

Using this subset of projects and project events, we compare individuals interviewed just before a given project event to those interviewed just after it. Our identifying assumption is that the timing of Chinese project events—i.e., commitment, start, and end dates—is independent of the timing of the GWP interviews. This is plausibly the case. There is no obvious reason why the interview dates of a U.S.-based survey enumeration firm would systematically be related to the timing of Chinese project events. Nevertheless, we take several measures to ensure that our control group (individuals interviewed just before a project event) is comparable to our treatment group (individuals interviewed just after a project event). First, we only compare individuals interviewed in the same province and year, as we control for province-year level confounding factors through province-year fixed effects. Second, we limit the sample to include only individuals interviewed 30 days before and after the event to further mitigate the probability that individual opinion is driven by other events.[33] Third, we add survey-level control variables to ensure that the effect is not driven by any underlying implementation of the GWP survey in a specific wave. Based on these measures, it is arguably random if an individual is interviewed before or after a project event, which allows us to interpret our results in a causal manner.

Specifically, we estimate the following equation:

$$Support_{ipcdy} = \beta post_{icdy} + \gamma X_{ipcdy} + \delta S_d + \zeta_{pcy} + \epsilon_{icpdy}, \tag{1.1}$$

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[32]Commitment and start dates are significantly more available for more recent years, which is unsurprising since it should be easier to gather information on more recent events where online media coverage should be better. We capture this by the inclusion of fixed effects in the analyses below. There is also some evidence that commitment dates are more (less) often available for projects in the production sector (ODA projects), and start dates for economic infrastructure projects.

[33]We choose 30 days as it roughly corresponds to one month, but also test robustness to other event windows.

where $Support_{ipcdy}$ is a binary variable that takes a value of one if individual i living in province p of country c interviewed on day d in year y approves of China's government (as introduced in the previous section). $post_{icdy}$ indicates whether or not an individual has been interviewed within 30 days after a project event in the country—either commitment, start of the implementation period, or completion.[34] X_{ipcdy} are individual-level control variables, which include a binary variable if the respondent is female, the respondent's age in years and its square, an education indicator, and an urban area indicator.[35]

S_d represents survey-level control variables, including binary variables for the day of the week and an indicator counting the days of the GWP survey windows.[36] ζ_{pcy} denotes province-year-fixed effects. Finally, ϵ_{ipcdy} is the error term. We cluster standard errors at the level of the treatment, which is the country×interview date. β is the coefficient of interest that shows the average effect of Chinese projects on support.[37]

β is our coefficient of interest. It will likely represent a lower bound of the true effect. First, it is likely that public opinion in the pre-project period is affected by other, earlier, projects, which biases our coefficients towards zero.[38] Second, it is likely that news related to project events disseminate in the days just prior to an event. However, it appears unlikely that within such short periods of time, the nature of disseminated news changes systematically in one direction. If reporting prior to the event exists at all, it seems likely that reports that are positive (negative) about China in the days before commitment will also be positive (negative) after commitment. We would then attribute parts of the effect to the control group, which would bias our estimated effect downwards. For example, consider Kenya's Standard Gauge Railway (SGR). The SGR was officially committed on May 11, 2014 but became known through the media earlier when the China Road and Bridge Corporation started negotiating with Kenya's government. The Daily Nation, one of Kenya's most influential newspapers, started publishing articles that made reference to the proposed SGR by early 2014. While the vast majority of this media coverage occurred well after the project's commitment date, five articles were published before commitment, but only one of those within 30 days before the official commitment date. Given that

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[34]To be conservative, we include the treatment day in the control group. By doing so, we assume that events take some time to unfold. Results are robust to instead excluding the event day from the analysis.

[35]The education variable takes a value of one if the respondent has 1–8 years of schooling, a value of two for 8–15 years, and a value of three for 15 years or more. The urban area indicator is one if the respondent lives in a rural area or village, two for a small town, three for the suburb of a large city, and four for a large city. Panel A of Table 1.B.6 provides descriptive statistics of all variables employed in the individual-level analysis.

[36]When we further add fixed effects for each month, results are virtually unchanged (see Table 1.C.1).

[37]Our results are robust against a variety of different levels of clustering standard errors, including two-way clustering for country and interview date, country and interview wave, province and interview date, province and interview wave, country, province, wave, and date. When we cluster for country×interview wave or wave, our results remain similar, but are slightly weaker in terms of statistical significance.

[38]Results are robust when leaving out those projects where another project was committed, started, or completed in the six months prior to the project event.

this article is assigned to the control group, it could bias our estimate downwards. We will thus interpret our results as lower-bound estimates.

Estimating the effects of development projects on Chinese government approval for all individuals in a given country is suitable to the extent that (country-wide) reporting about projects drives attitudes rather than local news or personal experiences on the ground. For China and other donor governments, country-level attitudes are likely more important than local attitudes in the pursuit of soft power. However, by additionally estimating provincial effects, we can also account for local experiences. While our main analysis focuses on project events anywhere in a country, we also estimate a variant of eq. 1.1 in which we add an interaction term of $post_{icdy}$ with a binary indicator for projects that are placed in the province of the person interviewed, testing whether opinions of individuals who have been interviewed within 30 days after a project event in the province differ from those interviewed elsewhere in the recipient country. We thus also present results that compare individuals of the same province interviewed before a project date to those interviewed thereafter.

1.3.2 Micro-level Results

Table 1.1 shows our main results for the short-run impact of Chinese development projects on public opinion towards China's government, based on the event-study approach of eq. 1.1. Column 1 of panel A investigates the effect of project events for all projects in our sample—small and large ones alike and independent of whether or not they are concessional. The coefficient on *post* shows that there is no significant difference in opinion about the Chinese government across individuals interviewed after as compared to before a project event. In columns 2–4 of panel A, we investigate these events separately by event type. This reduces the number of projects included in the analysis substantially. Column 2 shows the average effect of 9 committed projects, column 3 that of 19 projects that start the implementation phase, and column 4 that of 20 completed projects. The results show that only project completion significantly affects public opinion, at the ten-percent level of significance. Being interviewed after the project end date increases Chinese government approval by 3.0 percentage points.[39] This effect is sizable in light of the sample mean of 70 percent and given that we likely obtain lower-bound estimates for the reasons outlined above.

We offer four potential explanations for this strong result: First, completed projects are often touted by recipient and donor governments as visible achievements. Project

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[39] We have tested heterogeneity on a number of dimensions: We find no evidence that individuals with better education or access to information evaluate projects differently. Nor do we find lagged approval of China's government, one's own government, or the U.S. government to affect how China's projects affect approval. While we do not report these results in a table to reduce clutter, they are available on request.

Table 1.1 – Chinese projects and government support, event study results

	(1) All	(2) Commit.	(3) Start	(4) End
	Panel A: All projects			
Post	0.0108	-0.0509	0.0225	0.0303*
	(0.0121)	(0.0373)	(0.0154)	(0.0156)
Observations	29,331	5,610	15,362	15,465
Number of countries	35	9	19	20
Number of provinces	420	128	185	247
Number of projects	53	10	21	22
Province-year FE	✓	✓	✓	✓
	Panel B: ODA projects			
Post	0.0228	-0.0421	0.0462**	0.0412**
	(0.0145)	(0.0812)	(0.0183)	(0.0171)
Observations	19,017	1,744	10,528	12,226
Number of countries	22	3	14	15
Number of provinces	265	58	125	171
Number of projects	35	3	15	17
Province-year FE	✓	✓	✓	✓
	Panel C: Large projects			
Post	0.0243	0.0347	0.0413*	0.0574**
	(0.0185)	(0.0561)	(0.0249)	(0.0272)
Observations	13,783	2,169	5,619	7,865
Number of countries	19	5	7	11
Number of provinces	254	61	62	161
Number of projects	24	5	8	11
Province-year FE	✓	✓	✓	✓

Notes: The dependent variable is binary and indicates whether or not an interviewed individual approves of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. Panel A shows results for the full sample of projects that contain information on the specific event date and fall into the interview window. Panel B only includes "Official Development Assistance-like" projects. Panel C only includes large projects, i.e., those with a size of US$ 1 million or above. All specifications include individual-level and survey controls as well as province-year fixed effects. Standard errors are clustered by country-date: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

completion dates erase any uncertainty about whether a project will actually reach completion. Second, the completion of a project ends negative spillovers or externalities directly stemming from the construction or other elements of project implementation. By contrast, when individuals initially hear of a project commitment, they might not expect positive effects from the project and only realize such effects after project completion. Third, individuals might only learn about where and what types of projects China's government has provided after a project's completion. Finally, there is a fourth, more technical, explanation. As can be seen in Figure 1.B.5, approval of China's government is above average on some days prior to the respective commitment and start dates, which makes it less likely we observe positive approval effects of these project events. To the contrary, there are no significant differences from average approval rates prior to project completion dates, so there is no downward bias for these dates.

A skeptical reader might worry that the presence of pre-trends could render our results spurious. To guard us against such possibility, we draw 60-day periods from the GWP data at random. We do this 22 times, as this corresponds to the number of project end dates in our sample. We then draw one random date from each of the random periods and use those as placebo treatments in our regression analyses. We repeat this procedure 999 times.

Figure 1.B.6 shows the results of this randomization inference test based on 999 Monte Carlo replications. As can be seen, the results follow a normal distribution centered around zero. The p-value from an exact Fisher test shows that the absolute value of the t-statistic in the simulated data exceeds those of the original t-statistic in 8.1 percent of the regressions. These results indicate that spurious trends cannot explain the significant results from our event study.

To win the approval of the general public, donors often emphasize that their projects represent acts of generosity (Dietrich et al. 2019). Individual projects vary significantly in their financial terms and degree of generosity, and most donors provide a combination of highly concessional ODA and less concessional (or non-concessional) OOF. Panel B reports the effects of highly concessional projects only. At the five-percent level of significance, we again find that public opinion of the Chinese government improves when such projects are completed (column 4). Individuals interviewed within 30 days of the completion of a Chinese ODA project approve of the Chinese government by 4.1 percentage points more than individuals interviewed in the prior 30-day period.[40] It is not surprising that when compared to projects financed at market or near-market rates, projects financed on highly concessional terms more effectively increase public support for donor governments—either due to perceived generosity or competence.

Development projects also differ substantially in terms of their physical and financial

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[40]These results are robust to decreasing the bandwidth to 20 days and to increasing it to 40 or 60 days (Table 1.C.2).

size. We expect larger projects to produce greater public opinion effects not only because they signal more generosity but also because they possess higher levels of visibility within recipient countries. For one, larger projects have a higher probability that individuals within a developing country accurately attribute them to the donor country. Such attribution should not be taken for granted. Indeed, experimental evidence indicates that citizens often do not know which foreign governments finance specific development projects (Baldwin and Winters 2020). What is more, compared to smaller projects, larger aid projects are often more visible both in terms of their physical and media presence. Earlier research indeed suggests that development project visibility matters for both recipient and donor country governments.[41] Panel C includes only large projects, which we define as those with commitment values of US\$ 1 million or more. As expected, the observed effect for large projects (of 5.7 percentage points) is stronger than for all projects. Moreover, approval of the Chinese government increases when concessional and large projects commence, at the five- and ten-percent level, respectively (see column 2).[42]

Next, we test whether the effect of project events differs for respondents from the province where the project is located, compared to the effect for all individuals interviewed in the recipient country. To this end, we include the interaction of the treatment indicator with a binary variable that indicates that a project is from the same province as the interviewee.[43] The results show only two significant coefficients on this province-specific indicator in our twelve specifications (see Table 1.C.4 in Appendix B). First, the commitment of one additional project increases public approval for the Chinese government (among people interviewed in the project province) by 15.1 additional percentage points—as compared to the effect that is observed among all interviewees outside the project province (column 2). Second, albeit smaller in size, we also find that project completion events trigger a larger response for ODA projects (column 8). Since all other interaction terms are insignificant at conventional levels, it seems that the overall effects reported above are not driven by provincial- or local-level opinion effects, but rather by project-specific news or information that is accessible to citizens nationwide. Given that our event-study approach measures short-term effects where experiences with project outcomes should differ less across individuals, this result makes intuitive sense. To capture potentially heterogeneous individual experiences with project outcomes, we need to look at a longer time frame. We do this in the next section.

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[41]For recipients, completing highly visible projects with tangible site locations can benefit incumbent host country leaders' reelections (Marx 2018). Highly visible projects can also potentially send clearer signals of political support between donor and host governments (Strange 2021).

[42]For completeness we also tested the effects of projects with funding below US\$ 1 million and of OOF-like projects. Table 1.C.3 shows that 9 out of 10 coefficients are not significant at conventional levels. The exception is the effect of commitment-related news on small projects, which is negative at the ten-percent level of significance. However, this latter finding should not be overinterpreted as it relies on only four projects.

[43]Note that we include province-fixed effects that capture the constituent term of the interaction.

As an additional robustness test, we turn our attention to China-related events that might coincide with project events and potentially bias our estimates. One possibility is that high-ranking official visits by Chinese leaders take place around the same time as project events. We would then falsely attribute the effects of those visits to project events.[44] To mitigate such concerns, we investigate state visits by the Chinese presidents that occur around project dates. Using data from Wang and Stone (2023), we find that only five leader visits fall within the 30-day windows around project events (3 commitment dates, 1 start date, 1 end date). When we exclude these five events, our results are qualitatively the same, as we show in Table 1.C.5. If anything, they become stronger. This makes us confident that our results are not significantly biased by high-ranking diplomatic visits that happen to be at the same time. While we cannot fully rule out that other China-related events might affect our results, we deem it unlikely that these would bias our results in a systematic manner given that we observe no bias in case of state visits, which are clearly exceptional events.

Finally, we test whether we come to the same conclusions when we repeat the analysis using an alternative measure of China's soft power. Responses for other questions in the GWP potentially related to Chinese soft power are however of very limited availability. We test whether our results hold when we use a respondent's general opinion on China as outcome variable, which is the most populated variable after our main outcome. The underlying question is the following: "In general, what opinion do you have on the following nations? China," which we recode to 1 if the opinion is favorable, and 0 if the opinion is unfavorable. The question is answered by 60,000 individuals (compared to 900,000 that answered our baseline question whether or not they approve the job performance of the Chinese government). Despite this low number, our main results hold when using the opinion of China as dependent variable (Table 1.C.6).

1.4 The Longer Run: Macro-level Analysis

1.4.1 Empirical Strategy

A disadvantage of the above event study is that we can only include a small set of projects that are committed, started, or completed during GWP survey windows in a particular country. This also implies that we rely on only a small portion of interviews from the GWP. Moreover, while the event study allows us to identify causal effects, we can only test effects shortly after an event has happened and cannot analyze potential longer-term effects. We therefore re-analyze our dataset at the annual level in order to exploit the full range of projects. We estimate two sets of equations, one at the province level and one

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[44]In this regard, Goldsmith et al. (2021) find that leader visits can increase public approval among foreign citizens, indicating that public diplomacy has the potential to impact soft power variables.

at the country level.[45] The province-level regressions allow us to analyze more granular potential effects and are instrumental for our identification strategy at the country level, as we explain below. The country-level regressions capture the public opinion impacts of Chinese government-financed projects on an entire country regardless of individuals' physical proximity or even direct exposure to projects. From the perspective of a donor government interested in maximizing its soft power, the country level is arguably more important. Starting with the province level, we estimate the following regression equation:

$$Support_{cpy} = \beta ChineseProjects_{cpy-1} + \sum_j \delta_j X^j_{cpy} + \zeta_{cp} + \eta_{cy} + \epsilon_{cpy}, \qquad (1.2)$$

where $Support_{cpy}$ is the share of individuals that approve of China's government in province p of country c interviewed in year y.[46] $ChineseProjects_{cpy-1}$ denotes the number of Chinese development projects completed in a province in the previous year;[47] X^j_{pcy} are our individual-level control variables introduced in the previous section and averaged at the province level; ζ_{cp} are province-fixed effects; and η_{cy} denotes fixed effects for country-years. The inclusion of country-year fixed effects implies that any effects of development projects that affect public perception equally across provinces are netted out so that the effect of projects at the province level has to be interpreted relative to this average effect. Again, ϵ_{pcy} is the error term; we cluster standard errors at the level of countries.[48]

Estimates obtained from eq. 1.2 likely suffer from endogeneity bias as projects are not randomly distributed over time and space. For example, reverse causality might bias our results if development projects are deliberately placed in regions to increase support at times when the Chinese government is unpopular (Asmus et al. 2022). Conversely, provinces with individuals who possess relatively favorable opinions of China in a certain year could obtain more projects if China believes that public support is conducive to project success or if provincial leaders maintain close ties to the Chinese government. For instance, Blair et al. (2022) find that Chinese development projects (in Africa) are often situated in regions where public opinion about China is more favorable relative to other locations. Jones (2018) similarly shows that public opinion about China in Africa is more favorable in the home regions of political leaders in aid-receiving countries, which tend to receive a higher share of Chinese-funded projects (Dreher et al. 2019).

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[45]Panels B and C of Table 1.B.6 show the descriptive statistics on the province and country levels.

[46]We make use of sampling weights by GWP when aggregating individual responses to the province or country level. Although we believe that the GWP weights are preferable, Table 1.D.2 in the Appendix presents our main regression results where we weight provinces with the number of individuals surveyed as share of total survey respondents in the country-year wave. Results are very similar.

[47]Given that this specification aims to test the longer-run effects of Chinese development projects, completion dates are arguably most appropriate.

[48]Results are robust when we cluster at the level of (i) country×year and provinces, (ii) provinces, and (iii) province×year. When we cluster at the level of provinces, the first-stage F-statistic is however lower (8.6).

We address the endogeneity of development projects using a Two-Stage-Least Squares (2SLS) regression, replacing $ChineseProjects_{cpy-1}$ in eq. 1.2 with $\widehat{ChineseProjects}_{cpy-1}$, based on the following first-stage regression:

$$ChineseProjects_{cpy-1} = \widetilde{\beta}(Input_{y-3} \times \mu_{cp}) + \sum_j \widetilde{\delta}_j X^j_{cpy-1} + \widetilde{\zeta}_{cp} + \widetilde{\eta}_{cy-1} + \widetilde{\epsilon}_{cpy-1}, \quad (1.3)$$

where we rely on a two-part instrumental variable for Chinese development projects introduced in Bluhm et al. (2020). First, relative to domestic demand, China overproduces cement, iron, steel, and other construction inputs. China's government instructs Chinese commercial actors to offload excess production in foreign markets. Bluhm et al. show that excess supplies of physical project inputs in China leads to increases in Chinese development projects abroad. More precisely, we use a proxy for China's supply of potential project inputs that includes (logged) production of six raw materials: aluminum, cement, iron, steel (all measured in 10,000 tons), glass (measured in 10,000 weight cases), and timber (10,000 cubic meters). As in Bluhm et al., from these six inputs, we draw the first factor using factor analysis and detrend the resulting time series.

The first factor of Chinese raw material production captures annual fluctuations in (potential) Chinese project inputs, but does not include spatial variation in terms of the countries and provinces that receive the additional financing from China. The second part of the instrument, also introduced in Bluhm et al. (2020), is based on the intuition that locations that receive Chinese development projects more frequently will be more impacted by changes in China's overall supply of project inputs. We thus interact the measure for China's project inputs described above with a province's probability of receiving Chinese development projects, which we proxy with the share of years during 2000–2014 in which at least one project was completed in a given country or province. Following a growing body of research on aid effectiveness, we thus interact a variable that varies exclusively over time with a variable that varies over space (ADM1 regions), resulting in an instrumental variable that varies in both dimensions.[49]

In eq. 1.3, $Input_{y-3}$ is the (logged and detrended) factor of the production levels of the six physical project inputs; μ_{cp} is the province-specific probability of receiving Chinese development projects;[50] ζ_{cp} are province-fixed effects, and η_{cy-1} are fixed effects for

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[49]See Werker et al. (2009), Dreher et al. (2019), Lang (2021), and Gehring et al. (2022), among others. Dreher et al. (2021b) propose a second instrument—the change in China's currency reserves in a year interacted with the probability of receiving Chinese development finance. In our comparably short sample, currency reserves and the production of China's construction materials are highly correlated (0.85) so that little information is added by including the second instrument. When we do, results are almost identical, with substantially lower first-stage Kleibergen-Paap F-statistics.

[50]Specifically, we calculate this as the share of years in which at least one Chinese development project was completed over the entire time these data are available as follows: $\mu_{cp} = 1/15 \sum_{t=1}^{15} \mu_{cpt}$, where μ_{cpt} is a binary variable indicating if a development project has been completed in a given province in year t.

country-years, capturing the production of Chinese project inputs. On average, projects are completed within two years after commitment.[51] The time from commitment to project completion however largely varies by project sector, so using the time of project completion instead of commitment in combination with a two-year lag allows us to measure the impact of development projects more precisely. Allowing one additional year for the projects to take effect, we lag completed projects by one year. We lag input factor production by two additional years so that it is measured in $y - 3$ relative to the year of an interview.

The intuition behind our identification approach resembles a difference-in-differences design. We investigate a differential effect of surplus Chinese project inputs on public opinion in regions with a high probability versus those with a low probability of receiving Chinese development finance. The identifying assumption is that other than the effect of Chinese development finance on public opinion, public opinion in provinces with differing probabilities of receiving development finance from the Chinese government will not be differentially affected by changes in China's production of physical project inputs, after controlling for province- and country-year-fixed effects and the other variables in the model. As in any difference-in-differences setting, we rely on a conditionally exogenous treatment and the assumption of parallel trends across groups. In controlling for country-year-fixed effects, Chinese production volumes of project inputs cannot be correlated with the error term and are thus (conditionally) exogenous to China's provision of international development projects. For different trends to exist, these trends across the treatment and control groups—provinces with a high probability and provinces with a low probability of receiving development finance from China—would have to vary in tandem with period-to-period changes in the production of project inputs.

We assess the validity of this approach in several steps following Bluhm et al. (2020). First, the identifying assumptions of the above approach could be violated if fluctuations in China's production in project inputs had different effects on "regular" and "irregular" recipient provinces' probabilities of receiving Chinese development projects, and if such effects had unique consequences for public opinion toward China's government across these provinces. In other words, this approach provides plausibly exogenous timing of the intervention, but its validity still rests on parallel pre-treatment trends across regular and irregular provinces that receive high and low levels of Chinese development finance. In Figure 1.B.7 in the Appendix, panels A and B display raw production volumes of the six input materials—both in levels and linearly detrended. Panels C and D report the first factor of the production volumes that we extract via factor analysis (again, both in levels and detrended). Panels E and F plot variation in the number of projects completed and public opinion for districts with above- and below-median probabilities

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[51]Figure 1.B.3 in the Appendix visualizes the average time between project event dates; see Figure 1.B.4 for sector-specific results.

to receive development finance from China. Overall, there is minimal concern that the parallel trends assumption is violated. We observe a global upward trend in material production. After purging the linear trend, the series trends upwards until 2010 and then turns downward. This trend is reflected in China's global provision of development projects over the same period, as we would expect, since the former serve as inputs into the latter. Moreover, the probability-specific trends for projects and public opinion appear generally parallel for the provinces with above and below median probabilities to receive Chinese projects. There is no apparent non-linear trend that resembles the trend of the detrended first factor of Chinese domestic input production in one of the two groups (above and below median probability to receive development finance) more than the other (Christian and Barrett 2017).

Second, to obviate the assumption of parallel pre-treatment trends, we allow for correlated random trends. As Bluhm et al. (2020) point out, the identifying assumption is $Cov(Input_{y-3} \times \mu_{cp}, \epsilon_{cpt}) = 0$, conditional on the set of control variables and fixed effects. This approach minimizes potential confounding factors. One possibility is that the detrended input series might be correlated with production volumes or prices of other commodities apart from our six input materials. If so, and if the time-varying effects of these variables on public opinion were equal across all regions in a country, then detrending the raw series and including country-year-fixed effects would capture such effects. If the effects were instead linear but different across provinces, then province-fixed effects would capture them. Finally, if time-varying effects were both non-linear and different across provinces, one would need to account for such potential shocks. We address this possibility in the robustness tests below (Appendix Section 1.D.2).

Third, a separate concern is whether China's domestic production of physical project inputs could be correlated with China's trade or foreign direct investment volumes (Bluhm et al. 2020, Dreher et al. 2021a). China's share of world manufacturing value added has risen steadily since 2000, and this coincided with a large demand shock for raw materials (Autor et al. 2016). What is more, frequent recipients of Chinese development projects might also be popular destinations for Chinese foreign direct investment projects or have closer trade ties with China. If so, differences in public opinion across provinces in developing countries might actually be due to trade or foreign direct investment rather than Chinese-financed development projects. To investigate this possibility, we conduct robustness tests below that control for annual volumes of exports to China, imports from China, and Chinese foreign direct investment. We interact these volumes with a set of variables that makes it more or less likely that a province will be affected by variation in China's overall trade or investment (Appendix Section 1.D.2).

Our identification strategy is related to a growing literature that employs shift-share instrumental variables. Such instruments are often constructed as macro-level shocks to a variety of industries that have varying degrees of local exposure. Within this setting,

one can achieve identification in two ways using alternative assumptions. In the first approach, if local industry shares are plausibly exogenous, they can be taken as instruments (Goldsmith-Pinkham et al. 2020). In the second approach, even when variation in local exposures may be endogenous, one can alternatively stake identification on exogenous variation in time-series shocks.[52] Our approach is somewhat different as we rely on endogenous exposure to a single (and perhaps endogenous) shock. Rather than arguing that the shock is necessarily exogenous, we rely on alternative assumptions discussed above.[53]

Next, we turn to country-level regressions and employ the same approach used in our province-level regressions shown in eq. 1.2 and eq. 1.3. We aggregate the predicted number of projects in all provinces in a year from eq. 1.3 to the level of countries. We then use these aggregates as instrumental variables for the total number of new projects at the country level.[54] Alternatively, one might consider to directly estimate 2SLS regressions at the country level. However, the corresponding first-stage Kleibergen-Paap F-statistics are too low (see panel A of Table 1.D.3 in Appendix D).[55] This is why we use the following first-stage regression equation that relies on the predicted number of projects:

$$ChineseProjects_{cy-1} = \widetilde{\beta} \sum_p Chinese\widehat{Projects}_{cpy-1} + \sum_j \widetilde{\delta}_j X^j_{cy-1} + \widetilde{\zeta}_c + \widetilde{\eta}_{y-1} + \widetilde{\epsilon}_{cy-1}. \quad (1.4)$$

In the second stage, we then run country-level regressions with individual-level variables

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[52]For the panel case, Borusyak et al. (2022) show that the estimator is consistent when the covariance between the detrended input series and a weighted average of the within-location time variation in unobserved factors affecting public opinion approaches zero in large samples. This is likely with reasonably large T, combined with a set of fixed effects, and can be supported by including proxies for the remaining unobserved variation (Bluhm et al. 2020).

[53]We conduct several tests suggested by Christian and Barrett (2017) to probe the validity of these assumptions. In addition to visually examining trends in Figure 1.B.7, we conduct a randomization inference test where we reassign the number of projects and the corresponding instrumental variable to different countries and years in the sample, just as we did for the event study in Figure 1.B.6 above. As can be seen from the results of 999 regressions reported in Figure 1.B.8, coefficient estimates are concentrated around zero. According to an exact Fisher test, the coefficient from our main estimate above (indicated by the vertical dashed line) is significantly different from the randomized coefficients (p-value: 0.078). This holds when we break the timing structure required for identification and instead randomize the entire time series between countries, years within countries, and countries within years. In short, it is unlikely that any omitted variables correlate with our variables of interest in a way that spuriously produces our main results.

[54]Note that standard errors are estimated consistently as long as the second-stage error term is not correlated with the instrumental variable of the regression we use to predict the generated instrument (Wooldridge 2010). In our case, the exclusion restriction using province-level probabilities to receive aid rather than probabilities at the country level does not pose additional demands. When we employ wild bootstrap at the second stage, results are unchanged. Papers using a dyadic zero-stage regression to construct their instrumental variable just like we do are Frankel and Romer (1999), Rajan and Subramanian (2008), and Dreher and Langlotz (2020).

[55]The low power of the instrument compared to Dreher et al. (2021b) can be explained by the shorter sample period in our study (2006–2017). When we repeat the first-stage regression with the full 2000–2017 sample, the instrument has sufficient power. We show these results using the out-of-sample prediction in panel B of Appendix Table 1.D.3. They are similar to those of our baseline approach reported below.

averaged at the country level and controlling for fixed effects for countries and years:

$$Support_{cy} = \beta Chinese\widehat{Projects}_{cy-1} + \sum_j \delta_j X_{cy}^j + \zeta_c + \eta_y + \epsilon_{cy}, \qquad (1.5)$$

where $Support_{cy}$ is the share of individuals that approve of China's government in country c interviewed in year y. $Chinese\widehat{Projects}_{cy-1}$ denotes the predicted number of projects completed in a country in the previous year; X_{cy}^j are our j individual-level control variables introduced in the previous section; ζ_c are country-fixed effects; and η_y denotes fixed effects for years. Again, ϵ_{cy} is the error term; we cluster standard errors at the level of countries.

1.4.2 Macro-level Results

Table 1.2 presents the results on the longer-term effects of completed development projects, following eqs. 1.2–1.5. Columns 1 and 2 show the results for all projects, at the level of provinces and countries, respectively. Columns 3 and 4 show the analogous regressions for ODA projects only; columns 5 and 6 those for large projects. While we calculate probabilities to receive aid for these regressions based on ODA and large projects, respectively, these probabilities are highly correlated with those to receive any type of development project from China. This violates the exclusion restriction to some extent. While we report these separate results for completeness, we acknowledge that our approach does not allow to neatly identify separate effects across project types.[56]

Panel A presents the results from ordinary least-squares (OLS) regressions that leverage within-country or -province variation. Consistent with the results from the event study, we find significant and positive (conditional) correlations at the level of countries. However, controlling for country-year fixed effects, we do not find additional effects at the province level. This suggests that the reputational benefits of completed development projects are not primarily based upon improved conditions near project sites. This pattern suggests that the public opinion gains resulting from Chinese development projects are attributable to changing national perceptions rather than the firsthand observations by or experiences of people with direct project exposure.

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[56]What is more, the local average treatment effects (LATE) that we estimate with our input material-based instruments might capture similar projects in case we use them to predict ODA projects, large projects, or all of them. Very small projects, for example, are less likely to depend on input materials than large ones.

Panel B in Table 1.2 reports the reduced-form estimates for the same set of regressions. Here we regress average attitudes towards the Chinese government on our instrumental variable (in addition to the fixed effects and control variables). If our identification strategy holds in the presence of an effect of development projects on approval rates, we should also observe strong reduced-form effects. Indeed, there is a sizable and significant effect of the instrumental variable on approval at the level of countries and provinces. At the country level, the coefficient is positive and statistically significant for all projects combined (shown in column 2), ODA projects (column 4), and large projects (column 6) alike. The effect at the province level is negative and statistically significant at conventional levels (as shown in columns 1, 3, and 5), which suggests that those living in close proximity to Chinese development projects (i.e., those who have more direct project exposure) develop less favorable views of the Chinese government, compared to the average effect in the same country and year. This finding is consistent with previous research that provides evidence of higher levels of corruption, political capture, ethnic tension, social protest, and environmental degradation in areas within close proximity of Chinese development projects (Isaksson and Kotsadam 2018a,b, Dreher et al. 2019, Isaksson 2020, Iacoella et al. 2021, Baehr et al. 2022). The effects will be passed through with the same sign if the corresponding first-stage regression is sufficiently strong and the coefficients on our instrument are positive, as expected.

In panel C of Table 1.2, we present our main results in which we instrument development projects completed one year ago with our instrument two years earlier as in eq. 1.3. At the country level (columns 2, 4, and 6), the coefficients increase slightly compared to the corresponding OLS estimates and remain statistically significant at least at the ten-percent level. Each additional Chinese development project that reaches completion increases public approval for the Chinese government by 0.21 percentage points. While this longer-run effect is smaller than the short-term increase of 3.03 percentage points identified in the previous section, this finding demonstrates that public opinion effects persist over a longer period of time. On balance, it appears that China's development program pays off in terms of soft power acquisition.[57]

Panel D in Table 1.2 reports our corresponding first-stage regression results. Recall that we instrument the number of country-wide projects with the sum of predicted province-level projects. It is reassuring that the corresponding first-stage Kleibergen-Paap F-statistics are all high with values of 464 (all projects), 555 (ODA projects), and 651 (large projects). As expected, we observe a positive relationship between our instrumental

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[57]Table 1.D.1 provides sector-level results. We discuss those results in detail in Appendix 1.D.1. We also repeated the analysis with opinion on the Chinese nation (rather than the Chinese government) as alternative measure of soft power. Since this alternative question is answered by 60,000 individuals only (compared to 900,000 that answered our baseline question whether or not they approve the job performance of the Chinese government), we assumed this number to be too low to estimate precise estimates and indeed this holds true (Table 1.D.4).

Table 1.2 – Chinese projects and government support, instrumental variables results

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: OLS					
Chinese projects $_{y-1}$	-0.00380	0.00184**	-0.00390	0.00167**	-0.000135	0.00127*
	(0.00301)	(0.000852)	(0.00353)	(0.000741)	(0.00342)	(0.000669)
	Panel B: Reduced form					
Input*probability $_{y-3}$	-0.139**		-0.173**		-0.188*	
	(0.0677)		(0.0721)		(0.0997)	
$\sum_p$Input*probability $_{y-3}$		0.00252**		0.00233**		0.00172*
		(0.00109)		(0.000972)		(0.000893)
	Panel C: 2SLS					
Chinese projects $_{y-1}$	-0.0722*	0.00206**	-0.0864**	0.00189**	-0.0682*	0.00140*
	(0.0389)	(0.000973)	(0.0409)	(0.000859)	(0.0400)	(0.000771)
	Panel D: First stage					
Input*probability $_{y-3}$	1.929***		1.998***		2.758***	
	(0.410)		(0.375)		(0.719)	
$\sum_p$Input*probability $_{y-3}$		1.222***		1.230***		1.230***
		(0.0568)		(0.0522)		(0.0483)
Level	Province	Country	Province	Country	Province	Country
Project type	All	All	ODA	ODA	Large	Large
Observations	6,296	452	6,296	452	6,296	452
Number of countries	91	90	91	90	91	90
Number of provinces	1,399	-	1,399	-	1,399	-
Country FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country-year FE	✓		✓		✓	
Province FE	✓		✓		✓	
F-stat	22.14	464.4	28.32	555.1	14.72	650.6

Notes: The dependent variable in panels A–C is the share of individuals that approve of the Chinese government in a given province/country, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The dependent variable in panel D and variable of interest in panels A–C is the number of Chinese development projects completed in the previous year ("Chinese projects $_{y-1}$"). Columns with project type "ODA" include only "Official Development Assistance-like" projects. Columns with project type "Large" include only projects with a size of US\$ 1 million or above. Columns with level "Province" ("Country") contain results of regressions at the province-year (country-year) level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

variable and the number of development projects completed. A two-standard deviation increase in *Input* (which is 0.6) creates an additional project in a province hosting projects in six years over the 2000–2014 period (the 10th percentile in the sample), but only by approximately 0.2 projects in a province hosting one project in the sample period (the median over the 2000–2014 period).[58]

At the level of provinces (columns 1, 3, and 5 of panel C), the coefficients stay negative but increase by an order of magnitude compared to the corresponding OLS estimate (and gain statistical significance at conventional levels). Measurement error, reverse causality, and omitted variables seem to conspire to bias our OLS coefficients upwards, therein highlighting the need for instrumentation. Each additional Chinese development project reduces public approval for the Chinese government by 7.22 percentage points (relative to any country-wide effects that we capture in country-year fixed effects). Reassuringly, the coefficients in the first-stage regression presented in panel D are highly significant and the first-stage Kleibergen-Paap F-statistics associated with the province-level regressions are considerably larger than the conventional rule-of-thumb value of 10.[59]

Several factors may explain why development projects yield popularity gains for the donor government in recipient countries and why they are less pronounced in the target provinces themselves.[60] We test possible explanations in the next subsection.

1.4.3 Testing Mechanisms

The existing literature suggests several mechanisms that may explain how aid affects public approval of a donor government. We test these mechanisms with country-level regressions following eq. 1.5 in panel A of Table 1.3, where we replace our dependent variable, $Support_{cy}$, with potential intermediate outcomes.

First, previous research shows that Chinese development projects lead to short-run economic growth through the creation of jobs and the spreading of economic activity (Bluhm et al. 2020, Guo and Jiang 2021, Dreher et al. 2022). Higher incomes in recipient-country populations lead to higher standards of living which may result in more satisfied citizens. In turn, this may result in gratitude towards the donor government. Rather than using aggregate measures of income or remote-sensing measures such as nighttime

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[58]Projects committed in earlier years can affect today's support of the Chinese government as well. To account for this, Appendix Table 1.D.7 estimates cumulative effects. Specifically, we use moving averages for both project numbers and our instrumental variables of up to five years. The coefficient size remains similar but is less precisely estimated with moving averages of several years. We discuss this in greater detail in Appendix Section 1.D.1.

[59]Specific values are 22 (all projects), 28 (ODA projects), and 15 (large projects). The first-stage F-statistics remain strong when we compute Kleibergen-Paap F-statistics that are robust to heteroskedasticity, autocorrelation, and clustering (Olea and Pflueger 2013).

[60]Note, however, that the province-level results are not robust to how we define Chinese development funding. As can be seen in Table 1.D.5, coefficients at the province level do not reach statistical significance when we replace the number of projects with a binary indicator for any project or measure it in (log) US$ amounts.

lights, we measure income at the household level. Specifically, we analyze the logged self-reported per capita annual income in international dollars in column 1 and perceived income on a 4-point scale that ranges from 1 ("Finding it very difficult on present income") to 4 ("Living comfortably on present income") in column 2. The results show that both actual and perceived income increase after the completion of Chinese development projects in recipient countries. An additional completed project increases the reported income by 0.4 percent and perceived income by 0.004 on the 4-point scale. Turning to perceived living standards, we find no significant overall effects on the perceived *change* of living standards (columns 3 and 4), but significantly more citizens report their living standard as good (column 5). In the year after a project's completion, citizens are 0.15 percent more likely to express that they have a good living standard. Although these quantitative effects are modest, this first set of results supports the idea that better income and living conditions are a channel that links Chinese development projects and support for the Chinese government.

Second, development projects might improve the delivery of public services in recipient countries. For example, foreign-financed education and health projects might improve social welfare, education outcomes, or health conditions of the recipient population (Martorano et al. 2020, Cruzatti C. et al. 2023). To test this, we use the Community Basics Index, which is the simple average of the share of respondents who express satisfaction with the following seven public amenities: education, healthcare, housing, water, air, roads, and public transport.[61] As the results in column 6 of Table 1.3 show, citizens are more likely to report satisfaction with community basics after the completion of Chinese projects. Public service provision thus appears to be a second channel that may link Chinese development projects and support for the Chinese government.

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[61]More precisely, the index is computed based on the answers to the following questions: "In the city or area where you live, are you satisfied or dissatisfied with [sector]?"—"educational system or the schools," "availability of quality healthcare," "availability of good affordable housing," "quality of water," "quality of air," "roads and highways," and "public transportation systems." Only respondents answering to all questions are included. We drop the answers "Don't Know" and "NA" before calculating the index.

Table 1.3 – Testing mechanisms, instrumental variable results

	(1) Income	(2) Perceived income	(3) Living std up	(4) Living std down	(5) Living std good	(6) Community basics	(7) Corruption	(8) Migration	(9) Environment
Panel A: Country level									
Chinese projects $_{y-1}$	0.00397*	0.00364**	0.000181	0.000454	0.00152**	0.000896**	-0.00118***	6.74e-05	-0.000435
	(0.00236)	(0.00147)	(0.000604)	(0.000786)	(0.000584)	(0.000415)	(0.000433)	(0.000399)	(0.000552)
Observations	417	481	472	472	484	462	470	474	473
Number of countries	90	95	94	94	95	93	94	94	94
Mean of dependent variable	7.419	2.455	0.448	0.249	0.532	0.562	0.784	0.599	0.494
F-stat	328.6	329.6	333.2	333.2	328.8	316.9	328.7	332.1	338
Panel B: Province level									
Chinese projects $_{y-1}$	0.225	-0.210**	-0.0619	0.0603*	-0.0408	0.0354	0.0231	0.0511	0.0203
	(0.200)	(0.0948)	(0.0395)	(0.0344)	(0.0464)	(0.0277)	(0.0377)	(0.0446)	(0.0480)
Observations	6,037	6,686	6,478	6,478	6,705	6,405	6,549	6,517	6,542
Number of countries	91	95	94	94	95	93	94	94	94
Number of provinces	1,415	1,465	1,439	1,439	1,465	1,444	1,443	1,456	1,448
Mean of dependent variable	7.540	2.476	0.433	0.249	0.543	0.577	0.799	0.590	0.495
Country-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
F-stat	5.037	21.61	21.37	21.37	21.97	12.18	22.59	23.74	21.22

Notes: This table shows regression results at the country level following eq. 1.5 and at the province level following eq. 1.3, where we change the dependent variable as indicated in the column header. "Income" is respondents' average logged self-reported per capita annual income in international dollars. "Perceived income" is respondents' average perception of household income on a scale from 1 ("Finding it very difficult on present income") to 4 ("Living comfortably on present income"). "Living std up (down)" measures the share of individuals that indicate that their standard of living is going up (down). "Living std good" measures the share of respondents satisfied with their current standard of living. "Community basics" is an index taken from the Gallup World Poll for everyday life in the community, including education, housing, and infrastructure (we do not include the responses "Don't know" and "NA"). "Corruption" is the share of respondents that replied "yes" to the following question: "Is corruption widespread within businesses located in (this country), or not?" "Migration" is the share of respondents that replied "yes" to the following question: "Is the city or area where you live a good place or not a good place to live for immigrants from other countries?" "Environment" is the share of respondents that replied "yes" to the following question: "In the city or area where you live, are you satisfied or dissatisfied with efforts to preserve the environment." All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

We also test three channels that might reduce approval rates of China's government. First, earlier research that focused on African countries finds evidence of increases in reported corruption around Chinese project sites (Brazys et al. 2017, Isaksson and Kotsadam 2018a). In stimulating economic activity and the availability of public and private goods, Chinese projects may increase demand for corruption. In turn, greater perceived corruption could lead to blame for China's government. On the other hand, improved living conditions might reduce individuals' willingness and opportunities to pay bribes.[62] To test these possibilities, we analyze the effects of Chinese project events on the average share of individuals that confirm widespread corruption within businesses in their respective country. In line with Dreher et al. (2022), we find evidence of *fewer* rather than more reports of corruption after the completion of Chinese projects. The absence of corruption-enhancing effects of Chinese development projects may also help explain their overall positive effects on Chinese government approval.

Second, popular accounts often suggest that many Chinese development projects rely heavily on Chinese rather than local labor (Cervellati et al. 2022). While a large share of this labor is used during the implementation stage of the project, some staff, like project managers and maintenance workers, may remain on the project after its completion. Potential resentment toward foreign labor in general, and reservations towards Chinese migrant workers in particular, might help explain negative sentiments towards the Chinese government. We test this by looking at citizens' attitudes towards migration (in column 8). More precisely, we compute the share of respondents that confirm that the city or area where they live is a good place for immigrants from other countries. We do not find evidence for a change in the attitudes towards migration. Again, the absence of anti-immigration sentiments may help explain the overall positive effects on Chinese government approval.

Finally, Chinese development activities might induce environmental degradation. Baehr et al. (2022) find that Chinese development projects lead to forest cover loss in Cambodia and Tanzania. Construction-heavy, large-scale infrastructure, mining, and energy projects are especially likely to cause damage to the natural environment in project regions through heavy use of soil and water, pollution, soil degradation, and deforestation. We measure satisfaction with environmental protection based on the question "In the city or area where you live, are you satisfied or dissatisfied with efforts to preserve the environment?" (in column 9). We do not observe significant changes in attitudes towards satisfaction with environmental protection in the aftermath of completed Chinese development projects. Environmental damage does not seem to harm the formation of more positive attitudes towards the Chinese government in recipient countries.

In panel B of Table 1.3, we report the corresponding province-level results following

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[62]Such positive effects of income and development on corruption are well documented (e.g., Paldam 2021).

eq. 1.3 for the same dependent variables. Again, these results have to be interpreted relative to the overall country effects (which are absorbed by the country-year fixed effects). We find that individuals living in project provinces feel *worse* off, controlling for country-level effects. They report their living standard to be deteriorating and experience greater difficulties to live on their current income. In contrast to Isaksson and Kotsadam (2018a), however, we do not find evidence for higher reported corruption in businesses or the government in project provinces. Attitudes towards migration also do not change and individuals living in project provinces do not report more or less satisfaction with communal or environmental services.

Deteriorating living standards relative to the country level might be one explanation for why the effect of Chinese development projects on popular support for the Chinese government is lower in provinces that host projects. However, the tests for mechanisms do not provide direct evidence for why this is the case. We suggest several possible explanations. One possibility is that project quality might be lower than originally expected, and this quality difference might be more strongly experienced locally in provinces with project sites.[63] Within provinces, individuals are able to compare their firsthand experiences and observations to their ex ante expectations, which might lead to negative ex post evaluations. The jobs, revenues, goods, and services generated by Chinese government-financed development projects might not meet the expectations that were created when the projects were announced or initiated. Mismatches between ex ante expectations and ex post evaluations are perhaps less common, or at least less striking, among individuals not living near project sites and lacking direct, visible exposure to project outcomes.

Alternatively, perceptions in provinces receiving Chinese projects might also be less positive because of fungibility. If Chinese government funding supports projects that would have otherwise been supported by a different financier, and if the alternative project was expected to be of a higher quality or greater importance to the people living in that area, individuals in project provinces might hold more negative views of the Chinese government in project areas compared to the country level (Cruzatti C. et al. 2023). The scale of our study and data availability make it difficult to pin down these channels. Future research could look specifically at identifying the mechanisms behind the differential province- and country-level effects in the short and longer run.

In short, though the effect on the support for China's government is lower in project provinces, we find overall positive, country-level public opinion gains for China's government. This latter finding is arguably more consequential for donors looking to use aid as a soft power instrument. However, to comprehensively gauge whether aid promotes foreign support for donor governments, one must also consider whether development

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[63]Relatedly, negative spillovers—such as increased levels of corruption and environmental degradation—might be greater than originally anticipated. These points can also explain the difference in effects that we find between the short and longer run.

projects shape attitudes beyond the host country. The following section thus turns to third-country and global effects of Chinese development projects.

1.5 Perceptions of the Chinese Government in the Global South

A net assessment of the potential soft power effects of aid must also account for how development activities are perceived in countries other than the recipient country itself. As such, in addition to provincial- and national-level tests discussed above, we also consider whether Chinese development projects shape attitudes towards the Chinese government at the global level. Development projects do not exist in a vacuum and can often be observed and assessed by individuals who live far away from where projects take place. These observations do not stop at national borders and information about individual development projects often resonates with international audiences. Of course, in the process of doing so, this information can be repackaged, distorted, and presented differently to different audiences. Sri Lanka's notorious Hambantota Port, the "birthplace" of the "debt trap diplomacy" narrative, offers perhaps the best-known illustration in the context of the BRI (Brautigam 2020).

International, third-country reactions to overseas development projects are important for donor governments. While donors hope to win hearts and minds within the national and subnational jurisdictions where they finance development projects, they also care about how such activities are viewed on a global scale. We therefore analyze whether our findings at the country level translate into global support for the Chinese government. If yes, then earlier research only considering local or national public opinion effects might underestimate the attitudinal gains of Chinese development finance for China's governments. If not, the positive recipient-country effects might instead be offset by negative global reactions to Chinese financing from a global soft power perspective.

We test the public opinion effects of Chinese development projects at the global level relying on the macro-level instrumental-variables framework.[64] To this end, we run a modified version of eqs. 1.4 and 1.5 and estimate the effects of projects completed anywhere around the world (rather than only in the recipient country itself) on the country-level approval rate of the Chinese government. We aggregate the predicted number of projects in all provinces in a year to the global level. Since these aggregates are a time series without cross-sectional variation, we omit year-fixed effects as the latter would be perfectly correlated with the predicted number of projects worldwide.[65]

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[64]We focus on the longer term, as our micro-level analysis does not allow identifying the effects of (many) events that hit all countries globally due to the lack of an adequate control group. In addition, we believe that the longer term is more relevant for analyzing global soft power.

[65]Alternatively, one might consider to directly estimate 2SLS regressions at the global level. Given

Table 1.4 shows the results. As can be seen in column 1 of panel A, projects completed globally do not affect public approval of the Chinese government. The positive effects we find at the country level are either not sufficiently large to affect global public opinion about the Chinese government or positive effects are cancelled out by negative opinions about development finance elsewhere. Either way, China's development finance does not significantly affect global approval of its government. The remaining columns of Table 1.4 focus on subsets of countries that we consider to be of particular importance for soft power acquisition to either China's government or its competitors for global influence: recipients of China's aid in the sample period (column 2), frequent recipients of China's aid (defined as countries that receive aid in more years than the median value of three, column 3), the world's least developed countries (column 4), countries in Asia (column 5), countries in Africa (column 6), countries with a low voting alignment with China in the UN General Assembly (defined as the lowest tercile of voting similarity, column 7), with a middle voting alignment (column 8), and high voting alignment (column 9), and countries with a low, middle, or high approval of the Chinese government (again defined as terciles, of the average approval rate over the sample period, in columns 10–12).

The results show positive and significant effects for the samples of African countries, politically neutral countries in terms of UN voting alignment, and countries with an already high level of approval of the Chinese government. By contrast, countries with an already low opinion of the Chinese government show a further reduction in their approval rate in response to Chinese global development finance activities. This suggests that opinions about the Chinese government become more polarized as China provides more projects. Quantitatively, 51 additional projects in the previous year improve support for the Chinese government on the African continent by one percentage point. Based on the median number of new projects per year worldwide (of 122) this estimate implies an aid-induced increase by more than 2.2 percentage points. Likewise, 56 additional projects in the previous year improve support for the Chinese government in "swing states"—those that are in the middle tercile according to their voting alignment with China in the United Nations—by one percentage point. The same holds for 54 projects in countries with a high ex ante approval of the Chinese government. The negative effect in countries that disapprove of the Chinese government is slightly stronger in absolute terms, with 42 projects leading to an additional percentage point of disapproval. It thus seems that China's development projects increase its popular approval in three important subsets of countries.[66]

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that input materials would then only vary over time—and fixed effects for years would thus have to be excluded—this approach arguably violates the exclusion restriction (many variables that vary from year to year are likely to be correlated with input materials and approval of the Chinese government). For what it is worth, panel C of Appendix Table 1.D.3 shows that when we do so, coefficients are positive and insignificant, in line with the average results we obtain in our main analysis based on aggregated predicted projects.

[66]In Africa, China seeks to pursue natural resources and access to markets (Dreher et al. 2018b);

Our global analysis also considers whether projects given to a particular set of countries affect public opinion differentially. The further panels of Table 1.4 investigate whether public perceptions of Chinese development projects implemented abroad depend on who receives the projects. More precisely, panels B, C, and D investigate whether projects affect public opinion more strongly when they are given to countries 'closer' to one's own. In panel B, we therefore give larger weight to projects completed in countries with a greater similarity in the voting behavior in the United Nations General Assembly (UNGA), using an index provided by Voeten et al. (2009).[67] Panel C examines geographically proximate countries, measured by the inverted logarithm of the population-weighted geographic distance between two countries (data from Mayer and Zignago 2011). Panel D focuses on ethnic similarity using the inverse distance-adjusted ethno-linguistic fractionalization index on dissimilarity in ethno-racial characteristics (Kolo 2012). Again, we do this for the global sample in column 1 as well as all subsamples in columns 2–12.[68]

The results show no significant effect of the completion of Chinese development projects on public opinion globally, independent of how we weight projects (column 1). However, if China values soft power among a certain target group of countries, such as its (frequent) aid recipients, African recipients, or Asian recipients (columns 2–3 and 5–6), using development projects as a soft power tool can pay off: Aid creates support for the Chinese government in third countries that are politically aligned with the recipient country. Development projects also create a more favorable opinion in politically aligned third countries that are either politically neutral or China's friends (columns 8–9). When looking at the impact of Chinese development projects in third countries that are either geographically or ethnically proximate to the recipient country, results are mixed. For example, while China can attract neutral countries through projects in ethnically aligned third countries (column 8), such projects further reduce support in low-opinion countries if projects are carried out in Asia and ethnically proximate countries (columns 5 and 10).

Taken together, Chinese development finance leads to soft power gains in important third countries when these are politically aligned with the recipient country. This provides the Chinese government with a tool to simultaneously enhance its influence among groups of aligned countries. In the next section, we examine if Chinese development finance also

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swing states in the United Nations General Assembly are those likely to be most accessible to exchange a specific vote for aid (Fuchs et al. 2015); and countries with high approval are more likely to be allies of China than those where approval rates are low.

[67]We use the S score which measures the voting similarity between two countries in a given session. It is calculated as $S_{ab} = 1 - (\sum |Y_{av} - Y_{bv}| \ / \ V)$, where $v = 1, ..., V$ indexes votes, a and b refer to two countries, and Y refers to votes, taking on one of three alternatives: yea ($Y = 1$), abstain ($Y = 2$), and nay ($Y = 3$). An S score of 1 means complete agreement, a score of -1 total disagreement between the two countries. Also see Bailey et al. (2017) for further details.

[68]These regressions include the control variables age, age squared, gender, education, and urban, as well as fixed effects for countries and years. The inclusion of year-fixed effects affects the interpretation of our results, given that they capture any average effects the projects might have on public opinion globally. We also control for projects given to the country of interview.

boosts popular support for recipient governments.

Table 1.4 – Chinese projects and government support at the global level, instrumental variables results

	(1) All	(2) Aid recipient	(3) Freq. recipient	(4) LDC	(5) Asia	(6) Africa	(7) Political foes	(8) Political neutral	(9) Political friends	(10) Opinion: low	(11) Opinion: neutral	(12) Opinion: high
						Panel A: Global South						
Chinese projects worldwide $_{y-1}$	7.71e-05	6.23e-05	0.000115	0.000160	-0.000145	0.000196**	1.07e-05	0.000180*	3.04e-05	-0.000236**	0.000199	0.000186**
	(6.38e-05)	(7.12e-05)	(9.26e-05)	(0.000103)	(0.000166)	(7.76e-05)	(0.000136)	(0.000102)	(0.000106)	(0.000113)	(0.000133)	(7.75e-05)
Observations	452	388	264	170	119	160	66	177	200	136	154	162
				Panel B: Third country (projects weighted by political proximity to recipient country)								
Chinese projects abroad $_{y-1}$	0.00122	0.00200**	0.00324***	0.000291	0.00407**	0.00478**	-0.00317	0.00239*	0.00438***	-0.00149	0.000119	0.00125
	(0.000777)	(0.000906)	(0.00110)	(0.00155)	(0.00156)	(0.00185)	(0.00226)	(0.00134)	(0.00133)	(0.00185)	(0.00118)	(0.000785)
Observations	443	382	257	169	113	160	64	177	200	126	154	161
				Panel C: Third country (projects weighted by geographic proximity to recipient country)								
Chinese projects abroad $_{y-1}$	0.00962	0.0104	-0.00566	0.00774	-0.109**	0.0298	0.0568**	0.0175	-0.00376	-0.0247	0.0303	0.00838
	(0.0140)	(0.0162)	(0.0234)	(0.0264)	(0.0491)	(0.0198)	(0.0221)	(0.0210)	(0.0200)	(0.0302)	(0.0292)	(0.0169)
Observations	449	388	263	169	119	160	64	177	200	132	154	161
				Panel D: Third country (projects weighted by ethnic proximity to recipient country)								
Chinese projects abroad $_{y-1}$	7.57e-05	0.000101	9.80e-05	5.27e-05	-0.00183***	0.000131	-0.000101	0.000189*	8.55e-05	-0.000478*	-6.42e-05	6.81e-05
	(6.62e-05)	(6.85e-05)	(9.17e-05)	(0.000114)	(0.000576)	(9.02e-05)	(0.000322)	(0.000102)	(9.60e-05)	(0.000275)	(0.000140)	(0.000101)
Observations	449	388	263	169	119	160	64	177	200	132	154	161

Notes: This table reports the results of the global analysis. Panel A shows the results from eq. 1.5, where we aggregate the number of projects to a time series at the global level. Column 1 ("All") reports the results for all countries in the sample. Columns 2–12 restrict the sample to subsets of countries. "Aid recipient" includes countries that have received any Chinese aid in the sample period. "Freq. recipient" includes countries that received aid in more than three years (median). "LDC" includes the Least Developed Countries according to the World Bank's income categories. "Asia" and "Africa" include countries from the respective continent. "Political foes" ("Political neutral," "Political friends") include those countries in the lowest (middle/highest) tercile of mean political agreement with China in the UN General Assembly. "Opinion: low" ("Opinion: neutral," "Opinion: high") are those countries in the lowest (middle/highest) tercile of mean approval of the Chinese government. Panels B–D report results for the number of projects completed in third countries ("Chinese projects abroad $_{y-1}$"). We aggregate the province-level predictions from eq. 1.3 to the global level, excluding the country of interview. Projects are weighted using the following spatial weights: "Political proximity" ("Geographic proximity"/ "Ethnic proximity") weigh the number of projects completed abroad by similarity in terms of UNGA voting alignment, geographic distance, and ethnic similarity. All specifications include the control variables age, age squared, gender, education, and urban. Panel A includes country-fixed effects. Panels B–D include country- and year-fixed effects and control for the number of projects completed in the country of interview. Kleibergen-Paap F-statistics are above 10 in all specifications. Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

1.6 Is Aid "Win-Win Cooperation" for Donor and Host Governments?

In addition to attitudes toward donors, the completion of development projects may impact citizens' views of their own government. Chinese development finance is often framed as "win-win cooperation" by the Chinese government, and if this principle holds true, we might also expect host governments to gain popularity for securing and implementing Chinese-financed projects. This contrasts with the conventional wisdom, which suggests that foreign aid may actually undermine state legitimacy. Many scholars have argued that when governments rely upon foreign aid (or other sources of "unearned income"), they are insulated from the consequences of poor policy decisions and more easily able to ignore taxpayers' preferences, effectively short-circuiting the accountability relationship between the governor and the governed (Djankov et al. 2008, Smith 2008, Bueno de Mesquita and Smith 2010). However, a new wave of empirical research calls the conventional wisdom into question. Dietrich and Winters (2015), Dietrich et al. (2018), and Blair and Roessler (2021) provide experimental evidence that the local receipt of foreign aid actually improves trust in government and citizen perceptions of the state. Marx (2018) finds that incumbent governments benefit from the implementation of aid projects. Similar effects in the case of Chinese development finance would be in line with the Chinese government's claims of "win-win cooperation" from the perspective of recipient country governments.

Table 1.5 reports results of tests for whether Chinese development projects affect citizens' support of their own government. We show results for both the micro and macro level, changing the dependent variable to the approval of the national government of the recipient country.[69] Column 1 reports results for our event study based on eq. 1.1. Column 2 is based on the same equation, but includes a binary variable indicating the project province. Columns 3 and 4 report the macro-level results at the country level based on eq. 1.5 and the province level based on eq. 1.3, respectively.

Starting with the event-study results, we observe positive public opinion effects for the national government in project provinces (column 2) but not elsewhere in the country (column 1). Government approval rises by 11.6 percentage points after the completion of a Chinese project in the project province. Comparing these effects with those on Chinese government approval, it appears that short-term popularity gains for the national government are locally constrained, whereas support for the Chinese government improves countrywide. This is an important result for governments that seek to increase their support base in the short run, e.g., prior to elections.

Turning to longer-term public opinion effects, we present macro-level results in columns

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[69]Specifically, government approval is based on the question "Do you approve or disapprove the job performance of the (leader/head/president) of this country?"

3 and 4. Approval rates of one's own government improve with the number of Chinese projects completed, both at the province and country level. An additional project increases the approval rate of the national government by 0.2 percentage points. While this effect is small compared to the sample average of 54.5 percent, it is comparable in size to the effect on the approval of the Chinese government as reported in Table 1.2. At the province level, the results are stronger with an additional Chinese project leading to a popularity gain of 9.9 percent. It indeed appears that Chinese development projects are "win-win" for both the Chinese government and the national governments of recipient countries, at least in the year following project completion.

Table 1.5 – Chinese projects and approval of the national government

	(1) Short-term country	(2) Short-term province	(3) Longer-term country	(4) Longer-term province
Post/project completed	0.0115 (0.0178)	-0.00148 (0.0180)	0.00210*** (0.000791)	0.0766* (0.0432)
Post*project province		0.116*** (0.0437)		
Observations	18994	18994	420	5,920
Number of countries	19	19	86	86
Number of provinces	248	248	-	1325
Number of projects	21	21	-	-
F-stat	-	-	341	20.26
Province-year FE	✓	✓	.	.
Country FE	✓	✓	✓	.
Year FE	✓	✓	✓	.
Country-year FE	✓	✓	.	✓
Province FE	✓	✓	.	✓

Notes: This table reports results for the effect of Chinese development projects on government approval in recipient countries. Columns 1 and 2 report short-term results from eq. 1.1. The dependent variable is binary and indicates whether or not an interviewed individual approves of the national government based on the question "Do you approve or disapprove the job performance of the (leader/head/president) of this country?" The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. Columns 3 and 4 report the longer-term results based on eq. 1.3 and eq. 1.5. The dependent variable is the share of individuals that approve of the national government in a given province/country, based on the same question. The variable of interest is the number of Chinese development projects completed in the previous year. Standard errors are clustered by country-date in columns 1 and 2, and by country in columns 3 and 4: *** p<0.01, ** p<0.05, * p<0.1.

1.7 Conclusion

This study provides a comprehensive analysis of whether and how foreign aid shapes public support for donor governments in developing countries. To address this question, we

created a new dataset of Chinese development project events (including precise project commitment, start, and end dates) and merged these data with granular, time-stamped data from Gallup World Poll in 126 countries and over 2,000 subnational jurisdictions. We chose to focus on Chinese development finance and its effects on foreign popular support for Beijing for several reasons: China's growing importance as an international donor and lender, its active pursuit of soft power, and the availability of high-quality, granular data on treatments and outcomes. Unlike previous analyses that only measure localized, short-run public opinion impacts near project sites, our approach provides a comprehensive picture of the public opinion effects of Chinese development projects by considering different time horizons and levels of geographical aggregation (provinces, countries, and groups of countries). Our analysis is underpinned by two causal identification strategies: a short-term event study wherein the timing of interviews can be considered as random relative to Chinese project events, and an annual analysis using an established instrumental-variables approach that exploits changes in China's overall production of project inputs and the differential effect of these shocks on different countries and subnational jurisdictions.

Our results show that, on average, public approval of the Chinese government increases in countries where Chinese development projects are completed. In the short run, this effect increases with the size of the project and the generosity of the financial commitment; in the longer run, it is lower among people who live in close proximity to completed Chinese development projects. Perhaps most importantly, we find that these development projects create a more favorable public opinion environment for China among countries in Africa, potential "swing states" in the United Nations General Assembly, and countries with higher baseline (ex ante) levels of public support for the Chinese government. We also find that Chinese development finance leads to soft power gains in important third countries—when these are politically aligned with the recipient country. These positive spillover effects allow the Chinese government to simultaneously enhance its public profile among groups of politically aligned countries. In short, we find that China's overseas development program enhances its soft power.

At the same time, our findings help clarify a key source of confusion among policymakers, journalists, and scholars: whether the Belt and Road Initiative (BRI) is a reputational asset or liability. The existing debate about the BRI suggests that its public opinion impacts are either overwhelmingly positive or negative. One camp argues that Beijing is gaining the upper hand in a zero-sum, great power competition for international influence by bankrolling big-ticket infrastructure projects that Western powers are unwilling to support. Another camp claims that Beijing is losing the battle for "hearts and minds" because of local exposure to the negative unintended consequences of Chinese development projects. Our findings suggest that neither camp is entirely right—or wrong. Consistent with previous studies, we find that individuals who live near completed Chi-

nese development projects form less favorable views of China (than individuals from the same country in the same year). However, unlike previous studies, we measure the public opinion impacts of Chinese development projects *outside* of the subnational jurisdictions where such projects take place. Our findings indicate that indirect treatment exposure improves public sentiment towards China. These countervailing effects among individuals who experience direct treatment exposure and individuals who experience indirect treatment exposure appear to be a "net positive" at the country level, creating a more favorable public opinion environment for China.[70]

The fact that those who are most directly affected by the costs and benefits of Chinese development projects develop less favorable views of the Chinese government, as compared to the average effect in the same country and year, also helps explain why there are still many signs of BRI "backlash" around the globe.[71] In this regard, our study complements previous studies, which provide evidence that those who live in close proximity to Chinese development projects have higher levels of exposure to various negative externalities—including political capture (Dreher et al. 2019, Anaxagorou et al. 2020), corruption (Isaksson and Kotsadam 2018a), ethnic tensions (Isaksson 2020), public protests (Iacoella et al. 2021), environmental degradation (Baehr et al. 2022), and declining rates of labor union involvement (Isaksson and Kotsadam 2018b).

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[70]That being said, a limitation of our study is that it does not capture the public opinion impacts of Chinese government-financed development projects in the Global North.

[71]Malik et al. (2021) find that at least 35% of the BRI infrastructure project portfolio has encountered major implementation problems, such as corruption scandals, labor law violations, and public protests.

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Appendices

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1.A Codebook for Chinese Development Project Dates Collection

This appendix summarizes the method we have used to gather specific commitment, start, and end dates of Chinese-financed development projects included in AidData's Geocoded Global Chinese Official Finance Dataset (version 1.1.1, Bluhm et al. 2020, Dreher et al. 2022), which is based on AidData's Global Chinese Official Finance Dataset (version 1.0) created with AidData's TUFF 1.3 methodology (Dreher et al. 2021b, Dreher et al. 2022). We outline the additional variables we have coded below. We have organized data collection instructions into general data collection principles and specific procedures that include detailed step-by-step coder instructions (largely omitted from this appendix). Readers interested in the full methodology document, which includes several detailed coding examples, may contact the authors.

1.A.1 Date Variables

When coding, use the date format "mm/dd/yyyy."

Commitment dates
The commitment date indicates a project was officially agreed upon and announced by the recipient and/or the donor government.

- Commitment date (*cdate*):

  – The date that a donor country and a recipient country reach agreement and exchange letters (换).

  – The date that a contract for future projects was signed.

  – The date that a construction company wins the bid of a project (中标).

  – The date that government authorities publicly announce a project or show commitment for a yet-to-begin project during a meeting report or a speech.

Note: While we provide coding definitions for commitment dates above, we did not search for commitment dates comprehensively and instead focused on start and end dates during our own research. The reason for this is that commitment dates have proven to be more difficult to effectively capture using the open-source methods we have developed.

Start dates

The start date indicates the start of the project's construction or groundbreaking ceremony. We separate start dates into opening ceremonies, start of implementation, and others to improve coding accuracy:

- Ceremonial date (*sdate_ceremony*):

  - Opening or groundbreaking ceremonies that signal the start of construction (in Chinese, often 开工 or 动工仪式).

  - Significant political or social figures appear at an event and signal the start of the project. An example is officials laying the first stone on a bridge before construction or officials giving a sign to signal the project is about to start. (揭牌仪式).

- Actual implementation date (*sdate_implementation*):

  - The date that donations, supplies, loans, or equipments are given (捐赠).

  - The date that performing teams, scholarships, or long-term technical (educational, medical) support are sent to countries in need.

  - The date that actual construction of a project, such as bridge, stadium, or building, started.

- Other (*sdate_other*):

  - If the data do not fit the above categories, note why in the variable *note_sdate*.

Note: If the start date found fits into more than one of the above categories, put the date in all relevant categories. For example, the actual construction begins at the opening ceremony on 05/06/2007, code 05/06/2007 for both opening ceremony and implementation.

End dates

The end date includes official completion dates, acceptance dates, end of implementation, start of utilization, and others:

- Completion ceremony (*edate_ceremony*):

  - The date that a ceremony was held to signal a project's completion (竣工仪式).

- Acceptance/Inspection date (*edate_acceptance*):

    - The date that a project passed inspection (验收).

- Actual implementation end date (*edate_implementation*):

    - The date that donations, supplies, loans, or equipment are received (捐赠).

    - The date that the donor country handed over a finished project to the receiver country (交接仪式).

    - The date that performing (medical) teams, scholarships, or long-term technical (educational) support finished assisting and leave the recipient country.

- Utilization date (*edate_utilization*):

    - The date that a project was fully finished and put into use (投入使用).

- Other (*edate other*):

    - If the data do not fit the above categories, make notes in the variable *note_sdate*.

Note: If the end date found fits into more than one of the above categories, we put the date in all relevant categories. For example, if a project was first put into use at the completion ceremony at 05/06/2007, code 05/06/2007 in both "end ceremony" and "start of utilization."

1.A.2 Searching for Dates Information with Online Search Engines

We used the search engine Baidu first and changed to Google if open-source project date information does not appear readily available for a given development project. Search results can vary significantly across search engines.

1.A.3 Specific Procedures

The coding steps below have been repeatedly tested by multiple coders and have proven to be efficient for collecting dates information on Chinese-financed development projects. Following this set of steps can potentially optimize coding workflow.

- Open Excel, click view, and freeze panes to keep the variables on display when scrolling down a worksheet.

- Open Google translation page, `https://translate.google.com/`, and translate the title of the project into Chinese.

- Open Baidu, enter China (中国), recipient country's Chinese name, a broad description of the project's name, and year.

1.B   Additional Figures and Tables

Figure 1.B.1 – Coverage of Chinese project dates over time, 2006–2017
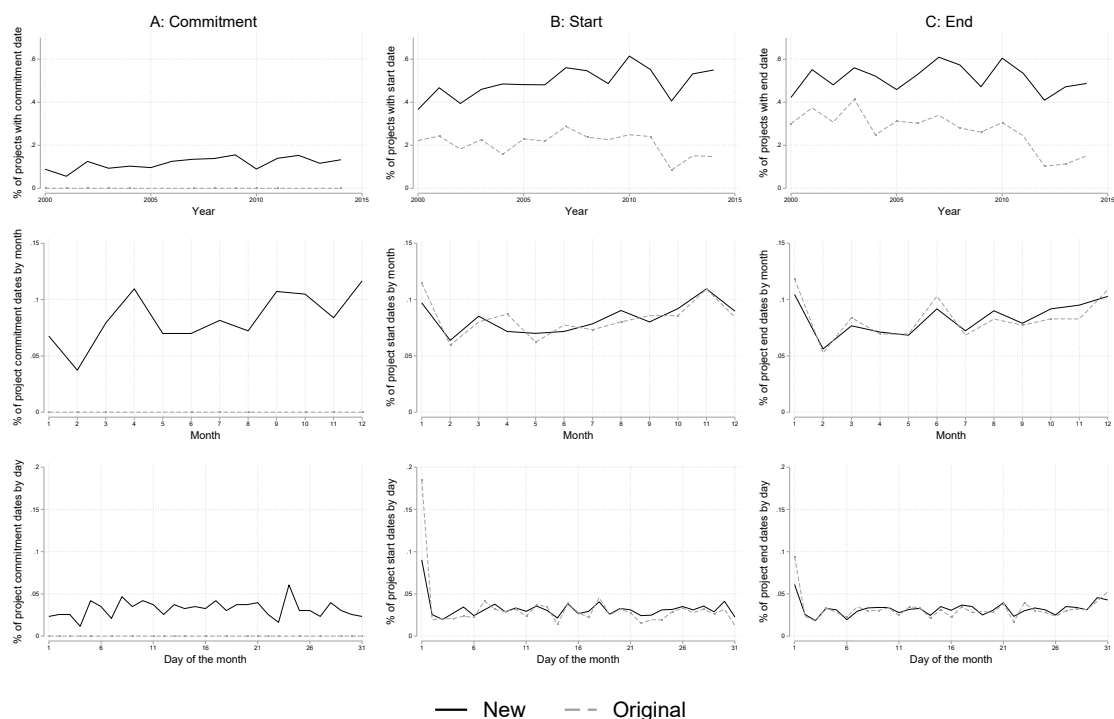
Notes: This figure shows the share of China's development projects for which we have information on commitment, start, and end dates. "New" refers to the dataset we provide in this study. "Original" refers to information available on project-level events in AidData's Geocoded Global Chinese Development Finance Dataset (version 1.1.1, Bluhm et al. 2020, Dreher et al. 2022). The top row shows the percentage of projects with information on the day of project commitment, start, and end by commitment year (note that no information on commitment dates is available in the original data). The middle and bottom rows show the percentage of projects committed, started, and ended per month of the year and day of the month, respectively.

Figure 1.B.2 – Distribution of Gallup World Poll interview dates over time, 2006–2017
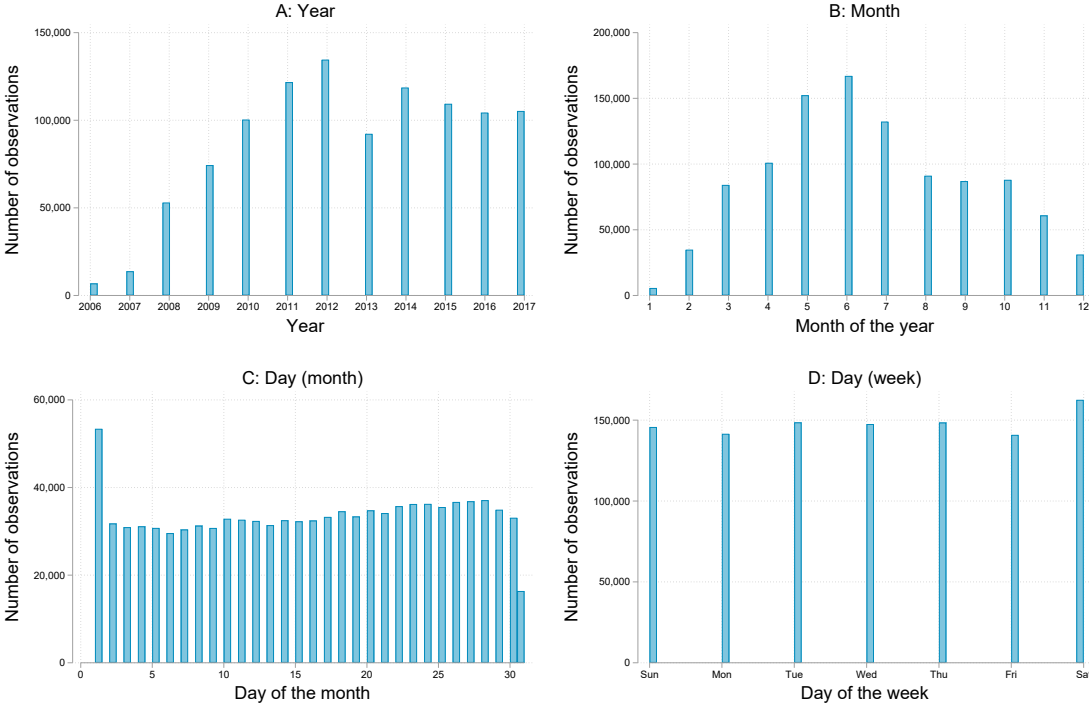
Notes: The figure provides an overview of the distribution of Gallup World Poll interview dates over surveyed years (panel A), months of the year (panel B), days of the month (panel C) and days of the week (panel D).

Figure 1.B.3 – Average duration of Chinese projects, 2006–2017
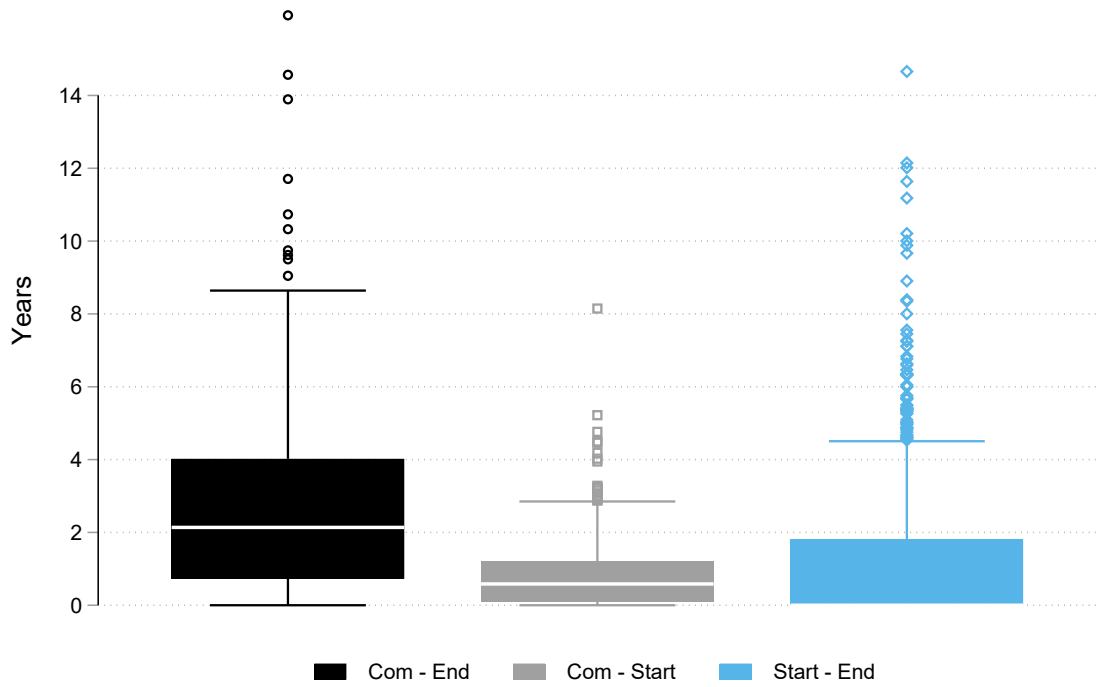
Notes: The figure displays conventional boxplots indicating the time between events for each project in years. 25th percentile, median, and 75th percentile are displayed by the boxes, the whiskers display the upper and lower adjacent values, and dots mark outliers. "Com-End" refers to the time between project commitment and completion, "Com-Start" to the time between project commitment and start, and "Start-End" to the time between start and end date. We exclude projects for which the relevant information is missing.

Figure 1.B.4 – Average duration of Chinese projects by sector, 2006–2017
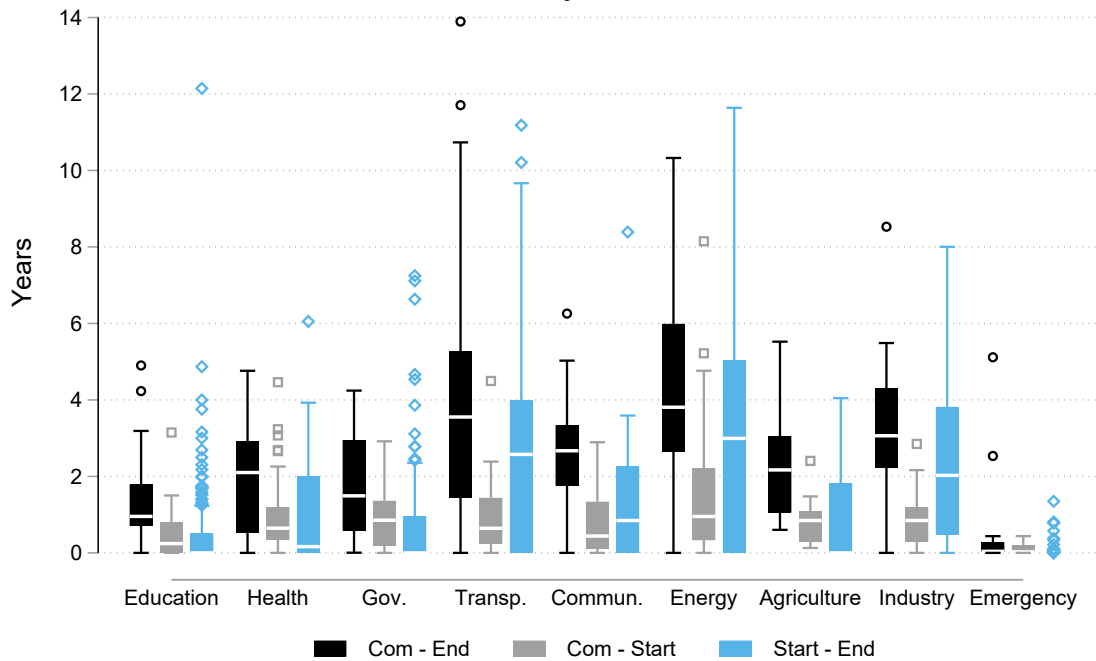
Notes: The figure displays conventional boxplots indicating the time between events for each project in years by sector for the nine sectors that received most projects. 25th percentile, median, and 75th percentile are displayed by the boxes, the whiskers display the upper and lower adjacent values, and dots mark outliers. "Com-End" refers to the time between project commitment and completion, "Com-Start" to the time between project commitment and start, and "Start-End" to the time between start and end date. We exclude projects for which the relevant information is missing.

Figure 1.B.5 – Chinese projects and support for the Chinese government, event study plots
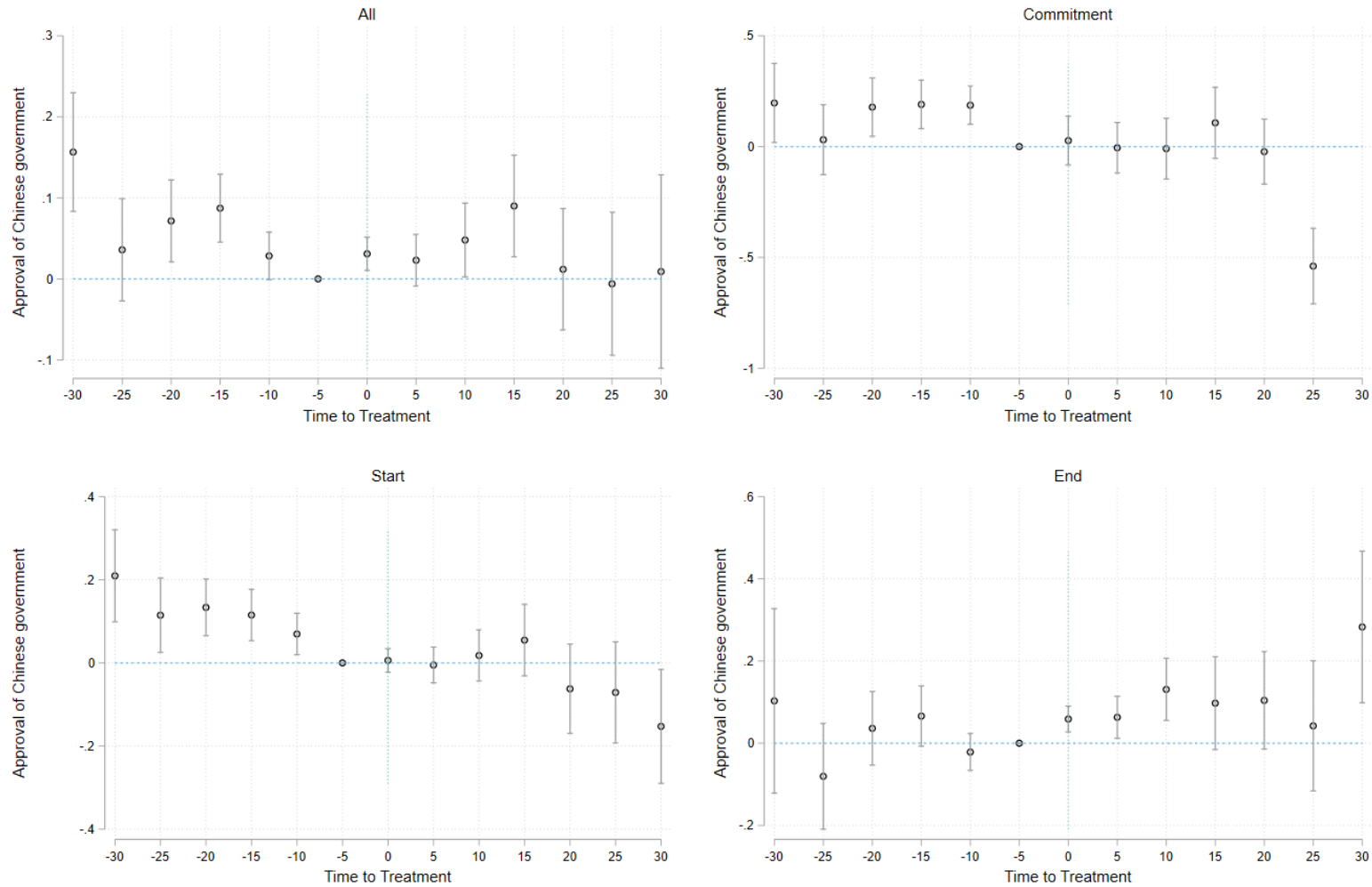
Notes: The figure displays the coefficients and the 90 percent confidence intervals for 13 binary variables indicating 5-day blocks from 30 days before to 30 days after a Chinese project event. The coefficient for the period between 5 to 1 days before the event is normalized to 0. Confidence intervals are based on heteroskedasticity-robust standard errors clustered by country×date.

Figure 1.B.6 − Randomization inference test, event study results
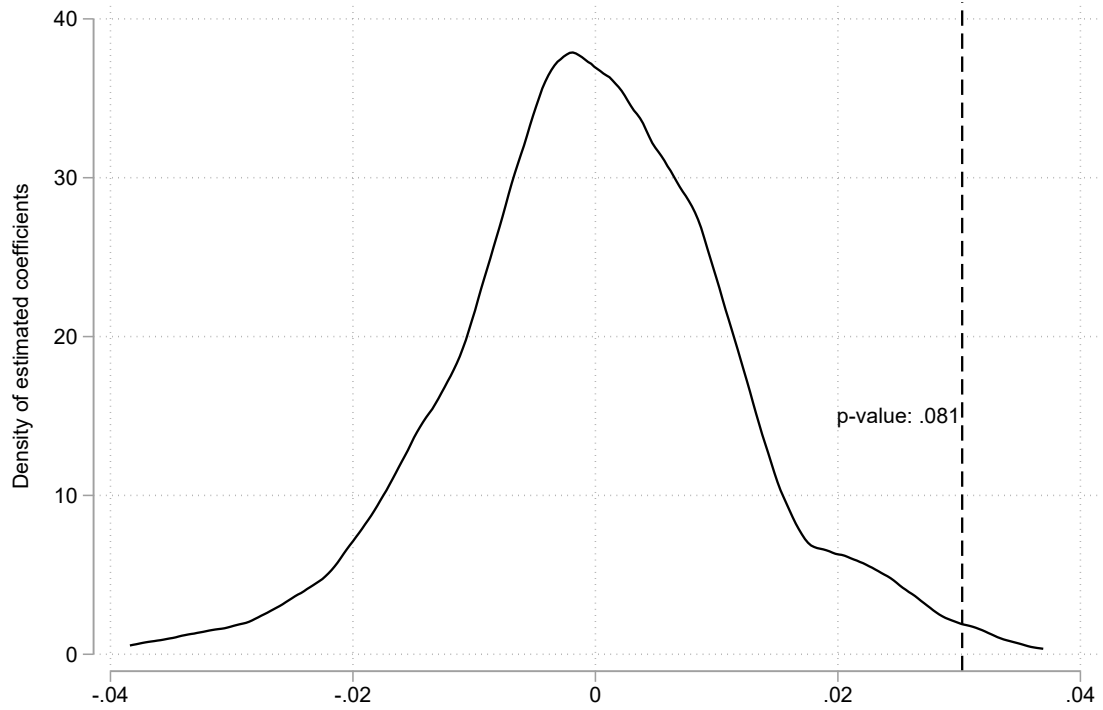
Notes: The figure shows the randomization inference test based on 999 Monte Carlo replications for the event study analysis. For this, we first draw 22 60-day event windows randomly from the GWP, and then draw one random date from each of these 22 windows that we use as placebo treatment. The original estimate for project completion from column 4 (Panel A) of Table 1.1 is shown by the dashed vertical line. The p-value is the proportion of times that the absolute value of the t-statistic in the simulated data exceeds the absolute value of the original t-statistic.

Figure 1.B.7 – Project inputs, Chinese projects, and support for the Chinese government, 2006–2014
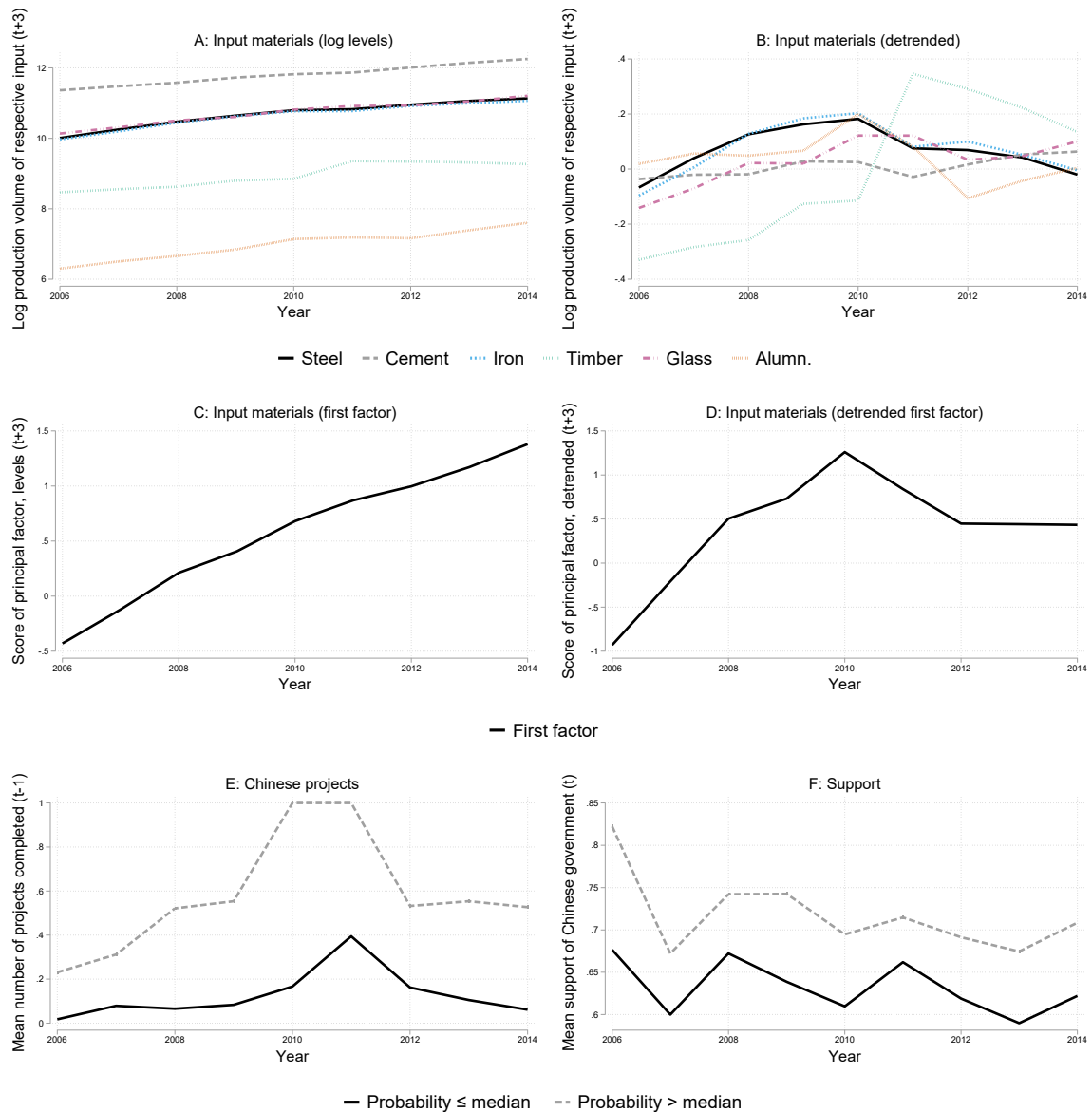
Notes: The figures show the time series of Chinese input material production in logs (panel A), detrended logged production (panel B), the first factor of these input materials (panel C), the detrended first factor (panel D), the average number of completed Chinese development projects in recipient provinces grouped by the median probability of receiving projects over the sample period (panel E), and the average support of the Chinese government grouped by the median probability of receiving projects over the sample period (panel F).

Figure 1.B.8 – Randomization inference test, instrumental variables results
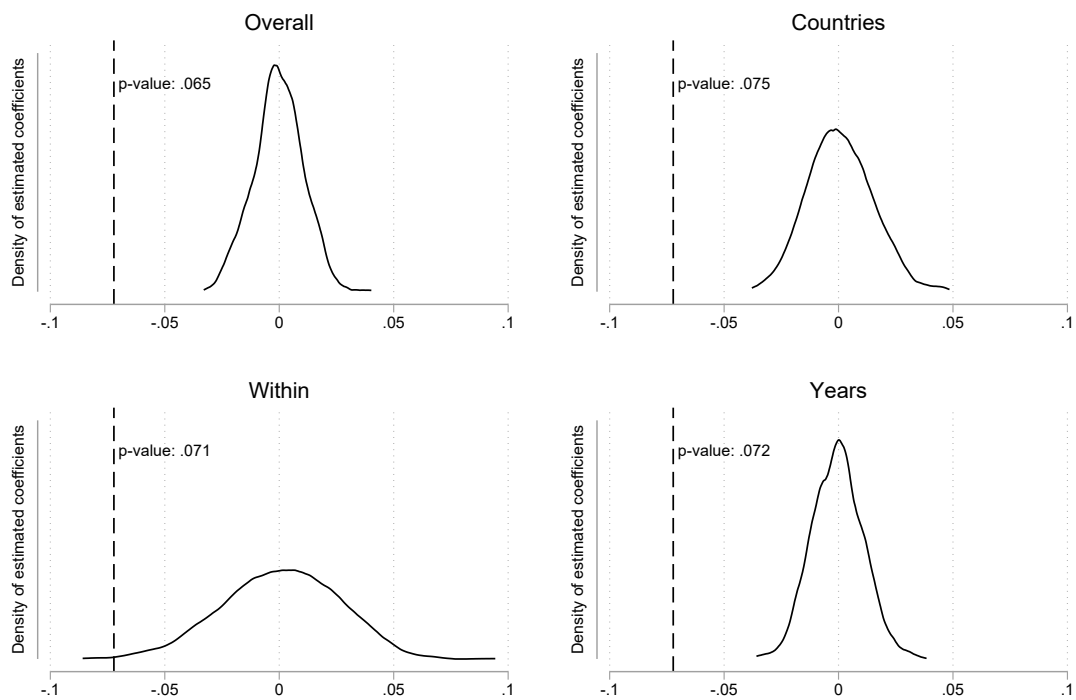
Notes: The figure shows the distribution of point coefficients of the completion of Chinese development projects based on 999 Monte Carlo replications under different randomization inference tests. "Overall" swaps the number of projects completed and the instrument for all observations, "Countries" swaps the entire time series between countries, "Within" swaps years within countries, and "Years" swaps countries within years. The original estimate from column 1 of Table 1.2 is shown by dashed vertical lines. The p-values are calculated as the proportion of times that the absolute value of the t-statistics in the simulated data exceed the absolute value of the original t-statistic.

71

Table 1.B.1 – List of countries

Afghanistan	*Guinea*	Northern Cyprus
Algeria	Haiti	*Pakistan*
Angola	Honduras	Palestina
Argentina	*India*	Panama
Armenia	*Indonesia*	Paraguay
Azerbaijan	Iran	Peru
Bangladesh	Iraq	*Republic of Congo*
Belarus	*Israel*	Romania
Bolivia	Ivory Coast	Russia
Brazil	*Kazakhstan*	Rwanda
Bulgaria	Kyrgyzstan	*Senegal*
Burkina Faso	*Laos*	*Sierra Leone*
Burundi	Latvia	*Somalia*
Cambodia	*Lebanon*	South Africa
Cameroon	*Liberia*	*Sudan*
Central African Republic	Lithuania	*Suriname*
Chad	*Madagascar*	Syria
Chile	Malaysia	Tajikistan
Colombia	*Mali*	Tanzania
Comoros	Mauritania	Thailand
Costa Rica	*Mauritius*	*Togo*
Dem. Rep. of the Congo	Mexico	Tunisia
Djibouti	Moldova	Turkey
Dominican Republic	Mongolia	Turkmenistan
Ecuador	Morocco	Ukraine
Egypt	Mozambique	Uruguay
El Salvador	Myanmar	*Uzbekistan*
Ethiopia	*Namibia*	*Venezuela*
Gabon	Nepal	*Vietnam*
Georgia	Nicaragua	Yemen
Ghana	Niger	*Zambia*
Guatemala	*Nigeria*	*Zimbabwe*

Notes: The table lists all countries and territories included in our regression analysis. For all countries, we map the Gallup spatial identifiers with the subnational regions at the ADM1 level from GADM. The list includes the 34 countries included in our micro-level event study (*in italics*), and the 91 countries in our macro-level analysis, totalling 96 countries. Israel, Namibia, Somalia, Sudan, and Suriname only feature in the micro-level analysis.

Table 1.B.2 – Individual-level event study: Project representativeness, 2006–2017

	(1) Commitment	(2) Start	(3) End	(4) Commitment	(5) Start	(6) End
ODA	-0.0231	-0.000805	0.000762	-0.00583**	-0.000290	0.000953
	(0.0165)	(0.00651)	(0.00679)	(0.00227)	(0.00329)	(0.00337)
Social infrastructure	0.00718	0.00448	-0.00141	0.00101	0.00229	-0.00101
	(0.0247)	(0.00723)	(0.00718)	(0.00257)	(0.00372)	(0.00382)
Economic infrastructure	0.0119	0.0160*	0.00805	0.00332	0.00776*	0.00277
	(0.0251)	(0.00870)	(0.00888)	(0.00309)	(0.00447)	(0.00459)
Production sector	0.0444	0.00219	-0.00451	0.00872**	0.000776	-0.00298
	(0.0294)	(0.0107)	(0.0108)	(0.00371)	(0.00538)	(0.00552)
Amount	-0.0242	-0.00739	-0.00407	-0.00135	-0.00372	-0.00181
	(0.0171)	(0.00559)	(0.00561)	(0.00195)	(0.00282)	(0.00290)
Year	0.00437**	0.00216***	0.000969	0.000563**	0.00112***	0.000441
	(0.00208)	(0.000719)	(0.000720)	(0.000243)	(0.000352)	(0.000361)
Africa	-0.0166	0.00162	-0.00298	-0.00267	0.00414	0.00246
	(0.0269)	(0.0108)	(0.0106)	(0.00290)	(0.00421)	(0.00431)
Asia	0.0130	-0.00756	-0.00451	0.00160	-0.000770	0.00130
	(0.0273)	(0.0112)	(0.0111)	(0.00314)	(0.00454)	(0.00466)
Europe	-0.0262	-0.0163	-0.0163	-0.00650	-0.00441	-0.00431
	(0.0447)	(0.0177)	(0.0179)	(0.00603)	(0.00874)	(0.00896)
South America	-0.0286	0.00625	0.00291	-0.00531	0.00526	0.00545
	(0.0518)	(0.0189)	(0.0185)	(0.00599)	(0.00868)	(0.00890)
Observations	429	1,771	1,798	3,485	3,485	3,485
R-squared	0.042	0.012	0.003	0.008	0.006	0.001

Notes: This table analyzes the representativeness of projects. One observation corresponds to a project in our event database of Chinese development projects. We regress a binary variable that takes a value of one if a project event date falls into a GWP survey window on the project characteristics listed below. Columns 1–3 include only projects with a commitment (start/end) date. Columns 4–6 include all projects independent of whether we have information on the commitment (start/end) date. "ODA" takes a value of one if the project is classified as official development assistance; "Social infrastructure" ("Economic infrastructure"/"Production sector") takes a value of one if the project is a part of the broad sector Social Infrastructure and Services (Economic Infrastructure and Services/Production Sectors) with the broad sector "Other" as reference category; "Amount" denotes the project's commitment amounts in US\$; and "Year" is the commitment year. Finally, we include binary variables for each of the five world regions named (with North America serving as reference category). Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.3 – Individual-level event study: Identifying projects with commitment date

Recipient	Title	Type	Flow	Sector	Year	ID	Value (US$ M)
Mali	Chinese organization signs agreement to build Confucius Classroom	Grant	OOF-like	Education	2008	31316	-
Cambodia	Construction project of the National Road No. 3762	Loan	ODA-like	Transport and Storage	2009	32180	20.67
Angola	3rd medical team to Angola	Technical assistance	ODA-like	Health	2013	34934	-
Argentina	China commits 2.1 Billion USD loan for rehabilitation of Belgrano Cargas railway	Loan	OOF-like	Transport and Storage	2014	36517	2100
Venezuela	Chinalco awarded 403 million USD contract for construction of Alcasa aluminum plant	Loan	OOF-like	Industry, Mining, Construction	2011	37914	-
Kazakhstan	China Development Bank commits 1 billion RMB for financing of Aktogay mine	Loan	OOF-like	Industry, Mining, Construction	2011	39557	167.8
Uzbekistan	China loans Uzbekistan 70.11m for purchase of Chinese electric locomotives	Export credits	OOF-like	Transport and Storage	2008	40070	95.15
India	China pledges to train 100 Indian officials on heavy haul transportation	Technical assistance	ODA-like	Transport and Storage	2014	42676	-
India	China opens Confucius Institute at University of Mumbai	Grant	OOF-like	Education	2012	43953	-
Laos	Preferential loan for Laos Xesalalong irrigation project	Loan	Vague	Agriculture, Forestry, Fishing	2011	47306	58.72

Notes: The table displays project-level information for committed projects included in panel A of column 2 of Table 1.1.

74

Table 1.B.4 − Individual-level event study: Identifying projects with start date

Recipient	Title	Type	Flow	Sector	Year	ID	Value (US\$ M)
Mauritania	China issues 2 billion yuan loan to fund Port of Friendship expansion project	Loan	ODA-like	Transport and Storage	2009	3	396.9
Ghana	Bui Dam Complex	Export credits	OOF-like	Energy Generation and Supply	2008	183	475.2
Mauritius	Exim Bank loans 260 mil for the expansion of the Sir Seewoosagur Ramgoolam Airport terminal	Loan	ODA-like	Transport and Storage	2011	1156	352.8
Congo, Rep.	Scholarships for higher education, 2012-2013	Scholarships/training	ODA-like	Education	2012	30143	-
Sudan	China Exim Bank loans 700 million USD for construction of new Khartoum Airport	Loan	ODA-like	Transport and Storage	2014	30543	700
Congo, D.R.	16th Chinese peacekeeping force	Technical assistance	ODA-like	Government and Civil Society	2013	30731	-
Congo, Rep.	22nd Chinese medical team	Technical assistance	ODA-like	Health	2013	31032	-
Senegal	Confucius Institute at University of Dakar	Grant	OOF-like	Education	2011	31282	-
Mali	Chinese organization signs agreement to build Confucius Classroom	Grant	OOF-like	Education	2008	31316	-
Madagascar	China donates anti-malaria medicine	Grant	ODA-like	Health	2008	35213	-
Togo	China sends 19th medical team to Togo	Technical assistance	ODA-like	Health	2011	35492	-
Zimbabwe	13th Chinese medical team	Technical assistance	ODA-like	Health	2013	35655	-
Pakistan	China provides relief material to Pakistan for internally displaced persons	Grant	ODA-like	Emergency Response	2009	35903	5.960
Somalia	China donates goods to Banadir Hospital	Grant	ODA-like	Health	2014	36408	-
Suriname	China Exim Bank commits 50 million USD loan to Suriname housing	Loan	Vague	Industry, Mining, Construction	2012	36772	52.95
Colombia	China donates two Harbin Y-12 aircrafts to Satena, Colombian national airline	Grant	ODA-like	Transport and Storage	2013	37138	-
Pakistan	China loans 1.35 billion USD for Suki Kinari Hydropower Project in Pakistan	Loan	OOF-like	Energy Generation and Supply	2017	39014	1350
Costa Rica	China offers 50 Scholarships per year to Costa Rican students	Scholarships/training	ODA-like	Education	2010	40099	-
Namibia	China donates N50 million to Hardap Inland Aquaculture Centre in Namibia	Grant	ODA-like	Other Social Infrastructure	2014	41578	4.607
India	China signs MoU to help improve Indian Chennai-Mysore Railway	Vague TBD	Vague	Transport and Storage	2014	42673	-
Sierra Leone	China constructs Ministry of Foreign Affairs building for Sierra Leone	Grant	ODA-like	Government and Civil Society	2010	43180	-

Notes: The table displays project-level information for started projects included in panel A of column 3 of Table 1.1.

Table 1.B.5 – Individual-level event study: Identifying projects with end date

Recipient	Title	Flow	Type	Sector	Year	ID	Value (US$ M)
Gabon	Loan for Grand Poubara Hydroelectric Project	ODA-like	Loan	Energy Generation and Supply	2013	85	114.6
Mauritius	China granted 480 billion CNY for the sewer netwok LOT2 project	ODA-like	Loan	Water Supply and Sanitation	2014	145	102.7
Côte d'Ivoire	Post-crisis reconstruction	ODA-like	Grant	Health	2009	718	80.06
Liberia	China contributes peacekeepers to UN mission in Liberia	ODA-like	Technical assistance	Government and Civil Society	2010	1552	-
Nigeria	China constructs four primary schools	ODA-like	Grant	Education	2012	2134	4.404
Guinea	China provides 335 million USD loan for Keleta dam	OOF-like	Loan	Energy Generation and Supply	2015	13823	371.8
Togo	China sends 18th medical team to Togo	ODA-like	Technical assistance	Health	2011	25286	-
Congo, Rep.	Scholarships for higher education, 2012-2013	ODA-like	Scholarships/training	Education	2012	30143	-
Sudan	China Exim Bank loans 700 million USD for construction of new Khartoum Airport	ODA-like	Loan	Transport and Storage	2014	30543	700
Zambia	CDB loans 179.5 million USD for Mansa-Luwingu Road	OOF-like	Loan	Transport and Storage	2016	30719	186.3
Vietnam	Exim Bank loans USD 250 million for Ninh Binh Nitrogenous fertilizer plant	Vague	Loan	Industry, Mining, Construction	2012	34478	515.2
Madagascar	China donates anti-malaria medicine	ODA-like	Grant	Health	2008	35213	-
Liberia	South-South Cooperation in Liberia	ODA-like	Technical assistance	Developmental Food Aid	2014	35267	1.110
Pakistan	China grants materials and funds for a digital seismic network in Pakistan	ODA-like	Grant	Emergency Response	2013	35615	-
Pakistan	China provides relief material to Pakistan for internally displaced persons	ODA-like	Grant	Emergency Response	2009	35903	5.960
Somalia	China donates goods to Banadir Hospital	ODA-like	Grant	Health	2014	36408	-
Colombia	China donates two Harbin Y-12 aircrafts to Satena, Colombian national airline	ODA-like	Grant	Transport and Storage	2013	37138	-
Indonesia	China develops earthquake and tsunami early warning system for Indonesia	ODA-like	Grant	Emergency Response	2010	37897	-
Venezuela	Construction of 3rd Joint Satellite	OOF-like	Loan	Communications	2017	38297	172.8
Costa Rica	China offers 50 scholarships per year to Costa Rican students	ODA-like	Scholarships/training	Education	2010	40099	-
Lebanon	Chinese engineers clear landmines in South Lebanon	ODA-like	Technical assistance	Government and Civil Society	2013	40968	-
Israel	China hosts 'Experience China' cultural event in Israel	OOF-like	Grant	Education	2009	41293	-

Notes: The table displays project-level information for completed projects included in panel A of column 4 of Table 1.1.

Table 1.B.6 – Descriptive statistics

	Obs	Mean	SD	Min	Max
Panel A: Individual level					
Approval of the Chinese government	29,331	0.70	0.46	0.00	1.00
Post	29,331	0.45	0.50	0.00	1.00
Gender	29,331	0.47	0.50	0.00	1.00
Age	29,331	36.37	15.40	13.00	99.00
Age squared	29,331	1,560	1,348	169	9,801
Education	29,331	1.65	0.64	1.00	3.00
Urban	29,331	2.43	1.14	1.00	4.00
Panel B: Province level					
Approval of the Chinese government	6,296	0.60	0.25	0.00	1.00
Chinese projects (province)	6,296	0.10	0.55	0.00	11.00
Province probability	6,296	0.04	0.09	0.00	0.80
Input	6,296	0.63	0.32	-0.93	1.26
Age	6,296	37.53	6.00	20.60	71.74
Age squared	6,296	1,693	540	461	5,202
Gender	6,296	0.52	0.11	0.00	1.00
Education	6,296	1.61	0.33	1.00	2.78
Urban	6,296	2.21	0.85	1.00	4.00
Panel C: Country level					
Approval of the Chinese government	452	0.63	0.17	0.12	0.97
Chinese projects (country)	452	1.46	4.70	0.00	84.00
Chinese projects predicted (country)	452	1.35	3.90	-0.00	65.00
Age	452	36.47	3.61	30.53	48.99
Age squared	452	1,601	318	1,111	2,783
Gender	452	0.51	0.02	0.45	0.57
Education	452	1.58	0.29	1.05	2.28
Urban	452	2.26	0.53	1.09	3.58

Notes: The table displays the descriptive statistics for the samples used in the micro analysis (Table 1.1, column 1, panel A) and the macro analysis (Table 1.2, column 2, panel C. for the province level and Table 1.2, column 1, panel C for the country level).

1.C   Extensions and Robustness of Individual-level Event Study

Table 1.C.1 – Chinese projects and government support, event study results, including month fixed effects

	(1) All	(2) Commit.	(3) Start	(4) End
Panel A: All projects				
Post	0.0157	-0.00966	0.0225	0.0299*
	(0.0125)	(0.0395)	(0.0156)	(0.0163)
Observations	29,331	5,610	15,362	15,465
Number of countries	35	9	19	20
Number of provinces	420	128	185	247
Number of projects	53	10	21	22
Province-year FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Panel B: ODA projects				
Post	0.0226	0.0377	0.0428**	0.0434**
	(0.0145)	(0.0849)	(0.0179)	(0.0174)
Observations	19,017	1,744	10,528	12,226
Number of countries	22	3	14	15
Number of provinces	265	58	125	171
Number of projects	35	3	15	17
Province-year FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Panel C: Large projects				
Post	0.0247	0.0391	0.0285	0.0590**
	(0.0205)	(0.0602)	(0.0283)	(0.0292)
Observations	13,783	2,168	5,619	7,865
Number of countries	19	5	7	11
Number of provinces	254	61	62	161
Number of projects	24	5	8	11
Province-year FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓

Notes: The dependent variable is binary and indicates whether or not an interviewed individual approves of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. Panel A shows results for the full sample of projects that contain information on the specific event date and fall into the interview window. Panel B only includes "Official Development Assistance-like" projects. Panel C only includes large projects, i.e., those with a size of US$ 1 million or above. All specifications include individual-level and survey controls as well as province-year fixed effects and month fixed effects. Standard errors are clustered by country-date: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.C.2 – Chinese projects and government support, event study results, alternative windows

	(1) All	(2) Commit.	(3) Start	(4) End
Panel A: 20 days				
Post	0.00800	-0.0373	0.0287*	0.0276*
	(0.0125)	(0.0376)	(0.0163)	(0.0160)
Observations	26,457	4,982	13,312	14,519
Number of countries	35	9	19	20
Number of provinces	397	119	180	231
Number of projects	53	10	21	22
Province-year FE	✓	✓	✓	✓
Panel B: 40 days				
Post	0.0127	-0.0484	0.0246	0.0291*
	(0.0119)	(0.0370)	(0.0152)	(0.0153)
Observations	29,821	5,813	15,759	15,681
Number of countries	35	9	19	20
Number of provinces	427	132	189	250
Number of projects	53	10	21	22
Province-year FE	✓	✓	✓	✓
Panel C: 60 days				
Post	0.0119	0.00991	0.0230	0.0274*
	(0.0118)	(0.0342)	(0.0151)	(0.0151)
Observations	30,868	7,247	17,038	15,839
Number of countries	36	10	20	20
Number of provinces	441	149	202	251
Number of projects	55	11	22	22
Province-year FE	✓	✓	✓	✓

Notes: The dependent variable is binary and indicates whether or not an interviewed individual approves of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The sample is restricted to individuals interviewed within 20 days before or after ("Post") a project-related event date in Panel A, 40 days in Panel B, and 60 days in Panel C. All specifications include individual-level and survey controls as well as province-year fixed effects. Standard errors are clustered by country-date: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.C.3 – Chinese projects and government support, event study results, alternative specifications

	(1) All	(2) Commit.	(3) Start	(4) End
	Panel A: OOF projects			
Post	-0.0178	-0.0329	-0.0440	0.0224
	(0.0216)	(0.0447)	(0.0295)	(0.0382)
Observations	11,277	3,866	4,834	3,239
Number of countries	15	7	6	5
Number of provinces	184	84	64	76
Number of projects	18	7	6	5
Province-year FE	✓	✓	✓	✓
	Panel B: Small projects			
Post	0.000304	-0.0915*	0.0122	0.0213
	(0.0159)	(0.0511)	(0.0197)	(0.0200)
Observations	15,548	3,441	9,743	7,600
Number of countries	19	4	12	11
Number of provinces	205	67	123	105
Number of projects	29	5	13	11
Province-year FE	✓	✓	✓	✓

Notes: The dependent variable is binary and indicates whether or not an interviewed individual approves of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. Panel A only includes "Other Official Flows-like" projects. Panel B only includes projects with a size below US\$ 1 million (or where information on financial values is not available). All specifications include individual-level and survey controls as well as province-year fixed effects. Standard errors are clustered by country-date: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.C.4 – Chinese projects and government support in project provinces, event study results

	(1) All	(2) Commit.	(3) Start	(4) End	(5) All	(6) Commit.	(7) Start	(8) End	(9) All	(10) Commit.	(11) Start	(12) End
	Panel A: All projects				*Panel B: ODA projects*				*Panel C: Large projects*			
Post	0.00663	-0.0715*	0.0219	0.0254	0.0217	-0.0409	0.0484**	0.0344*	0.0184	0.000968	0.0330	0.0584**
	(0.0131)	(0.0398)	(0.0161)	(0.0163)	(0.0158)	(0.0822)	(0.0192)	(0.0178)	(0.0196)	(0.0647)	(0.0250)	(0.0286)
Post * project province	0.0329	0.151**	0.00667	0.0402	0.00764	-0.0601	-0.0192	0.0586*	0.0394	0.141*	0.0844	-0.00528
	(0.0268)	(0.0699)	(0.0366)	(0.0331)	(0.0314)	(0.205)	(0.0390)	(0.0350)	(0.0378)	(0.0801)	(0.0567)	(0.0499)
Sum of coefficients	0.0395	0.0795	0.0285	0.0656	0.0293	-0.1011	0.0292	0.0929	0.0577	0.1422	0.1174	0.0531
P-value	0.1035	0.1967	0.4188	0.0425	0.3060	0.6193	0.4392	0.0074	0.1083	0.0231	0.0389	0.2833
Observations	29,331	5,610	15,362	15,465	19,017	1,744	10,528	12,226	13,783	2,169	5,619	7,865
Number of countries	35	9	19	20	22	3	14	15	19	5	7	11
Number of provinces	420	128	185	247	265	58	125	171	254	61	62	161
Number of projects	53	10	21	22	35	3	15	17	24	5	8	11
Province-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is binary and indicates whether or not an interviewed individual approves of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. "Project province" is a binary variable indicating if a province hosts the respective Chinese development project. Panel A shows results for the full sample of projects that contain information on the specific event date and fall into the interview window. Panel B only includes "Official Development Assistance-like" projects. Panel C only includes large projects, i.e., those with a size of US$ 1 million or above. All specifications include individual-level and survey controls as well as province-year fixed effects. Standard errors are clustered by country-date: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.C.5 – Chinese projects, government support, and leader visits, event study results

	(1) All	(2) Commit.	(3) Start	(4) End
Post	0.0197	-0.0827	0.0286*	0.0408**
	(0.0130)	(0.0594)	(0.0156)	(0.0165)
Observations	26,336	3,539	14,399	14,541
Number of countries	33	7	18	20
Number of provinces	384	92	161	246
Number of projects	48	7	20	21
Province-year FE	✓	✓	✓	✓

Notes: The dependent variable is binary and indicates whether or not an interviewed individual approves of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. We drop events that coincide with leader visits from the sample based on coding in Wang and Stone (2023). Three visits coincide with project commitment dates (Kazakhstan 2011, Argentina 2014, India 2014), one with a start date (India 2014), and one with an end date (Pakistan 2013). All specifications include individual-level and survey controls as well as province-year fixed effects. Standard errors are clustered by country-date: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.C.6 – Chinese projects and support for China as nation, event study results

	(1) All	(2) Commit.	(3) Start	(4) End
Post	0.0573	-0.130	0.0744	0.157**
	(0.0552)	(0.0786)	(0.0553)	(0.0654)
Observations	2,921	790	1,943	2,131
Number of countries	3	1	2	2
Number of provinces	26	8	12	18
Number of projects	5	1	2	2
Province-year FE	✓	✓	✓	✓

Notes: The dependent variable indicates whether or not an interviewed individual has a favorable opinion on China based on the question "In general, what opinion do you have on the following nations? China." The sample is restricted to individuals interviewed within 30 days before or after ("Post") a project-related event date. All specifications include individual-level and survey controls as well as province-year fixed effects. Standard errors are clustered by country-date: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

1.D Extensions and Robustness of Macro-level Analysis

1.D.1 Sector-specific Results

This appendix tests possible transmission channels by investigating sector-specific projects. Projects in some sectors potentially affect public opinion of China differentially than others, for example because they receive more attention (in the project location or elsewhere) or because they induce greater externalities. We also expect that projects targeted at large portions of a recipient country population should be more likely than more narrowly-targeted projects to produce positive, widespread public opinion gains for a donor government.

We test whether the number of Chinese development projects completed in a province or country in the previous year affects approval of the Chinese government differently if at least one of these projects was given to a specific main sector—social, economic, or production—or sub-sector.[72] To this end, we add a binary variable indicating the sector and the interaction of the binary variable with the number of projects to eq. 1.2 and estimate it with a Control Function (CF) approach. This implies that we control for the first-stage regression residual (shown in eq. 1.3 above) in all second stages. Alternatives to this approach are 2SLS employing the interaction of our instrument with the sector indicator or separate regressions for each sector. The first approach treats the interaction of the endogenous variable as separate, implying it "can be quite inefficient relative to the more parsimonious CF approach" (Wooldridge 2015, p. 429).[73] The second violates the exclusion restriction, as for each regression we have to assume that Chinese development projects affect public approval exclusively via the sector the regression focuses on.[74]

Panel A of Table 1.D.1 shows the results at the level of countries. First, no sector seems to be driving the positive effect of project completion on the support for the Chinese government. The positive effect of Chinese projects is reduced if at least one project went to the production sector. In terms of sub-sectors, the same holds for the agriculture and industry sectors. Agriculture includes agricultural equipment and demonstration centers, fertilizer factories, and land development. Industry includes mines, pipelines, and industrial plants for potash, aluminium, and platinum. If anything, these projects are rather private goods of commercial character that do not benefit a wider audience, which might explain this finding.

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[72]Note that we focus on the longer-term analysis in this appendix given that the number of projects in the event specification is too low for sector-specific analyses.

[73]This increase in efficiency comes at the cost of an additional assumption; that is, we need to assume that the bias is constant in all sectors. Note that we adjust standard errors to take account of the predicted estimator from the first stage.

[74]In the CF specification, the first-stage regressions (and F-statistics) are identical to those shown in columns 1 and 2 of Table 1.2.

At the province level in panel B, the picture is different. Chinese projects are seen as more positive if at least one of them is completed in the production sector. The positive effect is driven by projects completed in the agricultural sector, providing evidence that the transfer of technical equipment and land development is positively perceived around the places where these projects are undertaken. The same holds true for emergency aid. Finally, there is an additional negative support premium on the completion of water-related projects in project regions.

Both the positive country effect and the negative province effect do not seem to be driven by a specific sector, but rather prevail among all Chinese development activities. The sector-specific analysis provides some evidence in line with the expectation that projects targeted at large portions of a recipient country population are more likely than more narrowly-targeted projects to produce positive, widespread public opinion gains for a donor government.

Table 1.D.1 – Chinese projects and government support by sector, instrumental variables results

	(1) Social (S)	(2) Economic (E)	(3) Production (P)	(4) Education (S1)	(5) Health (S2)	(6) Water (S3)	(7) Government (S4)	(8) Transport (E1)	(9) Communication (E2)	(10) Energy (E3)	(11) Agriculture (P1)	(12) Industry (P2)	(13) Emergency (.)
							Panel A: Country level						
Chinese projects $_{y-1}$	0.00581*	0.00483**	0.00597***	0.00199***	0.00246	0.00206***	0.00191***	0.00219***	0.00412**	0.00195***	0.00544***	0.00204***	0.00201***
	(0.00304)	(0.00200)	(0.00140)	(0.000597)	(0.00183)	(0.000623)	(0.000551)	(0.000691)	(0.00207)	(0.000559)	(0.00150)	(0.000582)	(0.000574)
Sector dummy	0.0116	0.0262**	0.00458	0.0153	0.00280	0.00102	-0.00476	0.0305	-0.0338	0.0326	-0.0103	0.0636**	0.0173
	(0.00962)	(0.0131)	(0.0211)	(0.0161)	(0.0123)	(0.0438)	(0.0199)	(0.0322)	(0.0249)	(0.0235)	(0.0267)	(0.0316)	(0.0280)
Interaction	-0.00421	-0.00366*	-0.00491***	-0.000304	-0.000539	-0.00518	0.00166	-0.00402	-0.00183	-0.00162	-0.00399**	-0.0227**	-0.000872
	(0.00319)	(0.00199)	(0.00172)	(0.00235)	(0.00221)	(0.00927)	(0.00285)	(0.00437)	(0.00265)	(0.00658)	(0.00185)	(0.00934)	(0.00375)
Observations	452	452	452	452	452	452	452	452	452	452	452	452	452
Number of countries	90	90	90	90	90	90	90	90	90	90	90	90	90
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
							Panel B: Province level						
Chinese projects $_{y-1}$	-0.0668*	-0.0758**	-0.0711**	-0.0695*	-0.0705*	-0.0723**	-0.0724**	-0.0718**	-0.0725**	-0.0751**	-0.0720**	-0.0718**	-0.0713**
	(0.0369)	(0.0352)	(0.0353)	(0.0361)	(0.0376)	(0.0352)	(0.0353)	(0.0350)	(0.0350)	(0.0346)	(0.0350)	(0.0354)	(0.0353)
Sector dummy	0.00535	0.0275	-0.118*	-0.0134	-0.00268	0.0410	0.0187	-0.00536	0.0270	0.0438	-0.135*	-0.163	-0.0689*
	(0.0184)	(0.0271)	(0.0600)	(0.0273)	(0.0235)	(0.0486)	(0.0244)	(0.0414)	(0.0508)	(0.0350)	(0.0750)	(0.179)	(0.0393)
Interaction	-0.00954	0.00335	0.0367***	-0.00214	-0.00199	-0.0376**	0.00204	-0.00231	0.000512	0.0157*	0.0392***	0.0932	0.0216**
	(0.0107)	(0.00837)	(0.0110)	(0.0114)	(0.0109)	(0.0187)	(0.00714)	(0.0116)	(0.00896)	(0.00918)	(0.0129)	(0.0925)	(0.0103)
Observations	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296
Number of countries	91	91	91	91	91	91	91	91	91	91	91	91	91
Number of provinces	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399
Country-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports results from a Control Function Approach, where we control for the first-stage residual of column 2 (1) of Table 1.2 at the country (province) level. The dependent variable is the share of individuals that approve of the Chinese government in a given province/country, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The variable of interest is the number of Chinese development projects completed in the previous year ("Chinese projects $_{y-1}$") in the sector noted in the column header. "Social," "Economic," and "Production" are the main sectors. We also report results for key sub-sectors and emergency aid: "Education," "Health," "Water," and "Government" are sub-sectors of "Social"; "Transport," "Communication," and "Energy" are sub-sectors of "Economic"; and "Agriculture" and "Industry" are sub-sectors of "Production." "Emergency" is not included in a main sector. Panel B (A) contains results of regressions at the province-year (country-year) level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors (adjusted for uncertainty arising from the use of the predicted value from the first stage) are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

1.D.2 Tests for Robustness

We test the robustness of our key results in a number of ways. First, we complement the results shown in Tables 1.1 and 1.2 with analyses of Other Official Flows and projects with commitment volumes below US$ 1 million ("small"). Second, we define development finance in terms of a binary indicator for any project or commitment amounts rather than numbers. Third, we investigate different timings with which development projects could affect public opinion. Fourth, we test the robustness of results to variants of our instrumental variable. And fifth, we investigate robustness to other shocks originating in China that might potentially violate the exclusion restriction for our instrument to be valid.

Columns 1–4 of Table 1.D.5 shows the results for OOF and small projects at the macro level. While there is no significant effect of OOF projects on support for China's government, the effect of small projects stays significant at the country (but not province) level. Surprisingly, the coefficient is larger rather than smaller compared to that for large projects shown in Table 1.2.

In columns 5–8 of Table 1.D.5, we turn to the results for our alternative definitions of Chinese development funding. At the country level, our results are robust when we use a binary project indicator or (log) commitment amounts instead of the project count. The negative coefficient at the level of provinces, however, is estimated less precisely.

Table 1.D.6 investigates different timings with which projects might affect the support for the Chinese government. As can be seen, there are no significant effects two years after project completion, though the marginal effect is almost identical to the one-year lag we chose for our main analysis, both at the level of provinces and countries. The table also shows that deeper lags are insignificant as well. The same holds for future projects which serves as important placebo test.

Table 1.D.7 estimates cumulative effects to account for the possibility that today's approval of the Chinese government may be shaped not only by projects in the previous year, but also by those completed in earlier years. Our baseline results could overstate the true effects if provinces (or countries) that received projects in the previous year were also more likely to receive projects in earlier years. To account for this, we use moving averages for both project numbers and instrumental variables of up to five years. For all projects, at the province level, the coefficient increases and remains statistically significant when we average over two years. However, when we include further lags, the coefficient turns less negative and becomes statistically insignificant. At the country level, the coefficient size remains similar but is less precisely estimated with moving averages of several years. It thus seems that the public opinion effects of Chinese development projects are rather short-lived.

Table 1.D.8 probes our instrument in various ways. First, we employ an additional

instrumental variable, suggested by Dreher et al. (2021b). Dreher et al. show that the larger availability of foreign currency reserves increases the supply of China's development funding. This is because much of China's funding comes as interest-bearing loans, which represents a financially attractive means to hold such reserves. In line with Dreher et al., we interact China's net currency reserves in US$ with the probability of receiving projects, so that the instrument again varies across space as well as over time. Second, we calculate the probability of receiving projects based on pre-sample years (2000–2005). This has the advantage of being more plausibly exogenous unconditionally. It comes at the cost of reduced information from a small number of years, so that we expect the power of the instrument to be lower. Third, we purge the input materials that we use to construct our main instrument by China's GDP (relying on a regression of each input factor on China's GDP in constant currency units) so that we take account of China's varying domestic needs for input materials before constructing our instrument. Fourth, we replace the six input materials by just one—steel—before we interact it with the probability of receiving projects, in line with Dreher et al. (2021a), who originally introduced this instrument. Finally, we offer placebo regressions, where we instrument China's development projects with yearly volumes of US steel production, the first factor of all six inputs for the United States, and Chinese toilet paper production, respectively (Bluhm et al. 2020).

We restrict these tests to the province-level regressions given that they refer to the instrumental variable, which we construct on this level.[75] Table 1.D.8 shows that the results are robust to these perturbations of the instrument. Column 2 includes the second instrument, based on China's net currency reserves. Compared to column 1, which includes the original estimate based only on the input factor-based instrument, the coefficient hardly changes and is more precisely estimated. We can also use "historic" probabilities to receive projects as part of the instrument, as we show for the two instruments in column 3 or only the input factor-based instrument in column 4. Coefficients increase in size, with marginal (in-)significance. Column 5 shows results focusing on "overproduction," where we have residualized factor inputs by running a regression of each input on the log of Chinese GDP (in constant local currency) before the first factor was extracted. Again, results hardly change. When we base the instrument on just steel (in column 6), results are again similar, though the coefficient is less precisely estimated.[76] The placebo regressions in columns 7, 8, and 9 instead show a very weak first stage when we replace Chinese raw material inputs with US steel production, US raw material production, or Chinese toilet paper production, with a completely insignificant coefficient in the second

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[75]We only aggregate province-level predictions where F-statistics are above the conventional threshold (columns 1, 2, 5, and 6). For these, country-level results are almost identical to the baseline.

[76]More specifically, we use the linearly detrended log of Chinese steel production from the National Bureau of Statistics of China as the time-series shock. We standardize this variable before multiplying it with the exposure term so that the coefficient is comparable with that using the first common factor of all inputs.

stage.[77]

Table 1.D.9 probes the exclusion restriction for our instrument to be valid, both at the level of provinces (panels A and B) and countries (panels C and D). Column 1 adds the interaction of Chinese foreign direct investment outflows (in logs of current US dollars) interacted with the probability of receiving projects. Column 2 focuses on 'Imports' rather than FDI, defined as bilateral imports from China (in logs of current US dollars) from the IMF Direction of Trade Statistics. 'Exports' (in column 3) is the value of recipient-country exports to China (in logs of current US dollars) from the IMF Direction of Trade Statistics. We do so because countries that often receive aid from China are likely to be also countries were China invests and trades. The probability of receiving projects could thus proxy the probability of trading and investing. Given that material input production is likely to be correlated with trade and investment as well as with development projects, results for development projects might be spurious. However, Table 1.D.9 shows that results hardly change when we include these variables individually (in columns 1–3) or jointly (column 4). While we cannot rule out that other omitted variables violate the exclusion restriction, we consider it unlikely given that controlling for the three variables that are most plausibly related to China's development projects and its input material production hardly changes our results.

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[77]Column 7 uses a US construction steel production index from FRED hosted by the Federal Reserve Bank of St. Louis (Series IPN3311A2BS). We log, detrend, and standardize the series, just like Chinese steel production in the previous column. Column 8 constructs the logged, detrended first factor of US raw material production in equivalence to the input factor used for Chinese raw material production, relying on FRED data on US production of aluminum (IPG3313S), cement (IPG3273SQ), glass (IPG3272S), iron (IPG3311A2NQ), steel (IPN3311A2BS), and timber (IPG321S). Column 9 interacts the province-specific probability of receiving aid with Chinese production of household and sanitary papers (in tonnes from FAOSTAT) as a placebo instrument. We log, detrend, and standardize the series, just like the steel production.

Table 1.D.2 – Chinese projects and government support, instrumental variables results, alternative weights

	(1) All	(2) ODA	(3) Large
Chinese projects $_{y-1}$	-0.0565*	-0.0669**	-0.0555*
	(0.0309)	(0.0323)	(0.0328)
Number of countries	91	91	91
Number of provinces	1,399	1,399	1,399
Country-year FE	✓	✓	✓
Province FE	✓	✓	✓
F-stat	16.09	25.86	21.30

Notes: The table reports results from eq. 1.2, where we weight provinces with the number of individuals surveyed as share of total survey respondents in the country-year wave. The dependent variable is the share of individuals that approve of the Chinese government in a given province, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The variable of interest is the number of Chinese development projects completed in the previous year ("Chinese projects t−1"). All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.D.3 – Chinese projects and government support, instrumental variables results, using inputs as instrument in country-level and worldwide analysis

	(1) All	(2) ODA	(3) Large
Panel A: Eq. 1.3 on the country level			
Chinese projects $_{y-1}$	0.00733	0.00949	0.0196
	(0.0117)	(0.0121)	(0.0167)
Observations	452	452	452
Number of Countries	90	90	90
Country FE	✓	✓	✓
Year FE	✓	✓	✓
F-stat	3.217	3.529	7.334
Panel B: Eq. 1.3 on the country level, first stage based on 2000–2017			
Chinese projects $_{y-1}$	0.00183**	0.00168**	0.00122*
	(0.000873)	(0.000767)	(0.000663)
Observations	452	452	452
Number of countries	90	90	90
Country FE	✓	✓	✓
Year FE	✓	✓	✓
F-Stat	12.968	8.899	12.073
Panel C: Eq. 1.3 on the global level, projects instrumented with input factors			
Chinese projects worldwide $_{y-1}$	0.000133	0.000225	0.000299
	(9.40e-05)	(0.000159)	(0.000214)
Observations	477	477	477
Number of Countries	90	90	90
Country FE	✓	✓	✓
F-Stat	349.7	299.8	332.3

Notes: The dependent variable is the share of individuals that approve of the Chinese government, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The variable of interest is the number of Chinese development projects completed in the previous year ("Chinese projects $_{y-1}$"). Panels A and B are on the country level, panel C is on the global level. Column 2 only includes "Official Development Assistance-like" projects. Column 3 only includes projects with a size of US$ 1 million or above. Panel A uses the interaction of "Input" and "probability" as the instrument (instead of the aggregated predictions). Panel B estimates the first stage including the out-of-sample period 2000-2005. Panel C presents global results, using the time series of the Chinese input factors as instrument for Chinese aid. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.D.4 – Chinese projects and support for China as nation, instrumental variables results

	(1)	(2)	(3)	(4)	(5)	(6)
Chinese Projects $_{y-1}$	-0.160	0.00622	-0.194	0.0151	0.652	0.00989
	(0.136)	(0.0159)	(0.111)	(0.0110)	(1.292)	(0.0199)
Level	Province	Country	Province	Country	Province	Country
Project type	All	All	ODA	ODA	Large	Large
Observations	324	28	324	28	324	28
Number of Countries	14	14	14	14	14	14
Number of Provinces	155	-	155	-	155	-
Country FE		✓		✓		✓
Year FE		✓		✓		✓
Country-year FE	✓		✓		✓	
Province FE	✓		✓		✓	
F-stat	2.943	306.3	5.101	3460	0.429	107

Notes: The dependent variable indicates whether or not an interviewed individual has a favorable opinion on China based on the question "In general, what opinion do you have on the following nations? China." Columns with project type "ODA" include only "Official Development Assistance-like" projects. Columns with project type "Large" include only projects with a size of US$ 1 million or above. Columns with level "Province" ("Country") contain results of regressions at the province-year (country-year) level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.D.5 – Chinese projects and government support, instrumental variables results, alternative specifications

	(1) OOF	(2) OOF	(3) Non-large	(4) Non-large	(5) Binary	(6) Binary	(7) Amounts	(8) Amounts
Panel A: OLS								
Chinese projects $_{y-1}$	-0.00302	0.00431	-0.0168**	0.00625**	-0.0106	0.0104***	0.000541	0.000418**
	(0.0114)	(0.00404)	(0.00734)	(0.00267)	(0.0102)	(0.00244)	(0.000738)	(0.000183)
Panel B: Reduced form								
Input*probability $_{y-3}$	-0.0465		-0.134		-0.139**		-0.139**	
	(0.187)		(0.0950)		(0.0677)		(0.0677)	
$\sum_p$Input*probability $_{y-3}$		0.00378		0.00655**		0.0119***		0.000503**
		(0.00396)		(0.00292)		(0.00316)		(0.000232)
Panel C: 2SLS								
Chinese projects $_{y-1}$	-0.0356	0.00380	-0.0983	0.00630**	-0.323	0.0112***	-0.0159	0.000466**
	(0.140)	(0.00398)	(0.0907)	(0.00282)	(0.248)	(0.00298)	(0.00993)	(0.000215)
Panel D: First stage								
Input*probability $_{y-3}$	1.515**		1.008		0.399**		9.380***	
	(0.643)		(0.701)		(0.180)		(2.801)	
$\sum_p$Input*probability $_{y-3}$		0.990***		1.019***		1.037***		1.062***
		(0.0105)		(0.0230)		(0.0308)		(0.0357)
Observations	6,296	452	6,296	452	6,296	452	6,296	452
Level	Province	Country	Province	Country	Province	Country	Province	Country
Size	OOF	OOF	Non-Large	Non-Large	All	All	All	All
Number of countries	91	90	91	90	91	90	91	90
Number of provinces	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399
Country FE		✓		✓		✓		✓
Year FE		✓		✓		✓		✓
Country-year FE	✓		✓		✓		✓	
Province FE	✓		✓		✓		✓	
F-stat	4.989	13133	3.082	7454	5.027	1799	8.951	1258

Notes: The dependent variable in panels A–C is the share of individuals that approve of the Chinese government in a given province/country, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The dependent variable in panel D and variable of interest in panels A–C is the number of Chinese development projects completed in the previous year ("Chinese projects $_{y-1}$"). Columns with project type "OOF" include only non-"Official Development Assistance-like" projects. Columns with project type "Non-large" include only projects with a size of below US\$ 1 million or those without financial values reported. "Binary" indicates that we change the variable of interest to a binary variable indicating whether or not a Chinese development project was completed. "Amounts" indicates that we exchanged the variable of interest to the log amount of projects completed. Columns with level "Province" ("Country") contain results of regressions at the province-year (country-year) level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.D.6 – Chinese projects and government support, instrumental variables results, leads and lags

	(1) y-4	(2) y-3	(3) y-2	(4) y-1	(5) y+0	(6) y+1	(7) y+2
Panel A: Country Level							
Chinese projects	-0.00201	0.000101	0.000310	0.00206**	0.000804	-0.000168	0.000519
	(0.00153)	(0.00193)	(0.000805)	(0.000972)	(0.000513)	(0.00100)	(0.000711)
Observations	452	452	452	452	452	452	452
Number of countries	90	90	90	90	90	90	90
Baseline	.	.	.	✓	.	.	.
Country FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
F-stat	3,984	12,772	6,608	464.4	3,590	1,214	1,398
Panel B: Province Level							
Chinese projects	0.0323	-0.00110	-0.0702	-0.0722*	-0.0757	-0.171	-0.0780
	(0.0304)	(0.0295)	(0.0456)	(0.0389)	(0.149)	(0.151)	(0.166)
Observations	6,296	6,296	6,296	6,296	6,296	5,094	3,943
Number of countries	91	91	91	91	91	89	84
Number of provinces	1,399	1,399	1,399	1,399	1,399	1,321	1,199
Baseline	.	.	.	✓	.	.	.
Country-year FE	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓
F-stat	6.945	13.92	6.023	22.14	0.944	1.871	0.671

Notes: The table reports results from eq. 1.2 changing the lag structure of the dependent variable and the instrument. The dependent variable is the share of individuals that approve of the Chinese government in a given province/country, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The variable of interest is the number of Chinese development projects completed ("Chinese projects"). Column titles indicate the lag of "Chinese projects"; the instrumental variable ("inputs") is lagged by two additional years relative to the variable of interest. Column 4 reports the baseline. Panel B (A) contains results of regressions at the province-year (country-year) level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects indicated in the table. Standard errors are clustered by country: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.D.7 – Chinese projects and government support, instrumental variables results, moving averages

	(1) Lag 1	(2) Lag 1 – Lag 2	(3) Lag 1 – Lag 3	(4) Lag 1 – Lag 4	(5) Lag 1 – Lag 5
	Panel A: Country level				
Chinese projects (All)	0.00206** (0.000972)	0.00213 (0.00155)	0.00254 (0.00221)	0.00223 (0.00249)	0.00255 (0.00325)
F-stat	464.4	2466	4037	3478	1355
Chinese projects (ODA)	0.00189** (0.000858)	0.00264 (0.00208)	0.00362 (0.00318)	0.00414 (0.00348)	0.00545 (0.00446)
F-stat	555.1	479.9	581.3	522.2	370
Chinese projects (Large)	0.00140* (0.000770)	0.00133 (0.00132)	0.00169 (0.00187)	0.00187 (0.00225)	0.00298 (0.00320)
F-stat	650.6	2257	2098	503.8	343.2
	Panel B: Province level				
Chinese projects (All)	-0.0722* (0.0389)	-0.0914* (0.0535)	-0.0422 (0.0392)	-0.00890 (0.0418)	0.0139 (0.0433)
F-stat	22.14	13.89	20.57	11.11	8.919
Chinese projects (ODA)	-0.0864** (0.0409)	-0.139* (0.0826)	-0.0631 (0.0605)	-0.0154 (0.0666)	0.0179 (0.0672)
F-stat	28.32	7.896	8.585	4.701	4.242
Chinese projects (Large)	-0.0682* (0.0400)	-0.101 (0.0707)	-0.0590 (0.0558)	-0.0193 (0.0596)	0.0128 (0.0652)
F-stat	14.72	7.993	10.81	8.251	6.785

Notes: The table reports results from eq. 1.2, where instead of a one-year time lag we create moving averages of our treatment variable ("Chinese projects") and our instrument ("Inputs"). The dependent variable is the share of individuals that approve of the Chinese government in a given country/province, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The variable of interest is the number of Chinese development projects completed ("Chinese projects"). Column 1 reports the baseline. Column 2 reports averages, averaging lag 1 and lag 2 of both "Chinese projects" and "Inputs." Column 3 (4, 5) average lags 1 to 3 (4, 5). Panel B (A) contains results of regressions at the province-year (country-year) level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects (country FE and year FE in panel A, country-year FE and province FE in panel B). Standard errors are clustered by country: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.D.8 – Chinese projects and government support, instrumental variables results, robustness of the instrument

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Input +	Input +		Over-	Detrended	US steel	US input	Toilet
Shift	Input	reserves	reserves	Input	production	steel	placebo	placebo	paper
Share	*All*	*All*	*Historic*	*Historic*	*All*	*All*	*All*	*All*	*All*
Chinese projects $_{y-1}$	-0.0722*	-0.0762**	-0.175*	-0.166	-0.0797**	-0.0707	-0.142	-0.105	-0.131
	(0.0389)	(0.0377)	(0.0985)	(0.137)	(0.0395)	(0.0429)	(0.543)	(0.101)	(0.0992)
Observations	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296	6,296
Number of countries	91	91	91	91	91	91	91	91	91
Number of provinces	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399	1,399
Country-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
F-stat	22.14	11.11	6.840	7.822	12.19	15.09	0.145	2.241	3.190

Notes: The table reports results from eq. 1.2 changing the components of the instrumental variable. The dependent variable is the share of individuals that approve of the Chinese government in a given province based on the question "Do you approve or disapprove of the job performance of the leadership of China?" The variable of interest is the number of Chinese development projects completed ("Chinese projects"). Column 1 reports the baseline. In column 2, we add a second instrument consisting of the changes in China's net currency reserves interacted with the same probability of receiving aid (Dreher et al. 2021b). In columns 3–4, we replace the contemporaneous probability with the probability of receiving aid in the pre-sample period (2000-2006) ("Historic") and show results for both the two alternative instruments (column 3) and the baseline (column 4). Columns 5 and 6 change the definition of the shift variable. "Overproduction" implies that the factor inputs were residualized by running a regression of each input on the log of Chinese GDP in constant local currency before the first factor was extracted. We standardize this variable before multiplying it with the exposure term so that the coefficient is comparable with that using the first common factor of all inputs. "Detrended steel" uses the standardized, linearly detrended log of Chinese steel production from the National Bureau of Statistics of China as the time-series shock. Columns 7–9 contain placebo tests. 'US steel Placebo' uses a standardized, linearly detrended log US steel production index from FRED hosted by the Federal Reserve Bank of St. Louis (Series IPN3311A2BS) as a placebo instrument. 'US input Placebo' uses the first factor of US raw materials production indices from FRED of aluminum (IPG3313S), cement (IPG3273SQ), glass (IPG3272S), iron (IPG3311A2NQ), steel (IPN3311A2BS), and timber (IPG321S) as a placebo instrument. 'Toilet Paper' uses the standardized, linearly detrended log Chinese production of household and sanitary papers (in tonnes from FAOSTAT) as placebo instrument. All regressions are at the province-year level. All specifications include the control variables age, age squared, gender, education, and urban in addition to the set of fixed effects (country-year FE and province FE). Standard errors are clustered by country: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.D.9 – Chinese projects and government support, instrumental variables results, other "China shocks"

	(1) FDI	(2) Imports	(3) Exports	(4) All
Country level				
		Panel A: 2SLS		
Chinese projects $_{y-1}$	0.00189**	0.00174*	0.00190*	0.00184*
	(0.000939)	(0.000942)	(0.000987)	(0.000997)
		Panel B: First stage		
$\sum_p$ Input*probability $_{y-3}$	1.223***	1.222***	1.224***	1.224***
	(0.0571)	(0.0574)	(0.0571)	(0.0575)
Observations	452	438	438	438
Number of countries	90	87	87	87
F-stat	458.6	454	460.3	453.8
Province level				
		Panel C: 2SLS		
Chinese projects $_{y-1}$	-0.0665*	-0.0674	-0.0717*	-0.0663*
	(0.0364)	(0.0409)	(0.0399)	(0.0386)
		Panel D: First stage		
Input*probability $_{y-3}$	1.937***	1.904***	1.899***	1.920***
	(0.385)	(0.409)	(0.391)	(0.427)
Observations	6,296	6,207	6,207	6,207
Number of countries	91	88	88	88
Number of provinces	1,399	1,379	1,379	1,379
F-stat	25.38	21.70	23.52	20.19

Notes: The table reports instrumental variables results adding "China shock" control variables. Panels A and B report results on the country level based on eq. 1.4 and eq. 1.5, panels C and D report results on the province level based on eq. 1.2 and eq. 1.3. Panel A (C) shows the second stage of two-stage least-squares fixed-effects regressions where the dependent variable is the share of individuals that approve of the Chinese government in a given province/country, based on the question "Do you approve or disapprove of the job performance of the leadership of China?" Panel B (D) shows the corresponding first-stage least-squares fixed-effects regressions where the dependent variable is the number of Chinese development projects completed in the previous year. Each column adds a "China Shock" variable interacted with the country- (province-) specific probability of receiving aid. "FDI" are Chinese outward foreign direct investments (in logs of current US dollars) from UNCTAD. "Imports" are recipient-country imports from China (in logs of current US dollars) from the IMF Direction of Trade Statistics. "Exports" is the value of recipient-country exports to China (in logs of current US dollars) from the IMF Direction of Trade Statistics. "All" reports the results when including all three "China shock" control variables. Province-level specifications include province-fixed effects and country-year-fixed effects, country-level specifications include country- and year-fixed effects. All specifications include the control variables age, age squared, gender, education, and urban. Standard errors are clustered by country: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

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2

Bureaucrats, Ideological Biases, and the Policies of International Organizations

2.1   Introduction

International organizations (IOs) make momentous decisions on major global public policies. A well-established strand of research in political science has documented how states introduce their preferences into IO policy outputs through means that mirror domestic power struggles, like coalition formation, logrolling, and backroom deals (e.g., Carter and Stone 2015, Dreher et al. 2009, 2022, Kersting and Kilby 2016, Kilby 2009, Schneider and Tobin 2013). But, in addition to this 'geopolitical' side of IO decisions, there is also a 'technocratic' one. This is the realm of technical expertise, embodied by career staff who are highly trained in their respective work areas and often enjoy considerable autonomy in conducting the day-to-day business of their organization (Abbott and Snidal 1998, Barnett and Finnemore 1999, 2004). Their power rests on the purportedly dispassionate, evenhanded, and rules-based application of expert knowledge by staff who have shared beliefs due to their broadly similar academic training and professional socialization (Chwieroth 2010, Nelson 2014). This allows IO bureaucracies to present themselves as well-oiled machines: hierarchical structures with tight control over the application of technical knowledge and oversight over staff activities, thereby shielding IOs from the worst excesses of geopolitical interference and producing policy output that is streamlined and impartial.

In this article, we question this image of IOs. We argue that IO staff have room for discretion, especially when the core geopolitical interests of their most powerful member-states are *not* at stake, and this allows them to introduce their own biases into organizational output. A widely publicized recent event provides a case in point: in 2021, the World Bank discontinued its influential 'Doing Business' indicators amidst allegations of manipulations by staff, whether due to caving to pressures from a powerful shareholder (intervening to improve the ranking of China) or due to individual political biases (ideological dislike for the policies of a leftist Chilean government) (Brunswijck 2018, Deol 2021). The former case comes as no surprise, as IO staff are known to please important principals (e.g., Stone 2011, Lim and Vreeland 2013, Clark and Dolan 2021). But the latter possibility—that a staffer has an ideological leaning which may shape IO output—is a hitherto less appreciated possibility.

We posit that international bureaucracies are neither unitary nor politically neutral: they are staffed by individuals with ideological biases, which manifest in organizational output. In doing so, we advance international-relations scholarship by scrutinizing the 'micro-foundations' of intra-IO operations and uncovering the diverse ideological preferences at the level of the individual that shape the policy output of IOs.[1] Why and when should we expect the ideological preferences of staff to come to the fore of IO decision-making to shape outcomes? Social scientists have long documented how decisions with—direct or indirect—distributional implications force latent ideological preferences to come to the fore (Rosanvallon 2011, Caramani 2017). For Habermas (1970: p. 63), technocratic governance requires a "decision between competing value orders' when choosing specific means for addressing clearly defined goals and needs." The move down the ladder of abstraction—from general-application rules to concrete situations and decisions—means that technocratic experts "have to venture beyond rational choice and the scientific method, and make a value statement that would be indefensible on objective grounds and that would necessarily reflect [their] subjective ideological bias" (Centeno 1993: p. 311). Anticipating this dynamic, Weber (1978) famously understood that only a rigid division of labor between experts and policymakers can safeguard politics from being hijacked by well-placed staff in public administration. However, as we argue here, this Weberian division of labor in IOs is not as rigid as is often assumed. Under which conditions? We posit that this depends on the opportunity structure: instances of strategic importance for the most powerful member-states commonly yield 'high politics' that limit space for staff discretion (Stone 2008, 2011, 2013). Conversely, low-salience, business-as-usual organizational activities evoke limited oversight and maximize staff autonomy (Copelovitch 2010, Lang and Presbitero 2018, Dreher et al. 2022). It is here that

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[1]The literature has only recently begun to study the micro-foundations of IO decision-making. Copelovitch and Rickard (2021) document that IO leaders can be 'partisan technocrats.' Clark and Zucker (2023) show that staff learning explains variation in attention that IOs give to climate change.

we expect ideological leanings of staff to come to the fore and shape policy outcomes.

To empirically study the implications of ideological biases among IO bureaucrats, we focus on the IMF—one of the world's most powerful IOs—and the policy conditions in its loan programs. The IMF is notorious for its firm hierarchical structures and tight control over the types of knowledge and expertise that inform policy, while staff themselves are highly trained experts holding advanced economics degrees from a handful of global elite universities (Chwieroth 2008, 2010, 2013, 2015, Nelson 2014, 2017). These characteristics prompted the IMF's own in-house historian to describe it as a "tidy disciplinarian (both toward itself and others), physically small, nearly devoid of humor, and more interested in gaining respect than in being loved" (Boughton 2001: p. 996), while close observers likened the rigidity of its internal decision-making structures to "the multilateral equivalent of the Catholic Church" (Kapur et al. 1997: p. 622). Consequently, the IMF provides a hard test for our argument. Unlike some IOs with looser structures and more diverse expertise, the IMF provides an instance of *par excellence* streamlined technocratic operations. In addition, the organization always has a single staffer heading operations toward a given country, thus enabling an empirical design that leverages variation in what these individuals do on the job.

We collect individual-level data on these IMF staffers, the so-called mission chiefs (MCs), who have the primary authority for designing the IMF's policy advise vis-à-vis the member-state they are responsible for, whether in the context of lending programs or periodic economic surveillance. Our dataset covers nearly the universe of individuals holding such a post between 1980 and 2016. We follow the career and country deployment of 835 officials over time and, in addition, collect their biographic information. Subsequently, we combine this resource with data on policy conditions the IMF attached to all lending programs of the same period. To determine the ideological leaning of IMF conditionality, we coded 15,790 of these conditions along several dimensions based on their original text from IMF documents.

Our empirical strategy to link individual IMF staff to IMF conditions builds on the 'judge fixed effect' approach. Introduced by Kling (2006) and increasingly applied in recent economics scholarship (see review by Frandsen et al. 2023), this method uses the repetitive assignment of judges and variation in judge leniency to explain sentence lengths in court cases. We import this approach to political science and apply it to the IMF, making use of the fact that many MCs are assigned to multiple member countries over the course of their career (Beaudry and Willems 2022). While IMF conditionality for a country will, to a degree, depend on local economic conditions prevalent, the assignment of MCs across countries allows us to isolate the part of the variation that can be explained by the presence of a given MC.

First, we interrogate the possibility that MC assignment could be endogenous such that ideological leaning of MCs could influence their placement. We find that country-specific

macroeconomic and political fundamentals cannot predict the ideological orientation of incoming MCs. Further evidence from interviews with IMF staff, as well as from Beaudry and Willems (2022) and Clark and Zucker (2023), who use related approaches,[2] suggest that allocation of staff to countries is largely based on routine rotation procedures, seniority, availability, and managerial skills, as well as organizational diversity policies, rather than on a given staffers' perceived ideological preferences. As an endogenous assignment of MCs is thus unlikely, we leverage this institutional feature of the IMF to infer whether and how MCs affect conditionality.

In a first step of the empirical analysis, we find that conditionality varies strongly across MCs and that MC identity explains a substantial share of variation of conditionality across IMF loans. Then, for a more rigorous empirical approach, we estimate each MC's individual proclivity for the scale, scope, and content of conditionality by means of regressions that explain conditionality with MC fixed effects while controlling for borrowing-country fixed effects, time fixed effects, and macroeconomic fundamentals. The coefficients of these MC fixed effects indicate the 'bias' of a given MC; that is, an MC's systematic deviation from the conditionality we would expect the IMF to impose— on average—in borrowing countries that share similar characteristics. This allow us to draw conclusions about their revealed ideological leaning. Applying a 'jackknife' (Angrist et al. 1999) or *leave-one-out* logic, we compute an MC-specific bias measure that is based on all past and future country appointments, while excluding the MC's current country of responsibility to ensure the measure is independent of this country. This way, we can test whether MCs' revealed biases in other countries explain conditionality design in their current country.

Based on this strategy, we study whether the ideological leaning of MCs influences IMF output. We find that pro-conditionality bias of the MC in charge of an IMF program predicts the program's *number* of policy conditions. A one-standard-deviation increase in MC bias translates into 6% or 2.2 additional conditions assigned to the country. We also find that the bias explains an increase in the number of policy areas that IMF programs target. Turning to staff influence over the *content* of conditionality, we find that MC-specific biases also predict the number of conditions in certain policy areas.

For our core result, we use our new data on the ideological leaning of IMF conditionality that encompasses condition-level information on whether a loan condition calls for public spending limits, tax increases, and/or market liberalization. With the help of these data, we, first, estimate the ideological leaning of MCs and, second, examine how their ideological orientation impacts IMF program design. Our results suggest that IMF programs led by MCs with pro-market views are more likely to demand market-liberalizing reforms

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[2]Beaudry and Willems (2022) use variation in forecast optimism across IMF staffers to study the effects of overoptimistic growth forecasts on economic growth. Clark and Zucker (2023) study whether IMF resident representatives influence climate-related language in Article IV reports. Both articles find no endogenous assignment of IMF staff.

and that MCs who have revealed preferences for fiscal adjustment via tax increases in other programs are more likely to demand tax increases in the program they currently lead. We also provide an extension of this finding, drawing on past scholarship that examines the role of educational background in shaping IO staff views and preferences. The evidence suggests that MCs are more likely to demand cuts to public spending and advocate for market-liberalizing policies when they received their economics training at universities whose faculties have a reputation of strong faith in free markets and being skeptical of government intervention.

We then examine the scope conditions of our argument in relation to scholarship on political biases of IOs. Informed by the literature on *geopolitical* biases in IO decision-making (Copelovitch 2010, Stone 2011, Vreeland 2019), we expect the *ideological* biases of IO staff to matter most in cases that are of little political and economic interest to the IMF's major shareholders, whether because they do not entail substantial funding allocations or because they do not involve geopolitical allies. In line with this hypothesis, we find that the ideological leaning of IMF staff matters most in IMF programs under these conditions. The results suggest that the ideological biases of IO staff add to the geopolitical biases that powerful governments introduce. More generally, they cast doubt on prominent images of powerful IOs as cohesive and impartial bureaucracies, instead highlighting the role of individual staffers in shaping their policy output.

We proceed as follows. Section 2.2 provides the theoretical argument on the role of bureaucrats in IOs. Section 2.3 introduces the empirical setting and our main hypotheses. Section 2.4 presents the underlying data on IMF MCs and IMF conditionality. Section 2.5 presents our empirical approach. Section 2.6 presents our findings. Section 2.7 concludes.

2.2 The Power of Bureaucracies in International Organizations

International organizations are centralized bureaucracies with a degree of operational autonomy from the countries that create them (Abbott and Snidal 1998). Both the size of the bureaucratic apparatus and the degree of its autonomy vary across organizations, but many of the world's most important IOs employ thousands of international civil servants who—for the most part—carry out their day-to-day business without direct interference by member governments (Barnett and Finnemore 1999, 2004). Several factors contribute to the operational autonomy of IO bureaucrats. From the perspective of principal-agent theory, IO staff are the ultimate agents in a chain of nested principal-agent relationships starting with citizens in member countries as ultimate principals via their governments, IO governing boards and IO leaders (Nielson and Tierney 2003, Vaubel

2006). This long chain of delegation increases the likelihood of 'agency slack,' as many delegating actors are involved (Olson 1965). Further, political bodies within IOs that represent member interests typically form 'collective principals' (Nielson and Tierney 2003) with heterogeneous preferences (Copelovitch 2010, Schneider and Tobin 2013), thereby diluting supervision lines for the IO bureaucracy. To compound these effects, physical distance between principals and agents is often large, information asymmetry between them is substantial, and staff's decision-making commonly lacks transparency (Vaubel 2006, Dreher et al. 2022). These factors allow ample space for staff to influence organizational output according to their own preferences which may be distinct to those of their principals.

The idea that IO staff can systematically bias IO decision-making has primarily been approached through the lenses of public choice theory and organizational sociology. The standard argument in the former literature is that IO bureaucrats face incentives to engage in 'rent-seeking' (see, e.g., Vaubel 1986, Willett 2002, Dreher and Vaubel 2004, Dreher and Lang 2019). In the words of Vaubel (1996: p. 195), "there is little else [staff] can agree on but to pursue their common bureaucratic interest." Motivated by their interest to increase their "power, prestige, and amenities" (ibid.), they seek to expand budgets and responsibilities for their organization. Doing so may bias organizational output away from official mandates.

Organizational sociology accounts go beyond narrow self-interest and take political goals of staff into account. A common worldview among IO staff is, in this argument, favorable to the development of a distinct business culture that may meaningfully shape organizational output (Frey and Gygi 1989, Barnett and Finnemore 1999, 2004, Nelson 2014). For example, in the case of the IMF, employees are mostly trained economists who share a similar view on economic policymaking and have both few incentives and little room to challenge existing institutional views, thereby entrenching institutional culture (Chwieroth 2007, Chorev and Babb 2009, Vetterlein 2010, Woods 2014). In support of these arguments, scholars have documented favorable IMF treatment of member countries whose government officials share the political ideology and educational background of the IMF's bureaucracy (Woods 2006, Chwieroth 2013, Nelson 2014).

Both public choice theory and organizational sociology show how staff can bias policies away from official IO mandates and are thus related to our argument. However, both approaches treat IO bureaucracies as homogeneous actors with collective interests. The idea that ideological heterogeneity among IO staff may explain differences in IO output has not been comprehensively considered.[3]

If individual bureaucrats have an independent influence on IO policy output, what are the conditions under which this influence is strongest? We draw a distinction between

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[3]The approach that comes closest to ours is that of Clark and Zucker (2023), who argue that exposure to climate-vulnerable countries makes staff more attentive to climate change.

issues of high and low salience for member-states. High salience issues are of core concern to all members—for instance, pursuing a treaty change—or of particular importance to one or more very powerful members—for instance, linked to the strategic interests of the United States. In these cases, member-states use both formal and informal means to influence IO decision-making, and are likely to tightly supervise the actions of IO staff (Stone 2008, 2011, 2013, Lang and Presbitero 2018, Dreher et al. 2022). Indeed, there is ample empirical evidence to support this argument (e.g., Thacker 1999, Oatley and Yackee 2004, Stone 2004, Dreher et al. 2009, Henning 2009, Kilby 2009, Dreher and Sturm 2012, Carter and Stone 2015).

In contrast, in the case of low-salience issues—that is, the bulk of day-to-day functioning of an IO—the largely autonomous international bureaucracy is ostensibly there to guarantee the impartial application of its expert knowledge (Stone 2008, 2011, 2013). In other words, most of the time and in most cases, IO staff are left with considerable leeway to pursue their duties according to formal rules and legitimated expertise, with only periodic ex-post reviews of their performance (Martinez-Diaz 2009). Here, again, IO staff are seen as largely homogeneous actors that serve as bulwarks against political biases that member-states may try to introduce (Stone 2008). But it is in these low-salience instances, precisely because they attract less oversight and attention, that individual staffers may seek to advance their own political preferences.

2.3 Bureaucrats and Global Policies: The Case of the IMF

Our analysis builds on a strand of recent scholarship that unpacks the role of individuals in IOs, primarily in the field of global economic governance. In the context of the World Bank, scholars have documented that ability and experience of Bank staff shape the performance of projects that they manage and recipients they work with (Heinzel and Liese 2021, Limodio 2021, Moscona 2023). In the context of the IMF, Beaudry and Willems (2022) document that IMF staff differ in their degree of optimism in economic forecasts and that overoptimism increases the likelihood of recessions. Clark and Zucker (2023) show that climate topics feature more prominently in a country's economic surveillance reports when the responsible staff member previously worked on climate-vulnerable states. Copelovitch and Rickard (2021) find the IMF to demand more market liberalizing reforms when the IMF is led by a right-leaning Managing Director.

We extend this nascent literature with an in-depth assessment of how the ideological leaning of rank-and-file IMF staff influences policy choices. This empirical setting is selected for both substantive and theoretical reasons. Substantively, the IMF has extensive power in the design of economic reforms for its borrowing countries, commonly under-

going deep economic crises (Vreeland 2007). As these policies have direct and profound distributional consequences for millions around the world (Vreeland 2003, Dreher 2006, Rickard and Caraway 2018, Lang 2021), it is important to understand how they are devised in the first place. In terms of advancing theory in political science, analyses of the IMF have become a battleground for competing explanations on the functioning of international organizations. In line with a Lakatosian view of scientific progress, the analysis of crucial cases that have received extensive academic attention can enable theoretical innovation (Lakatos 1970).

To trace the 'micro-foundations' of IMF decision-making, we synthesize insights from different strands of scholarship. First, our argument aligns with views that the political preferences of individuals in the IMF matter (Copelovitch and Rickard 2021), but follows this logic down to the level of line staff, rather that of organizational leadership who are more overtly political figures. Second, we draw on accounts showing the ideological ('neoliberal') bend of IMF staff (Chwieroth 2008, 2015, Nelson 2014, 2017), but relax the assumption that such views are equally held by all. Third, we highlight how low-salience issues for member states (Stone 2013) are also high-autonomy issues for IMF staff, which opens up the possibility for their own individual preferences to seep through, thereby not strictly adhering to their mandated impartiality. In pursuing these avenues, we thus advance theoretical and empirical understanding of intra-organizational, individual-level determinants of IO decision-making, and how these shape ultimate outputs.

To test our argument, we focus on a core function of the IMF: providing funding to countries in need provided they implement policy reforms, known as conditionality. When a country turns to the IMF, an MC leads a small team that negotiates for the organization and designs the precise policy content of conditionality. MCs are mostly professional-level staff drawn from an appropriate regional department who take on responsibility for IMF operations within a country. The tenure in such posts is generally about two or three years, and most staff hold such appointments in multiple countries throughout their career.

The IMF's own Guidelines on Conditionality specify that conditions should be designed on the basis of objective criteria that "ensure *consistency* in the application of policies relating to the use of [IMF] resources with a view to maintaining the *uniform treatment of members*."[4] In principle, MCs are supposed to be neutral and follow the organizational line. The IMF's own guidelines state that officials "should strive to be non-partisan and politically non-interventionist."[5] These conditions are then clad with the expert authority of highly-trained IMF staff and presented as 'objectively correct' policy paths, thus feeding a 'there is no alternative' narrative (Séville 2017).

The conditionality attached to IMF lending offers an optimal opportunity to ascertain

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[4]https://www.imf.org/External/np/pdr/cond/2002/eng/guid/092302.pdf, emphasis added.
[5]See https://www.imf.org/external/np/cso/eng/2003/101003.htm.

possible individual-level biases among MCs. It is a key moment where their political preferences may become observable: being MC endows these staffers with wide-ranging power to design what they consider an appropriate reform package, thus selecting certain policies rather than others. In short, if IMF staff have ideological leanings that shape their decisions, it is in their capacity as MC that they can most overtly put these biases in practice and affect real changes in the policy environments of the IMF's borrowers—a level of power few other international civil servants have.

To be sure, part of the variation in the number and type of conditions the IMF requests from a prospective borrower will depend on well-established factors, like the scale of macroeconomic difficulties or geopolitical considerations. But we posit that another part can be explained by the respective MC who oversees the program. Some MCs may have more hawkish fiscal preferences or favor more aggressive tax policies than others. Similarly, some MCs may forcefully push for market liberalization—for example, through privatizations and labor market reforms—when others may shy away from such measures. It is these types of variation that we are interested in, and they are all observable, as we explain below.

We test several implications of our argument. The first step of our analysis—an initial 'proof of concept' test—is to examine whether there are systematic differences in IMF policy design when different MCs are involved:

H_1 : *The design of IMF conditions differs systematically across mission chiefs.*

From this analysis, we derive different measures of the policy preferences of MCs and then test whether these measures predict certain aspects of IMF conditionality. First, using measures of IMF conditionality that follow the related literature (Stone 2008, Dreher et al. 2015, Kentikelenis and Stubbs 2023), we expect that individual MCs will differ in terms of quantity of conditions they impose and the overall scope of conditionality; that is, the number of policy areas under reform.

H_2 : *The individual preferences of IMF mission chiefs affect the quantity (H_{2a}) and scope (H_{2b}) of IMF conditions.*

The hypotheses thus far all pertain to *indirect* ways for examining the ideological leaning of MCs. We now shift gear to more directly capture the ideological leaning of MCs by coding the conditions they design according to whether they advance spending cuts, tax hikes or market liberalization. Correspondingly, we expect that:

H_3 : *The individual preferences of IMF mission chiefs affect the ideological leaning of IMF conditions.*

While we expect the influence of MCs on the design of policy conditionality to be considerable, the scope conditions of our theory imply that their influence is smaller when it comes to high-salience issues, as outlined above. In these cases, we expect geopolitics to trump bureaucratic influence. Only in low-salience cases, where major shareholders have no strong interests in intervening in organizational decision-making,

do we expect bureaucrats to be able to decisively influence policy design. This is in line with comments from an IMF staffer, who explained that lending to large countries is always high-stakes for the organization: "consider loans to Argentina or Ukraine—these are always very strategic and political, and a lot of money is on the line. MCs have more space to act in countries where the IMF has less exposure—smaller and poorer countries."[6] Consequently, we posit that:

H_4 : *The influence of IMF mission chiefs on conditions is weaker when strategic political interests of the IMF's largest shareholders are at stake.*

2.4 Data

2.4.1 IMF Staff

We test these hypotheses with original data on 835 IMF staffers. The data we collect follow their career as MCs within the IMF. For the 1980-2016 period, we track the assignment of MCs in IMF programs for 131 different countries. Of the 835 MCs we observe, 339 go on more than one mission and 261 go on more than two missions; some are in charge of up to 30 countries over the course of their career. As an example, Figure 2.1 visualizes these assignments to countries with ongoing loan programs in the IMF's Western Hemisphere Department.[7] The numbers in the boxes contain the identifiers of individual MCs, and we highlight some to visualize exemplary career paths. Mission chief `MC315`, for instance, was first assigned to Nicaragua in 2005-07 and then to Mexico in 2009-10, a common career path in terms of assignment length and country sequence (from smaller to larger within the same regional department). `MC374` shows that an MC can be responsible for several countries at the same time or in quick succession (Costa Rica, Guyana, El Salvador, and Panama across the 1990s). And `MC809` had served on two prolonged missions to Dominica (1984–1988) and Jamaica (1991–1994), before being sent to Panama (1997–1998) and Guyana (1999–2000), showing that assignments sometimes lasted longer in the past.

In addition to these deployment patterns, we collected biographical data on MCs. We coded their year of birth, gender, nationality, education (highest academic degree and alma mater), and year of joining the Fund. Table 2.A.1 in the Appendix summarizes the data. About three-quarters of MCs have a PhD (73%), and almost all MCs were either educated in the United States (59%) or in Europe (32%). In terms of nationality, 58% of MCs hail either from the United States or Europe. Only 15% of MCs are women but this share increased in recent years (22% of MCs in 2010-2016).

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[6]Interview on April 13, 2023; granted on condition of anonymity.

[7]While our data also cover those MCs that are responsible for countries without ongoing programs—they are responsible for economic surveillance—we exclude them here for reasons of clarity. The dataset available as supplementary material includes all 835 MCs.

Figure 2.1 – Assignment of IMF mission chiefs on lending programs
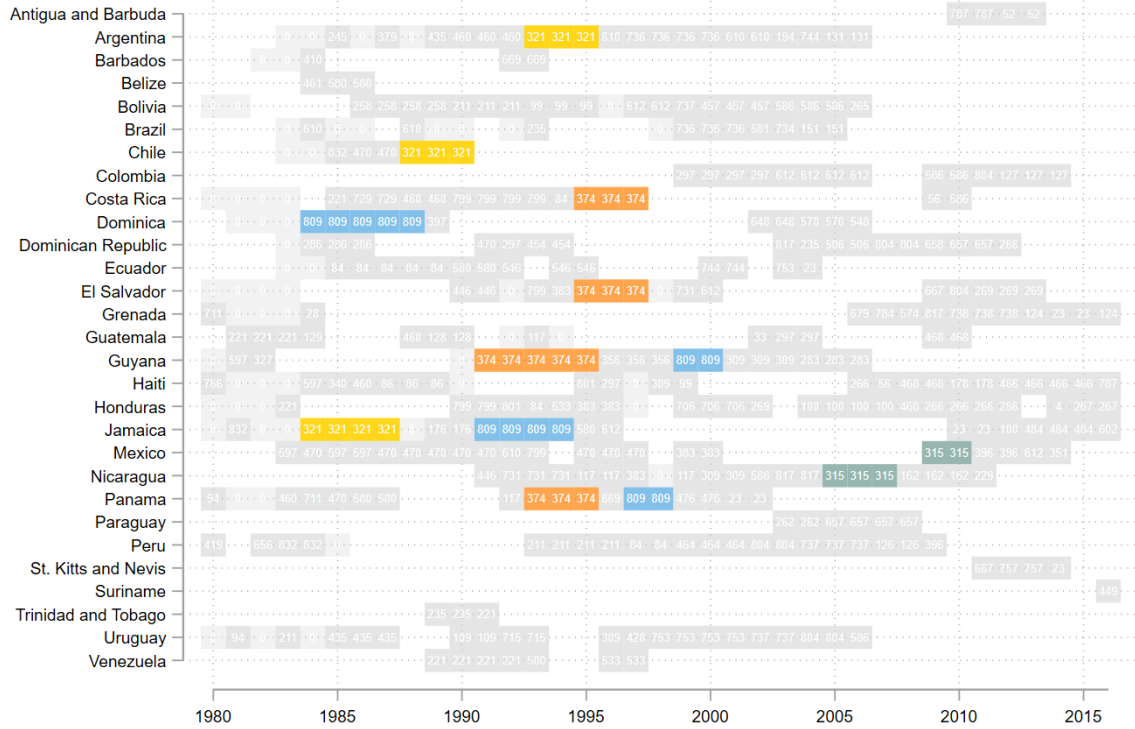
Notes: This figure shows the deployment of Mission Chiefs on IMF missions within countries of the Western Hemisphere department from 1980 to 2016. Numbers identify the respective MC. We highlight the career paths of four exemplary MCs: 315 (green), 321 (yellow), 374 (orange), and 805 (blue). The remaining IMF program years are marked in gray. IMF program years with missing information on the MC are marked in light gray.

We collected these data from various sources. Beaudry and Willems (2022) provide data on country deployments of 705 IMF MCs, relying on IMF travel reports and staff reports following country surveillance missions, where reoccurring semantic structures allowed text mining the names of the MCs. Since their coverage is not complete for our observation period, we expanded this dataset based on information contained in IMF staff reports for lending agreements or surveillance missions. Either the reports explicitly listed the MC, or the semantic structure gave away the MC's name, which we could extract through a text-analysis algorithm. In addition, we directly contacted IMF Country Offices and obtained detailed information on the MC deployment in the respective country. While some data for the early 1980s are not detectable, our data cover almost the universe of MCs from the mid-1980s onward. Out of 1,285 country-year pairs with an active IMF program after 1990, our final data cover all but 38 (97% coverage).

We collect MC biographical information from various sources. Data on IMF staff for 1980-2000 by Nelson (2014) includes information on many MCs, based on CVs collected at the IMF. We combine these data with biographical information that we gathered from

official IMF sources and on LinkedIn. Where data were still missing we used an extensive web search to fill up missing data points. Figure 2.A.1 in the Appendix illustrates the relative share of each source over time. We were able to collect information on education for 639 MCs and on nationality for 561 MCs. We coded gender by using a gender-prediction algorithm based on U.S.Social Security Administration baby name data. We do not use all data that we collected in this paper but publish all variables as supplementary material.

2.4.2 IMF Policies

To study the ideological valence of policies pursued by IMF MCs, we use data on 32,261 individual loan conditions collected by Kentikelenis and Stubbs (2023). The data cover the universe of conditionality attached to all 744 IMF loans over the 1980-2016 period. Based on these data and as anticipated in the Hypotheses section, we construct four measures of conditionality for each year of each IMF program. First, we calculate a simple count of the number of conditions per program-year as a measure of the *extent* of conditionality, following scholarship that quantifies the overall burden of adjustment required by an IMF program (e.g., Clark and Meyerrose 2023). Second, we make use of the fact that the conditions apply to various policy areas. The number of policy areas is often considered a more appropriate measure of the intrusiveness of conditionality compared to a simple count (Stone 2008, Dreher et al. 2015). This measure, usually called the *scope of conditionality*, considers a program that demands one reform each in five different policy areas to be greater in scope than a program that demands six reforms in one area.Third, we code a variable for each policy area that indicates the number of conditions *per policy area*. This set of variables allows examining which policy areas MCs are most likely to influence.

Finally, and most importantly for our purposes, we code new measures that indicate the *ideological leaning of individual policy conditions*. To construct these, we extracted the original text of 15,790 conditions and—per the codebook in the Appendix—coded their content along three dimensions: limits to public spending, increases of public revenue, and pro-market conditions. For each of these, we code three binary and non mutually-exclusive variables that indicate whether or not the respective condition falls into the category:

1. *Spending limits* indicate whether or not conditions conceivably ask for a reduction in public spending in the program country. Typical IMF conditions that fall into this category demand, *inter alia*, limits on the budget deficit or cuts to the public sector wage bill.

2. Increases of public revenues through *tax increases* or through reductions of tax

exemptions constitute an alternative approach to consolidate public finances. While spending limits tend to decrease a state's public spending ratio, tax increases tend to increase it. The two types of conditions thus map onto different ideological perspectives on the role of the state in the economy.

3. *Pro-market conditionality* includes conditions that are explicitly designed to expand the remit of private markets in the economy of the member country. Typical IMF conditions of this kind demand privatizations of state-owned enterprises or liberalization of labor laws.

For the empirical analysis, we code three variables that count the conditions per category that are applied in a given year to an IMF borrower.

2.5 Empirical Strategy

Our empirical strategy for identifying whether there is a connection between MCs and IMF conditionality follows the 'judge fixed effects' approach, which received its name from Kling (2006), who uses differences in judge leniency to explain variation in incarceration lengths. We apply the judge fixed effects approach in the context of the IMF, where we test whether different revealed preferences of IMF staff explain some of the variation in conditionality. Our strategy is inspired by Beaudry and Willems (2022), who use repeated MC assignment to study overoptimism in IMF growth forecasts.

Just like judges are assigned to multiple cases, IMF MCs are assigned to multiple countries over the course of their careers. We exploit this repeated assignment of MCs and the special institutional setup of the IMF to link each MC to conditionality attached to lending programs. In Figure 2.2, we provide descriptive evidence on the sort of variation that we study. The figure shows the average number of conditions per MC, and reveals substantial variation across MCs. When compared to the average IMF program, some MCs apply up to 20 fewer conditions while others apply up to 30 more conditions than the average IMF program.

This descriptive evidence already suggests that conditionality differs across MCs, but we scrutinize this more rigorously in a regression-based framework. We begin by regressing a measure of conditionality on MC fixed effects that indicate which MC is assigned to country i in year t:

$$IMF conditionality_{i,t} = \alpha_t + \delta_i + \sum_{k=1}^{K} \gamma_k MC_{i,t}^k + X'_{i,t}\eta + \epsilon_{i,t}. \tag{2.1}$$

$IMF conditionality_{i,t}$ indicates the logged number of conditions that program country

Figure 2.2 – Conditions and mission chiefs
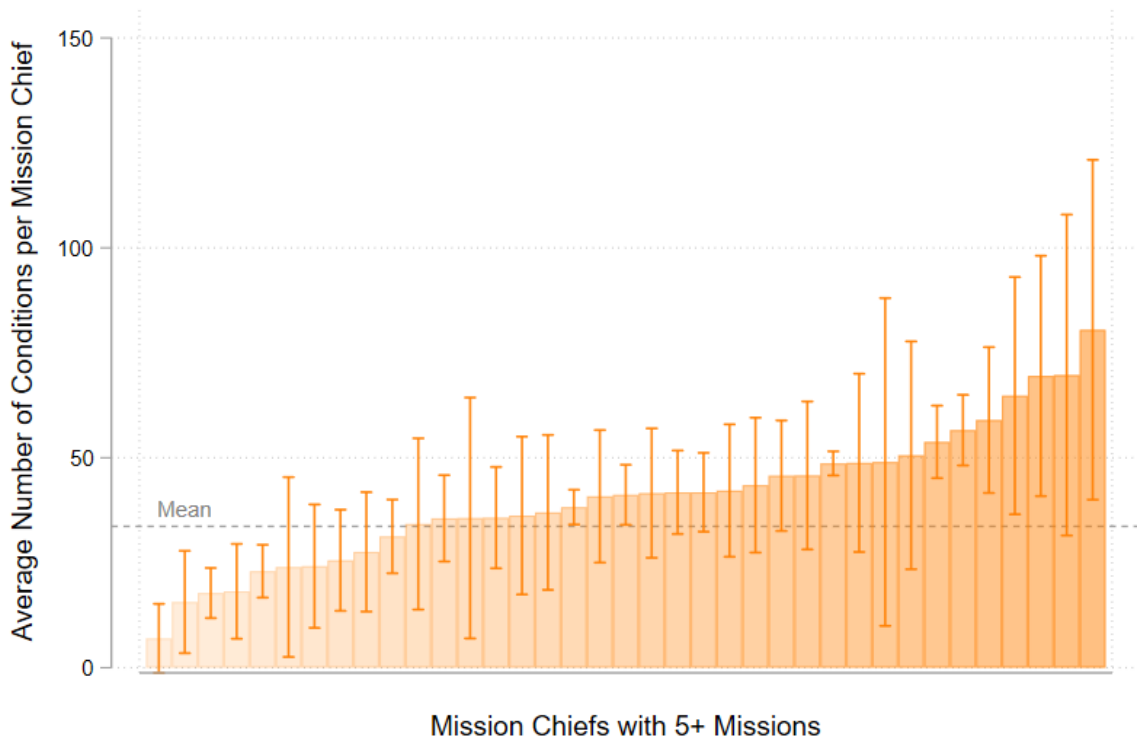
Notes: This figure shows the mean number of conditions per MC–year. For simplicity, we only include MCs with more than 5 missions. 95% confidence intervals.

i receives in year t.[8] The regression includes year fixed effects, α_t, country fixed effects, δ_i,[9] and a set of time-varying control variables $X'_{i,t}$.[10] The set of K binary variables $MC^k_{i,t}$ indicate whether mission chief k is assigned to country i in year t ($MC^k = 1$) or not ($MC^k = 0$). Here, we use the full set of 835 MCs. Estimating the coefficients γ_k in this model yields the 'mission chief bias' of each mission chief k. That is, the γ_k's estimate the extent to which conditionality under mission chief k deviates from what would have been expected based on the country's mean conditionality, the yearly mean conditionality and variables capturing the economic conditions in the country.

As a first step, we estimate these γ_k's and test whether they are jointly statistically significantly different from zero. If they are, we can conclude that conditionality systematically differs across MCs, after conditioning on unobserved time-invariant country characteristics, global time trends, and country-year-specific macroeconomic fundamen-

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[8]Our results remain similar when using the absolute number of conditions, or when we avoid outliers by winsorizing the number of conditions at 99%, as we show in Table 2.E.2.

[9]Our results remain robust even when introducing world region×year fixed effects in eq. 2.1, as we show in Table 2.E.2.

[10]These include GDP (ln), population (ln), trade (% GDP) and binary indicators for debt crises, currency crises, banking crises, and sovereign debt restructurings following the definitions from Valencia et al. (2008) and derived from Laeven and Valencia (2020). Throughout the paper, we refer to these as macroeconomic controls.

112

tals. We also estimate how the share of explained variation in conditionality changes when adding and removing MC fixed effects.

While this approach gives a first indication of whether conditionality depends on MCs, a limitation is that the MC-specific γ_k's capture variation in conditionality from all countries the MC ever was assigned to, thus including previous years of the IMF program in the country whose conditionality is to be explained. We circumvent this potential source of endogeneity by using a two-step jackknife or *leave-one-out* approach based on MC's previous and future appointments in countries $j \neq i$, following the logic of the jackknife-IV estimator (JIVE) (Angrist et al. 1999). Specifically, we first estimate equation 2.2 i times, dropping each country i once:

$$IMF conditionality_{j,t} = \alpha_t + \delta_j + \sum_{k=1}^{K} \gamma_i^k MC_{j,t}^k + \epsilon_{j,t}, \forall j \neq i. \tag{2.2}$$

We then standardize and store the estimated $\hat{\gamma}_i^k$ in a new variable $MCbias_{i,t}^k$, such that the value of this variable for country i in year t equals the $\hat{\gamma}$ of the active mission chief k that is estimated from equation 2.2 while excluding country i:

$$MCbias_{i,t}^k = \begin{pmatrix} \hat{\gamma}_{1,1}^1 \\ \vdots \\ \hat{\gamma}_{i,t}^k \\ \vdots \\ \hat{\gamma}_{I,T}^K \end{pmatrix}. \tag{2.3}$$

Intuitively, the variable $MCBias$ thus indicates the MCs' bias in all their other assignments while excluding their current assignment. For example, consider `MC231`, who was responsible for Jamaica (1984–1986), Chile (1988–1990), and Argentina (1993-1995). For their time in Argentina, the variable $MCbias$ indicates an estimate of their bias estimated from their time in Jamaica and Chile. If, during their tenure, Jamaica and Chile received more conditions than the fully specified two-way fixed effects model 2.2 would predict the value of $MCbias$ is positive, while it would be negative if they received fewer-than-expected conditions. Respectively, for their time in Argentina, the variable $MCbias$ indicates an estimate of their bias estimated only from their time in Jamaica and Chile.[11]

In the second step, we then use the variable $MCbias$ (whose values are obtained from all missions of MC k outside country i) to predict conditionality in country i:

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[11]Note that for this exercise, we can only use MCs with two or more missions. To avoid that $MCbias$ stems from only one other mission, we only include MCs with more than two missions. This leaves us with a set of 198 MCs who lead missions into 112 countries in our final sample. We show in Table 2.E.1 that this does not affect our results, and that the results also hold when further restricting the sample to MCs who went on four country missions or more.

$$IMFconditionality_{i,t}^k = \alpha_t + \delta_i + \beta MCbias_{i,t}^k + \epsilon_{i,t}. \qquad (2.4)$$

This allows us to estimate the effect of the active MC's bias in past and future missions on the number of conditions in the current mission. Following the standard approach in this literature, we cluster standard errors at the level of MC and country, which is the level of the treatment assignment.[12]

For our empirical estimates to be unbiased, the IMF should not systematically assign MCs to specific types of countries based on their ideological leaning. We investigated and excluded this possibility. First, we systematically studied the assignment of MCs and tested whether lagged program country characteristics predicted the ideological leaning of incoming MCs (see Appendix 2.D). We did not find any systematic associations, supporting the identifying assumption that an endogenous assignment of MCs is unlikely. Second, we interviewed IMF staff on the MC assignment process. Interviewees pointed out that the selection process follows primarily a bureaucratic procedure handled by the chief of a division of a regional department and human resources officers. An individual staffer needs to be at the appropriate career stage and have spent some time in the regional department to gain knowledge of the main issues affecting a region. In addition, cycles of IMF economic surveillance or lending shape this process, as—for instance—an MC is unlikely to be replaced at the beginning of negotiations on a loan or right before conducting a surveillance mission. MC candidates also need to not be perceived as having ties to their country of appointment, and there are considerations of diversity in terms of gender, race, and nationality that play into the selection process. This matrix of factors is hard to predict, thus contributing to what one current IMF staffer described as a "high degree of randomness."[13] Third, our approach draws on the empirical strategy developed by IMF insiders Beaudry and Willems (2022), who leverage the quasi-exogenous source of variation in MC assignments in a similar context. They defend this assumption by describing the bureaucratic assignment process in a similar way as our interviewees and argue that it is unlikely that IMF decision makers even have detailed knowledge on differences in MC-specific views and preferences as the IMF's recruitment policies are intended to yield ideologically homogeneous staff (IMF 1999, Momani 2005).

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[12]This strategy of two-way clustering is usually applied in the context of the judge fixed effect. We show in Table 2.E.3 that our results are robust to clustering standard errors at different units. In this context, note that the jackknife procedure creates variation within the individual MC, which derives from the mission to country i that is dropped when estimating the bias in eq. 2.2.

[13]Interview on April 12, 2023; granted on condition of anonymity.

2.6 Results

We present our results in the following order. Section 2.6.1 estimates how much of the variation in IMF conditionality is explained by the identity of the MC. This estimation also serves as the basis for the first step of the jackknife IV approach applied in the subsequent sections. Section 2.6.2 uses this approach to estimate whether there is an empirical link between MC preferences and the scope of IMF conditionality. Section 2.6.3 reports whether MC preferences explain variation in the policy areas that IMF conditions cover and section 2.6.4 turns to their ideological leaning. Section 2.6.5 examines the role of MC education in their ideological leaning. Section 2.6.6 provides evidence on the scope conditions of our argument by showing that the expected patterns are only observable under the circumstances that our theory predicts.

2.6.1 Mission Chief Fixed Effects

As a first step, we test whether the identity of the responsible MC explains variation in IMF conditionality. To do so, we regress the logged count of IMF conditions on the set of MC fixed effects and test whether the coefficients on these dummy variables are jointly different from zero. Only MCs who participate in more than two missions are included.

Table 2.1 presents the results for a test of joint significance. As indicated by p-values of Wald F-tests of joint significance, the MC fixed effects are jointly significantly different from zero with a confidence level larger than 99.99%. What is more, with $R^2 = 0.35$ they explain a substantial share of the variation in the number of IMF conditions. The MC fixed effects remain significant when introducing year fixed effects in column 2, country fixed effects in column 3, world-region×year-fixed effects in column 4,[14] and the aforementioned set of country-year specific macroeconomic controls in column 5. The R^2 statistics show that the full specification would explain 44.4% of the variation in IMF conditionality without the MC fixed effects. Adding the information on who serves as MC increases the share of the explained variation to 60.9%.

In the first rows of the table, we also report the estimated coefficients of six exemplary (out of 835) MC dummies included in these regressions. As the labeling of MC dummies follows the size of the estimated coefficients in column 5 in ascending order, the table reports the extreme values of the estimated coefficients.[15] For approximately 80% of MCs, coefficients lie between –0.6 and +0.7, implying deviations from halving the number of expected conditions to doubling it. These results suggest that the differences across MCs that were visualized descriptively in Figure 2.2 above are also visible in a regression-based

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[14]We define world regions per the IMF Regional Departments: Asia and Pacific, Europe, Middle East and Central Asia, Sub-Saharan Africa, and Western Hemisphere.

[15]For the labeling of MCs, coefficients that could not be estimated because of insufficient relevant observations are treated as zeros.

framework with fixed effects and controls.

Table 2.1 – IMF conditionality and mission chief fixed effects

| | All conditions *(ln)* | | | | |
	(1)	(2)	(3)	(4)	(5)
MC 1	0.47	0.41	0.11	-1.50	-1.89
	(0.34)	(0.36)	(0.34)	(0.74)	(0.76)
MC 2	-1.07	-1.18	-1.64	-1.58	-1.61
	(1.29)	(1.36)	(1.03)	(0.85)	(0.53)
MC 3	-0.73	-0.72	-1.20	-1.40	-1.41
	(0.83)	(0.88)	(0.70)	(0.65)	(0.32)
⋮	⋮	⋮	⋮	⋮	⋮
MC 833	0.13	0.25	0.67	1.15	1.18
	(0.51)	(0.56)	(0.55)	(0.65)	(0.57)
MC 834	0.20	0.43	0.02	2.65	2.09
	(0.34)	(0.43)	(0.50)	(0.65)	(0.73)
MC 835	-0.02	0.40	2.41	3.22	2.57
	(0.64)	(0.42)	(0.42)	(0.59)	(0.56)
Wald p-value (mission chief dummies)	0.00000	0.00000	0.00000	0.00000	0.00000
Observations	1152	1152	1152	1152	1110
Year FE	.	✓	✓	✓	✓
Country FE	.	.	✓	✓	✓
Region-year FE	.	.	.	✓	✓
Macroeconomic controls	.	.	.	.	✓
R^2	0.330	0.388	0.525	0.606	0.609
R^2 w/o Mission Chief FE	0.000	0.166	0.352	0.441	0.444

Notes: The table estimates the model specified in eq. 2.1. It reports Wald F-statistics and p-values for joint significance tests of the MC fixed effects.

2.6.2 Mission Chiefs and the Scope of Conditionality

Having shown that IMF conditionality varies across MCs, our next step is a more conservative approach. Here, we estimate whether the preferences for conditionality that MCs revealed in their missions in countries $j \neq i$ help predict conditionality in their current country i. To this end, we implement the two-step jackknife procedure that was discussed above. In the first step, we estimate coefficients on MC fixed effects as in Table 2.1 but drop each country once. In a second step, we use these estimated coefficients which are stored in the variable *MC bias* and which indicate the respective MC's revealed preferences for conditionality in countries $j \neq i$—to explain IMF conditionality in country i.

Table 2.2 presents the results. Models 1 and 2 demonstrate that the *MC bias* variable is positively and statistically significantly associated with the logged number of conditions. *MC bias* is *z*-standardized such that the coefficients imply the following effect size: MCs whose record of IMF conditionality in other countries is one standard deviation above

the mean, on average, set 6-7% more conditions in their current IMF program than the mean MC. This amounts to about 2 additional policy conditions at the mean.

The same pattern is visible when turning to the *scope of conditionality* in models 3 and 4. Here, the outcome variable is the *number of policy areas* of the 12 different sectors that IMF conditionality covers. Again, the respective MC bias has predictive power of the scope of conditionality. MCs, who design programs that cover a wider range of policy areas in other countries are also likely to cover more policy areas in the country they are currently assigned to.

Table 2.2 – Jackknife estimation: extent and scope of conditionality

	Number of conditions *(ln)*		Scope of conditions	
	(1)	(2)	(3)	(4)
MC Bias	0.062**	0.066**	0.218**	0.252***
	(0.027)	(0.026)	(0.087)	(0.086)
Observations	1121	1110	1121	1110
Macroeconomic controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Region-year FE	.	✓	.	✓
R^2	0.366	0.448	0.529	0.595
Mean DV	3.471	3.472	6.144	6.145

Notes: The table shows the results from eq. 2.4. The respective dependent variables are indicated in the first row. Standard errors clustered two-way on MC and country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.6.3 Mission Chiefs and the Content of Conditionality: Policy Areas

Having shown that MCs can influence the quantity of conditions, we turn to their content next, a more direct test for our argument on how MC ideological leaning might affect the policies they design. Initially, we examine whether the policy areas IMF loans target depend on MC idiosyncratic preferences. Indeed, IMF programs can demand reforms in multiple policy areas in a borrowing country: while some areas are covered by nearly all programs (e.g., debt management), others are only targeted occasionally (e.g., state-owned enterprise privatization). To test the effect of MCs on different policy areas, we use data that assigns IMF conditions to mutually-exclusive policy areas (Kentikelenis and Stubbs 2023).[16] Table 2.F.1 provides an overview of the total number of conditions by policy area over the sample period.

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[16]While Kentikelenis and Stubbs (2023) code 13 granular policy areas, we group some of them for our purposes: *Fiscal* sums *Fiscal Issues* and *Revenue and Tax Issues*; *Privatization* sums *SOE Privatization* and *SOE Reforms and Pricing*; *Social & Labor* sums *Labour Issues* and *Social Policy*. We provide an overview of the definitions of the policy areas in Table 2.B.1 in the Appendix.

As we show in Figure 2.F.1 in the Appendix, nearly all IMF programs include conditions on debt, finance, fiscal issues, and the external sector (trade and the exchange system). In contrast, the inclusion of conditions that target privatization or social and labor market policy is rarer and it is thus plausible to expect MCs leave their mark in such policy areas. Table 2.3 presents our estimation to test this, following the baseline jackknife analysis (of Table 2.2) but only using the logged number of hard conditions in the specific policy area.[17]

We do not find an effect in the policy areas of debt, finance and the external sector. These conditions are present in standardized format in most IMF programs, and their number does not significantly change with the individual MC. As expected, the opposite is the case when looking at policy areas with wider discretion for MCs. For conditions that target privatization or social and labor market policy, we find statistically significant effects of our respective MC bias measure on the number of binding conditions in these policy fields. Even though fiscal issues—both spending cuts and tax increases—are included in more than 80% of IMF programs, we also find that such conditions are highly responsive to MC bias. Our theoretical considerations lead us to suspect that this might be due to the fact that different approaches to fiscal policy map most directly onto differences in political ideology, and we turn to this issue next.

Table 2.3 – Jackknife estimation: policy areas

	Debt	Financial	Governance	External	Privatization	Austerity
	(1)	(2)	(3)	(4)	(5)	(6)
MC Bias	-0.014	-0.005	0.078***	0.037	0.061**	0.076**
	(0.041)	(0.039)	(0.029)	(0.025)	(0.031)	(0.030)
Observations	1110	1110	1110	1110	1110	1110
Macroeconomic controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Region-year FE	✓	✓	✓	✓	✓	✓
R^2	0.339	0.371	0.555	0.684	0.453	0.419
Mean DV	2.011	1.664	0.638	0.964	0.359	0.201

Notes: This table shows the results from eq. 2.4, only including conditions in the respective policy area. Standard errors two-way clustered on MC and country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.6.4 Mission Chiefs and the Ideological Orientation of Conditionality

We rely on our newly coded data on the ideological leaning of IMF conditionality to directly test whether MCs influence it. Going through all conditions in policy areas where

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[17]We focus on 'hard' conditions from here on because we are interested in the content of conditions that are politically binding and therefore most relevant (see the discussion in Rickard and Caraway 2014). Failure to meet 'soft' conditions, does not disqualify the country from receiving the next tranche of the IMF loan.

we would expect ideological differences over appropriate policy reforms to manifest most clearly, we categorized each condition along three dimensions: limits to public spending ('spending limits'), increases of public revenues ('tax increases'), and conditions that expand the reach of markets ('market-oriented'). We follow previous analyses and re-estimate the same models while using the logged number of these conditions as the respective outcome variables.

Table 2.5 presents the results. We do not find a significant effect of the MC's bias on the ideology measure for spending limits. We do, however, find significant effects when looking at both tax increases and market-oriented conditions. These results mirror previous findings in that MCs seem to have most leeway when conditions go beyond the standard IMF toolkit: while some level of spending limits is very commonly included in more than 75% of IMF programs, tax increases and market-oriented conditions are used less frequently.[18] The ideological preferences of the MC seem to be an important determinant of whether or not they are applied: A one standard deviation increase in the MC's ideological preference for tax increases in other missions is associated with a 11.3% increase in the number of tax increasing conditions in the current mission. Similarly, a one standard deviation increase in the pro-market bias of MCs leads to 5.3% more market-oriented conditions in IMF programs. These results provide the most direct evidence for our hypothesis that MCs are able to influence the content of IMF conditions according to their ideological leaning.

Table 2.4 – Jackknife estimation: ideological content of conditions

	Spending limits	Tax increases	Market-oriented
	(1)	(2)	(3)
MC Bias	0.000	0.120***	0.051**
	(0.039)	(0.034)	(0.023)
Observations	1110	1110	1110
Macroeconomic controls	✓	✓	✓
Year FE	✓	✓	✓
Country FE	✓	✓	✓
Region-year FE	✓	✓	✓
R^2	0.602	0.558	0.458
Mean DV	0.904	0.322	0.252

Notes: The table shows the results from eq. 2.4, only including conditions with the respective ideological content. Standard errors clustered two-way on MC and country level: *** p<0.01, ** p<0.05, * p<0.1.

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[18]Again, see Figure 2.F.1 in the Appendix for an overview of the probability of a specific ideology to be present in an IMF program.

2.6.5 Ideological Biases and the Role of Education

So far, we have estimated the ideological preferences of MCs by examining the preferences they revealed during other country assignments. Can we also trace them back to differences in the biographies of MCs before they join the IMF? In this context, a natural expectation is that education plays a role (Krcmaric et al. 2020). Most IMF staffers have as economics degree, often from a U.S. university (Chwieroth 2013, Nelson 2014). How economics is taught, however, differs across universities. In the time period in which most MCs in our sample received their education, some universities were strongly influenced by the 'monetarist' and 'neoliberal' paradigms, prominent at the University of Chicago and other so-called 'freshwater' universities close to the North American Great Lakes—an approach to economics that was skeptical of government intervention in market processes dominated research and teaching (Önder and Terviö 2015). Conversely, at many 'saltwater' universities located close to either of the two U.S. coasts, Keynesian and New Keynesian approaches, which see a stronger role for governments in economic processes, were more influential (Henriksen et al. 2022). If the education that IMF economists received is related to their ideological preferences, we would expect to observe systematically different policy outcomes for IMF programs led by MCs with degrees from saltwater and freshwater universities.

To test this we use the data we collected on each MC's *alma mater*. Figure 2.3 plots the most common universities where MCs studied.[19] In classifying universities as freshwater or saltwater schools we follow Önder and Terviö (2015), who study citation networks in economics and find a strong and constant division between these two schools of thought. We then use these data for estimating the following simple model by OLS:[20]

$$IMFconditionality_{i,t} = \alpha_t + \delta_i + \beta saltwater_{i,t}^k + \gamma freshwater_{i,t}^k + \mathbf{X}_{i,t}'\mu + \epsilon_{i,t}. \quad (2.5)$$

The binary variables $saltwater_{i,t}^k$ and $freshwater_{i,t}^k$ indicate whether MC k of country i in year t graduated from a saltwater or freshwater university, respectively. The reference category are MCs who graduated from other universities ($saltwater_{i,t}^k = freshwater_{i,t}^k = 0$).

We find that there are significant differences between MCs with different educational backgrounds. As shown in Table 2.5, MCs who graduated from 'saltwater' universities deviate from the standard approach of IMF conditionality. 'Their' IMF programs include significantly fewer spending limits and pro-market conditions. In contrast, MCs educated at traditionally market-liberal 'freshwater' universities—which promote an approach to economics resembling the one the IMF is notorious for—design IMF programs that are

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[19]Figure 2.F.2 in the Appendix shows a graph that groups universities by country.

[20]Note that we deliberately keep this model simple in order to provide a complement to the arguably more complex jackknife IV approach.

Figure 2.3 – Education of mission chiefs
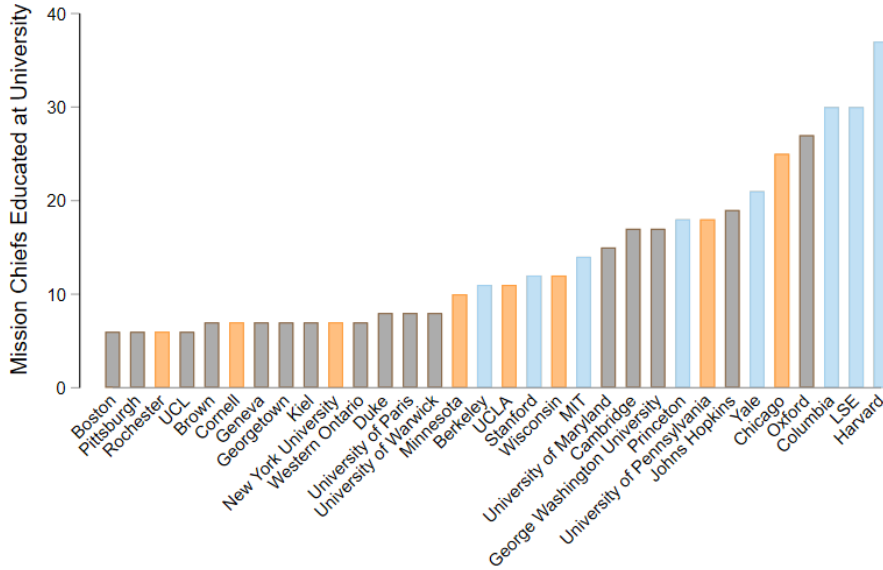
Notes: This figure shows the most frequent alma mater of IMF MCs. Universities classified as belonging to the 'freshwater' school are plotted in orange, 'saltwater' schools are plotted in blue. Coding based on Önder and Terviö (2015).

Table 2.5 – Freshwater schools vs. saltwater schools

	Spending limits		Tax increases		Market-oriented	
	(1)	(2)	(3)	(4)	(5)	(6)
Freshwater universities	-0.065		0.040		-0.006	
	(0.066)		(0.051)		(0.059)	
Saltwater universities	-0.141**		-0.028		-0.107**	
	(0.062)		(0.056)		(0.051)	
Chicago		-0.051		0.092		-0.046
		(0.080)		(0.097)		(0.079)
Harvard		-0.287***		-0.069		-0.165*
		(0.107)		(0.115)		(0.096)
Observations	1121	1121	1121	1121	1121	1121
Macroeconomic controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
R^2	0.554	0.555	0.450	0.450	0.384	0.383

Notes: The table shows the results from eq. 2.5. 'Freshwater universities', 'Saltwater universities', 'Chicago', and 'Harvard' are dummies indicating that the MC graduated from the respective university (type). Standard errors clustered two-way on MC and country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

not systematically different from the average IMF program. The results are similar when considering only the prototypical university of the respective type of school, i.e., freshwater University of Chicago and saltwater Harvard University. These results are in line with the view that the ideological leaning of staff manifests itself in the policy conditions that IMF program countries face. They also show that heterogeneity in the ideological leaning among IMF staff can be traced back to the type of university that individual staffers attended.

2.6.6 Conditional Delegation

While we have presented evidence suggesting that MCs introduce ideological biases into the IMF's policy output, the literature on IOs emphasizes *political* biases introduced by powerful member governments. As anticipated earlier, we argue that these findings can be reconciled. According to the theory of conditional delegation and informal governance, powerful governments only intervene in cases of strategic importance (Stone 2011). The rest of the time, they let the IO bureaucracy decide independently in the context of less salient cases to avoid undermining the legitimacy of the IO. Consequently, we suspect that MCs will be more influential when powerful governments do not intervene in IMF decision-making. We expect that the incentive to intervene is related to economic and political factors. First, loan size matters and animates member-states. While large IMF programs to countries like Greece, Argentina, Ukraine and Turkey are typically accompanied by long and heavily politicized negotiations (e.g., Arpac and Bird 2009), smaller programs to poorer economies are much more removed from the 'high politics' of IMF lending and thus more likely to be left to the discretion of IMF staff. Second, the US, the IMF's most powerful shareholder, has been shown to skew IMF decisions in favor of its closest geopolitical allies (Stone 2008, Dreher et al. 2009, Lang and Presbitero 2018). We thus hypothesize that the bureaucracy's influence is most clearly visible for countries that are not among the U.S. closest allies.

To test these hypotheses, we estimate our baseline specification for separate samples. In Panel A of Table 2.6, we split the sample by loan size and find no statistically significant effect for large IMF programs and stronger effects for small IMF programs. This also holds when only excluding the 25% largest IMF programs, suggesting that the effect disappears only for the largest programs. Panel B proceeds analogously for our measure of alliance with the United States: UNGA voting similarity. Columns 2 and 3 show that the baseline effect is not observable for U.S. allies and stronger for countries that are not closely allied with the United States. Again, the result holds when only excluding the 25% closest U.S. allies, suggesting that the effect disappears only for the countries where the geopolitical interest of the dominant IMF shareholder is largest. In sum, these result support the view that bureaucrats are more influential in cases where political

122

interference by major shareholders is less likely.

Table 2.6 – Jackknife estimation: extent of conditionality and conditional delegation

| | All conditions *(ln)* | | | |
	(1)	(2)	(3)	(4)
MC Bias	0.076***	0.052	0.112**	0.087***
	(0.028)	(0.032)	(0.051)	(0.033)
Observations	1121	550	540	835
Sample	all	50% largest programs	50% smallest programs	75% smallest programs

| | All conditions *(ln)* | | | |
	(1)	(2)	(3)	(4)
MC Bias	0.076***	0.064	0.115***	0.111***
	(0.028)	(0.040)	(0.036)	(0.032)
Observations	1121	536	565	849
Sample	all	US alignment top 50%	US alignment bottom 50%	US alignment bottom 75%

Notes: The table shows the results from eq. 2.4. Panel A subsets the sample with respect to IMF loan size measured by Special Drawing Rights. Panel B subsets the sample with respect to voting behavior in the UNGA. Column 1 shows the baseline results, columns 2-4 subset the sample by UNGA voting similarity with the US. Standard errors clustered two-way on MC and country level: *** p<0.01, ** p<0.05, * p<0.1.

2.7   Conclusion

International organizations are often assumed to be controlled by a unitary and impartial international bureaucracy. Political biases in IO policy output, if present at all, result from interference of powerful governments with technocratic decision-making by homogeneous IO staff. Our results challenge this perspective. Instead, we make three interrelated arguments. First, in organizational activities that have low salience for powerful member-states, the IO bureaucracy has wide leeway to shape its outputs, compared to being encumbered by political intervention in 'high politics' cases. Second, despite supposed tight hierarchical structures and homogeneous worldviews of IO staff, we reveal substantial heterogeneity in how IO staffers perform their duties. Finally, we trace this heterogeneity down to the ideological biases that these staffers have, thus casting doubts on IOs' claims of apolitical, technocratic decision-making.

Our analysis is based on the IMF, one of the world's most powerful institutions diffusing economic policies (Simmons et al. 2008). But how representative is the IMF of the universe of IOs? We do not claim that the processes we have uncovered will identically unfold across diverse organizational forms. Governance structures, member-state interference, staff cultures and the external organizational environment all shape the degree to which individual staff have leeway to act independently and apply their ideological predispositions into organizational practices. This is an area for fruitful future comparative research. But, regardless of how these processes play out, technocratic global governance cannot be solely seen through the assumption of cohesive, ideal-typical bureaucracies,

but instead begs questions on its micro-foundations—this is where we can observe how supposedly dispassionate rule-abiding bureaucrats deviate from their mission, with potentially momentous political-economic consequences.

These findings have important theoretical, methodological, and substantive implications. First, in terms of theory, our analysis contributes to the nascent literature on the micro-foundations of decisions in global governance (Copelovitch and Rickard 2021, Clark and Zucker 2023). That is, rather than treating IO bureaucracies as black boxes, there is promise and payoff in better understanding the role of individual staffers in shaping outputs in global public policy. This is not to approach them as all-powerful actors or rogue players. Rather, under certain conditions—most notably, lax oversight by member-states—opportunity space enables staffers to shape output based on their own ideological preferences. This has clear implications for how we understand IO decision-making and opens up the possibility for studying and specifying the scope conditions for different theoretical approaches.

Second, the methods we import to political science promise to be useful for studying a wide range of questions in the field of international organization and beyond. So far mainly used in economics and criminology in the context of courts and firms (Frandsen et al. 2023), the linkage of bureaucrats, decision-makers and other politically relevant individuals to policy outcomes facilitates important research on both international and national politics. Using these methods, future research could study not only similar mechanisms within other international organizations, but it could also turn to national-level technocratic agencies to examine the conditions under which they produce policy output that is less biased.

Finally, substantively, our analysis points to the limits of accountability within global governance. Political scientists have long vexed over the degree to which international organizations can be held accountable (Hale 2008, Grant and Keohane 2005, Koppell 2010, Park 2022). For the most part, these debates center on *organizational* accountability. This is entirely appropriate for unpacking questions of transparency over operations, ability of member-states to control the organization, and liability when things go wrong. But our research suggests that there is another dimension that is often overlooked. This issue was highlighted by Joseph Stiglitz—one-time World Bank chief economist—who noted that "it is often difficult in large organizations to design incentives that lead to individual accountability, even when organizational accountability exists. […] Much bureaucratic behavior is designed to assure that there exists collective responsibility for failures, eroding individual responsibility" (Stiglitz 2003: p. 122). This can allow staff actions to fly under the radar, or even be swept under the organizational carpet, to safeguard reputation and appearance of neutrality (Wade 2009). Our findings provide clear evidence of individual-level variation in designing organizational policy outputs, thus bringing up to the fore dynamics that usually remain obscure.

Appendices

2.A   Data on IMF Staff

The data collection on MC assignments and the various data sources are described in the main text. Note that when combining data on MC assignment, we privilege official information and information directly obtained from staff reports over the text-mined information that we collected. Furthermore, If a MC name was missing in a country in year t, but identical in year t_{-1} and t_{+1}, we filled the gap by assuming the MC stayed in charge for the entire period.

Figure 2.A.1 − IMF mission chiefs: data coverage and data sources
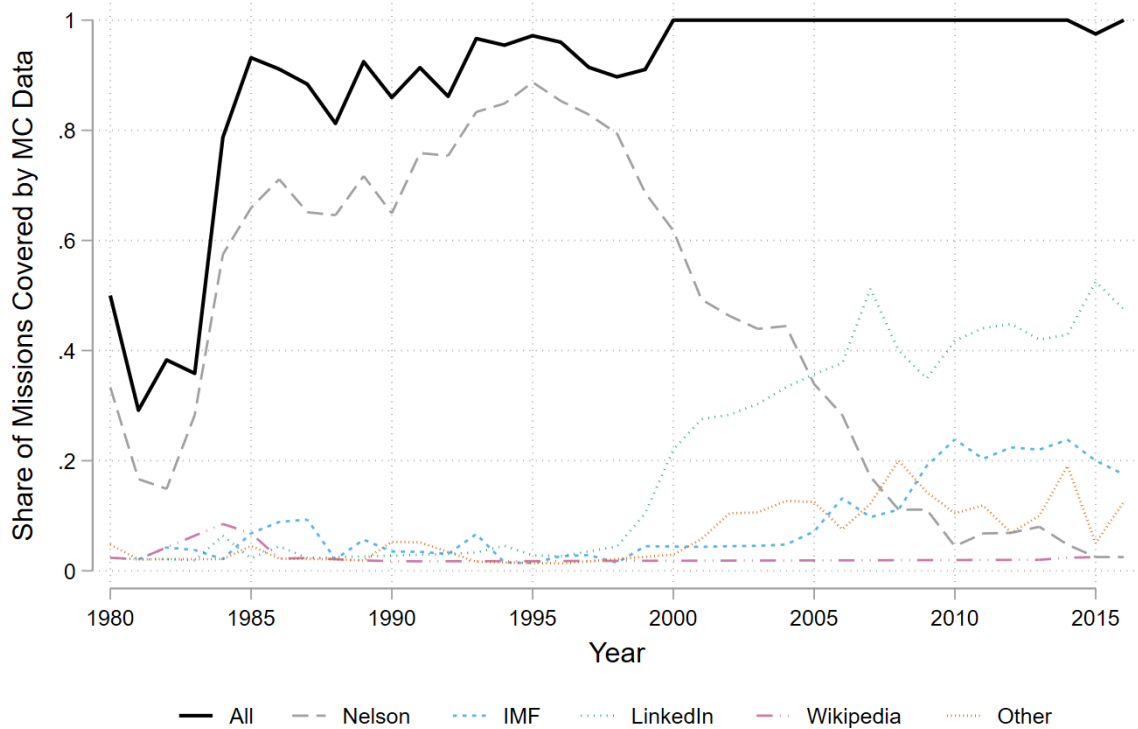
Notes: This figure shows the coverage of the MC variable and the distribution of sources for the MC level characteristics over time. 'All' depicts the share of mission years where we identified the responsible MC. 'Nelson' indicated the share of MCs where individual information derives from information obtained from Nelson (2014). 'IMF' indicates that we found the information in official IMF resources. 'LinkedIn' indicates that information was obtained from the social network LinkedIn. 'Wikipedia' indicates the platform Wikipedia as source. 'Other' includes other, mostly web-based sources.

Table 2.A.1 – Data on mission chiefs

	Count	Mean	SD	Min	Max
Number of missions	835	2.14	3.18	0.00	30.00
Female	785	0.15	0.36	0.00	1.00
Has PhD	639	0.73	0.44	0.00	1.00
US educated	639	0.59	0.49	0.00	1.00
Europe educated	639	0.32	0.47	0.00	1.00
American	561	0.14	0.35	0.00	1.00
European	561	0.44	0.50	0.00	1.00

Note: The table shows summary statistics of data on the individual-level characteristics of IMF mission chiefs.

2.B Data on IMF Conditions

Table 2.B.1 − IMF policy areas

Policy Area	Details
Debt	Debt management, External arrears, Public debt, guarantees, and contingent liabilities
Finance	Financial institutions, Treasury bill issuance and auctions, Government securities, Monetary policy, Central Bank issues
Fiscal	Expenditure policy and administration, Expenditure audits, accounting, and financial controls, Fiscal transparency, Budget-related issues, Domestic government borrowing or debt, Public investment, Public sector arrears, Wage and non-wage arrears, Revenue issues, Audits of private enterprises, Tax policy, legislation and administration, Customs administration, User fees
External	Trade-related issues, Exchange system, Capital account liberalization, Foreign direct investment, Surrender requirements
Privatization	Restructuring of public enterprises, pricing policies and subsidies, Transference of SOE management to the private sector, Regulatory reforms in utilities, price controls and marketing restrictions, Audits of SOEs, Marketing board-related conditions, Corporatization, rationalization, etc., Clearance of arrears to the public sector, other SOEs, or elsewhere, All activities related to the privatization of non-financial SOEs, Liquidation of SOEs, Bankruptcy proceedings of SOEs
Social & labor	Wage and employment limits, Pensions, Social security institutions, Any other measures affecting labor, Changes in any social policy as long they are restrictive or neutral, Increases in prices or removal of consumer subsidies for food, water, public transport, or other basic needs goods, Restructuring of social policy related ministries, Changes in any social policy that contain both restrictive and redistributive measures or are ambiguous

Notes: The table provides details on the policy areas. 'Policy Area' displays the policy areas as defined in this paper. *Fiscal Issues* sums *Fiscal Issues* and *Revenue and Tax Issues*, *Privatization* sums *SOE Privatization* and *SOE Reforms and Pricing* and *Social & Labor* sums *Labour Issues* and *Social Policy*. 'Details' provides details on the specific policy area. For further details, see the codebook from Kentikelenis and Stubbs (2023).

2.C   Coding of IMF Conditions

This appendix describes the method which we applied to gather three new measures for IMF conditionality. For this, we reexamined the descriptions of each individual condition provided by Kentikelenis and Stubbs (2023).[21] The three new variables are called spending limits, tax increase, and pro-market. They all are binary measures coding whether or not the condition fits in the respective category. The three variables are not mutually exclusive. In the recoding effort, we focus on a subset of the 32,261 conditions in the sectors fiscal policy, labor, privatization, revenue and tax policy, state owned enterprises, and social policy, since the exact wording of the conditions available in these sectors allows us to code ideology. These conditions constitute about 50% of the total conditions. In contrast, most conditions in the sectors debt, environment, external sector, finance, institutions, and others, are often limited to a few words which renders the coding of the variables impossible. These three new variables are therefore restricted to a subset of conditions and we make the assumption that variables in the uncoded sectors are 0. We outline the exact coding for each variable below:

- **Spending limits**: Spending limits indicate whether or not conditions conceivably lead to a reduction in public spending in the member country. Examples include:

    - limits on the budget deficit,
    - cuts to the public sector wage bill (i.e., to the number or remuneration of public sector staff),
    - removal or reduction of subsidies (e.g., for fuel or food),
    - any other fiscal space-limiting condition,
    - "spending targets," "targets for the budget balance," "budget targets," "public financial management" (PFM), etc. that should be met,
    - budgets that are approved by / satisfactory to the IMF.

  Any condition not directly related to limiting public spending is coded as 0.[22]

- **Tax increase**: Tax increases capture whether or not conditions mandate tax increases in the member country. Examples include:

    - the introduction or increases of Value-Added Taxes (VAT or sometimes referred to as general sales taxes - GST),
    - the broadening of VAT base,
    - the removal of tax exemptions,
    - any kind of custom taxes,
    - the introduction of other types of new taxes.

  Any condition not directly related to raising taxes is coded as 0. This includes conditions to making administrative changes to tax collection, or increasing efficiency/effectiveness, introducing a new software for customs administration, setting

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[21]Data are available at https://www.imfmonitor.org/conditionality/.

[22]We do no count privatizations related to state-owned-enterprises as spending limits.

up new agencies for tax collection (e.g., a large taxpayers office (LTO)), and general references to "strengthening tax/customs administration").

- **Pro-market**: Pro-market indicates whether or not conditions are explicitly designed to expand the remit of markets in a wide range of policy areas in the member country.[23] Examples include:

  - policies that decrease the role of the public sector in the national economy,

  - privatizations and SOE reforms,

  - labor issues (e.g., liberalization of employment laws, hiring and firing) and broader economic deregulation.

  We count any reduction in subsidies as pro-market.

We ensured inter-coder reliability by double-coding conditions, and subsequently identifying and resolving of any divergent coding. In rare instances of persistent ambiguity as to how a condition should be classified, we coded conservatively (i.e., did not classify the condition as falling into the respective category). Further, we initially attempted to code the intrusiveness of each condition, following earlier work by the IMF's Independent Evaluation Office (Lamdany et al. 2007). However, inter-coder reliability remained too low across all coding attempts—a function of unclear language used in conditions—and we subsequently abandoned this effort.

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[23]We do no count reductions of public sector employment as pro-market.

2.D   Assignment of Mission Chiefs

As discussed in the main text, the causal interpretation of our results requires assuming that the assignment of IMF MCs to a given country is unrelated to their ideological preferences. While our background research suggests that such an endogenous assignment is unlikely, in the following, we test whether there is empirical evidence to suggest that the IMF takes MC-specific ideological preferences into account when assigning MCs to a given country. If the IMF systematically assigned MCs with certain ideological views to countries with certain economic characteristics our results would be biased. This is why we test if the ideological leaning of the incoming MCs can be explained by observable characteristics of the member country.

Hence, we select a set of country-year-specific variables that the existing literature considered as determinants of IMF conditions. This set includes binary indicators for the existence of banking, currency, and debt restructuring and crisis following the definitions from Valencia et al. (2008) and derived from Laeven and Valencia (2020), GDP, GDP growth and current account balance from the World Bank's database, as well as UNGA vote alignment with the U.S. and a dummy indicating UNSC non-permanent membership. We then use lagged values $(t - 1, t - 2, t - 3)$ of this set of observables of country i as explanatory variables in regressions of the *MC bias* of the active mission chief in country i in year t.

The results are reported in Table 2.D.1. As can be seen, none of these variables systematically predicts the bias of the incoming MC. This supports the view that the MCs' preferences are orthogonal to key characteristics of the country to which the MC is assigned.

Table 2.D.1 – Explaining MC bias with program country characteristics

	Lag 1	Lag 2	Lag 3
GDP	-0.222	-0.083	-0.145
	(0.354)	(0.400)	(0.406)
Trade	-0.331*	-0.275	-0.210
	(0.178)	(0.178)	(0.204)
Population	-1.191	-1.112	-1.275
	(0.969)	(0.971)	(1.068)
Banking crises	0.310	-0.102	-0.073
	(0.189)	(0.144)	(0.117)
Currency crisis	0.030	0.113	-0.354***
	(0.148)	(0.122)	(0.133)
Debt crises	-0.393	-0.355*	0.221
	(0.249)	(0.212)	(0.167)
Debt restructure crises	0.251*	-0.091	-0.232
	(0.132)	(0.243)	(0.182)
GDP growth	0.005	-0.007	-0.004
	(0.006)	(0.006)	(0.006)
Capital account balance	-0.009**	-0.009**	0.003
	(0.004)	(0.004)	(0.007)
UNSC nonp. member	-0.163	-0.286***	-0.081
	(0.133)	(0.109)	(0.157)
US UNGA agree	-0.120	0.036	0.119
	(0.198)	(0.196)	(0.168)
Observations	1074	1066	1044
Year FE	✓	✓	✓
Country FE	✓	✓	✓
Region-year FE	✓	✓	✓
R^2	0.463	0.448	0.441
Mean DV	0.034	0.026	0.020

Notes: The table reports results from a regression of the MC bias measure on country level variables. Standard errors clustered two-way on MC and country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.D.2 − Jackknife: Extent of conditionality, learning

	Bias
Experience	-0.004
	(0.006)
Observations	1151
MC FE	✓
R^2	0.694
Mean DV	0.081

Notes: The table reports results from a regression of the MC bias measure for the number of conditions on a measure for MC experience defined as the number of years that passed since the first mission of the MC. Standard errors clustered two-way on MC and country level: *** p<0.01, ** p<0.05, * p<0.1.

2.E   Robustness Tests

Table 2.E.1 − Jackknife: extent of conditionality, alternative minimum

	≥2 Missions		≥3 Missions		≥4 Missions	
	Number	Scope	Number	Scope	Number	Scope
	(1)	(2)	(3)	(4)	(5)	(6)
MC Bias	0.047**	0.220***	0.066**	0.252***	0.066**	0.266***
	(0.023)	(0.082)	(0.026)	(0.086)	(0.028)	(0.089)
Observations	1143	1143	1110	1110	1008	1008
Macroeconomic controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Region-year FE	✓	✓	✓	✓	✓	✓
R^2	0.442	0.584	0.448	0.595	0.444	0.586
Mean DV	3.470	6.150	3.472	6.145	3.485	6.166

Notes: The table shows the results from eq. 2.4. Columns 1–2 show the baseline including MCs with only two missions. Column 3–4 show the baseline. Columns 5–6 show the baseline including MCs with four missions or more. Standard errors clustered two-way on MC and country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.E.2 − Jackknife: extent of conditionality, alternative definition of conditionality

	Absolute		Winsorized (95)		Region×year FE in jackknife	
	Number	Number	Number	Number	Number	Number
	(1)	(2)	(3)	(4)	(5)	(6)
MC Bias	2.289***	2.153***	1.427**	1.389*	0.050	0.057**
	(0.853)	(0.811)	(0.712)	(0.709)	(0.031)	(0.029)
Observations	1121	1110	1121	1110	1121	1110
Macroeconomic controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Region-year FE	.	✓	.	✓	.	✓
R^2	0.458	0.523	0.427	0.496	0.364	0.447
Mean DV	37.531	37.572	36.480	36.511	3.471	3.472

Notes: The table shows the results from eq. 2.4. The dependent variables are indicated in column titles. Columns 1–2 report the results using the absolute number of conditions in eq. 2.2 and eq. 2.4. Columns 3–4 report the results using the 95% winsorized absolute number of conditions in eq. 2.2 and eq. 2.4. Columns 5–6 report the results using region×year fixed effects when estimating the MC bias measure in eq. 2.2. Standard errors clustered two-way on MC and country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.E.3 – Jackknife: extent of conditionality, alternative clustering

	Country		MC		Country year	
	Number	Scope	Number	Scope	Number	Scope
	(1)	(2)	(3)	(4)	(5)	(6)
MC Bias	0.066***	0.252***	0.066**	0.252***	0.066***	0.252***
	(0.024)	(0.082)	(0.025)	(0.093)	(0.022)	(0.086)
Observations	1110	1110	1110	1110	1110	1110
Macroeconomic controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Region-year FE	✓	✓	✓	✓	✓	✓
R^2	0.448	0.595	0.448	0.595	0.448	0.595
Mean DV	3.472	6.145	3.472	6.145	3.472	6.145

Notes: The table shows the results from eq. 2.4. Standard errors are clustered as indicated in column titles: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.F Descriptive Statistics

Table 2.F.1 – Descriptive statistics

	Obs	Sum	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
Conditions: number	1121	37.53	20.63	1.00	153.00
Conditions: scope	1121	6.14	2.23	1.00	13.00
Conditions: debt	1121	9.05	5.90	0.00	32.00
Conditions: finance	1121	6.00	4.54	0.00	29.00
Conditions: governance	1121	1.66	2.85	0.00	29.00
Conditions: external sector	1121	2.51	2.60	0.00	19.00
Conditions: privatization	1121	0.81	1.85	0.00	19.00
Conditions: austerity	1121	0.40	1.09	0.00	11.00
Conditions: spending limit	1121	2.59	3.25	0.00	24.00
Conditions: tax increase	1121	0.78	2.07	0.00	33.00
Conditions: pro-market	1121	0.55	1.44	0.00	14.00
MC Bias: number	1121	0.07	0.92	-5.54	5.26
MC Bias: scope	1121	0.10	0.87	-3.08	3.16
Number of missions	1121	8.37	5.08	3.00	30.00
Population (log)	1121	15.95	1.40	11.17	20.80
Trade (log)	1121	4.15	0.51	2.39	5.95
GDP (log)	1121	7.32	1.07	5.11	10.20
Banking crises	1121	0.04	0.19	0.00	1.00
Currency crisis	1121	0.05	0.22	0.00	1.00
Debt crisis	1121	0.02	0.15	0.00	1.00
Debt restructure crises	1121	0.03	0.18	0.00	1.00

Notes: The table shows the descriptive statistics for the baseline sample from eq. 2.4.

Figure 2.F.1 – IMF conditions by program
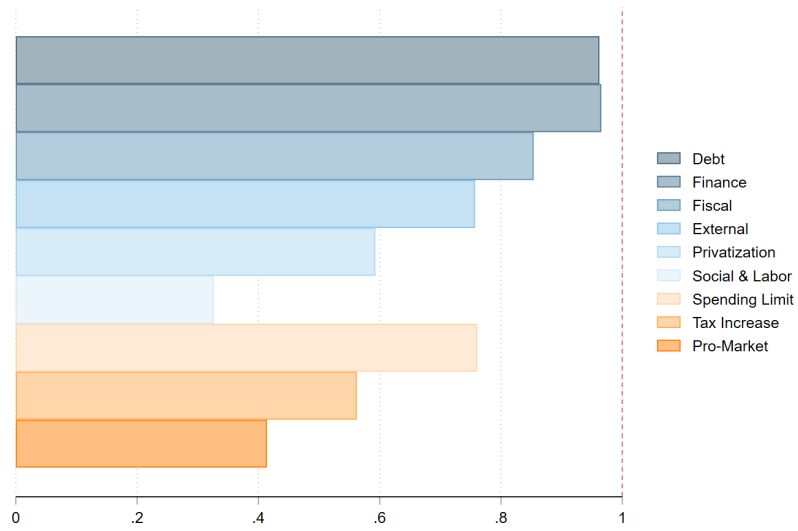
Notes: This figure shows the share of programs with at least one condition of the respective policy area or ideology.

Figure 2.F.2 – Education of mission chiefs: countries
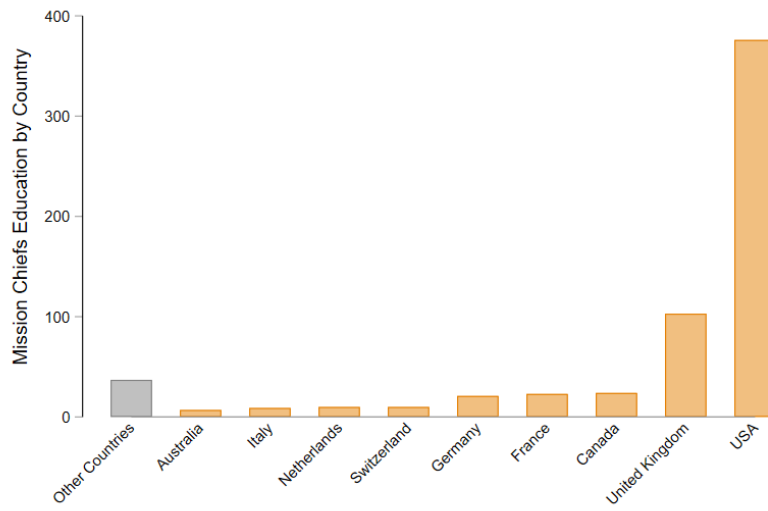
Notes: This figure shows the country of the IMF MCs' alma mater.

This chapter is co-authored with Andreas Fuchs,
Tobias Heidland, & André Gröger.

3

The Effect of Foreign Aid on Migration and Development

3.1   Introduction

In response to surging migration from low- and middle-income countries (Hanson and McIntosh 2016), Europe and the United States promote foreign aid[1] as a means to fight the 'root causes' of irregular migration from the developing world. For example, during the 2015 Valletta Summit on Migration, leaders of the European Union agreed to establish the EU Emergency Trust Fund for Africa (EUTF), endowed with EUR 5 billion, to address "the root causes of instability, forced displacement and irregular migration and to contribute to better migration management across the Sahel and Lake Chad, the Horn of Africa and North Africa."[2] In contrast to this apparent consensus among Western policymakers regarding the effectiveness of foreign aid for bringing development and curbing migration, the scientific evidence remains rather controversial (Qian 2015, Clemens and Postel 2018).

In this paper, we investigate the effects of foreign aid on migration and development

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[1] In this paper, we use the term "foreign aid" in a broad sense, which includes Official Development Assistance (ODA) as defined by the OECD and other types of development finance, such as credit-financed development projects, provided by official agencies.

[2] Similarly, in 2021, the United States' Biden-Harris administration presented their "Strategy for addressing the root causes of migration in Central America." It announced more than US$240 million in new humanitarian and bilateral and regional assistance "to build hope for citizens in the region that the life they desire can be found at home" (National Security Council 2021: p.4).

causally and globally. It systematically documents shifts in migration aspirations, individual welfare outcomes, and realized migration patterns, both in the short and longer term, on a global scale. Our innovative methodology connects the micro and macro levels of analysis. We do so by combining exceptionally rich data from almost one million individuals living in 106 countries from the Gallup World Poll (GWP) with data on subnational aid project allocation by the World Bank, the most important provider of foreign aid over the 2008–2019 period. This dataset of unprecedented detail allows us to exploit variation in foreign aid allocation over time within recipient countries and subnational province-year spells to plausibly identify causal effects. We implement two independent and complementary causal identification strategies that enable us to disentangle the (i) short-term announcement effect of aid project allocation, estimated by means of an event study, from the (ii) longer-term effect of local project implementation, estimated using an instrumental variable approach. We conduct extensive heterogeneity analysis and study these effects across different world regions, country income categories, target sectors of aid allocation, and respondents' individual characteristics.

With respect to the effectiveness of foreign aid for human development, there exists a long-standing theoretical and empirical dispute between optimists and critics that remains unresolved to date. The former argue that foreign aid may provide the necessary "big push" to help low- and middle-income countries overcome poverty traps and stimulate sustained positive development trajectories (Sachs et al. 2004). The latter argue that conventional aid is flawed in different ways that prevent it from achieving any positive human development goals or even leading to negative growth effects (Easterly 2003). Despite empirical evidence supporting positive effects (Clemens et al. 2012, Galiani et al. 2017), there is also evidence of unintended side effects of foreign aid, such as conflict escalation and deteriorating political institutions (Kersting and Kilby 2014, Nunn and Qian 2014, Bluhm et al. 2021), which might have important negative repercussions on human development. The effect of foreign aid on development must therefore be considered theoretically and empirically ambiguous.[3]

Due to the intricate relationship between migration and development, this ambiguity carries over to the effect of foreign aid on migration. One reason for this may be aid targeting, with some studies finding that donors systematically channel aid to source countries of migrants (Czaika and Mayer 2011, Bermeo and Leblang 2015), while others find no clear evidence that aid is distributed differently to major migration origin countries compared to other aid recipients (Clemens and Postel 2018). Moreover, conditional on foreign aid having positive effects on human development, additional income may lead to *more* migration in developing countries, rather than *less* if migration is costly and credit constraints are binding (Clemens 2014, Angelucci 2015, Bazzi 2017). As expected, the

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[3]For meta studies on the aid effectiveness literature, see Doucouliagos and Paldam (2009, 2015).

empirical literature examining the effect of aid on migration finds mixed results.[4] Studies that find negative effects of aid on migration typically find that the effects are small and restricted to certain forms of aid (Gamso and Yuldashev 2018, Lanati and Thiele 2018a,b). Negative effects are more likely if aid successfully improves local amenities (Dustmann and Okatenko 2014) or if aid money buys concessions that shift recipient countries' migration policies (Dreher et al. 2019). In contrast, a second strand of that literature documents a positive relationship between foreign aid and emigration across developing countries (Berthélemy et al. 2009, Belloc 2015, Clemens and Postel 2018). For example, improved infrastructure could increase migration by lowering migration costs (Morten and Oliveira 2018). Part of these diverging results are likely determined by empirical work investigating specific forms of foreign aid, focusing on specific aid recipient countries or regions, and specific time periods.

A good way of operationalizing the causal relationships between foreign aid, development, and migration is by means of the *aspiration-capability* framework (Sen 1999, Carling and Schewel 2018, La Ferrara 2019). The key idea is that individual migration aspirations can only translate into realized emigration if they are met with the respective capabilities.[5] We account for this by investigating the effect of aid on both migration *preferences* and *flows* and document systematically *where* aid translates into changes in migration aspirations and capabilities and *how* it affects migration flows. Moreover, we also carefully exploit the time dimension of this effect. In the short term, the announcement of aid projects may lead to local changes in individuals' aspirations. In the longer term, aid may change both aspirations and capabilities, respectively, through changes in life satisfaction as well as welfare outcomes, such as employment, income, or housing, which could materialize long after project announcement.

In the first part of the empirical analysis, we analyze short-term changes in individual migration preferences after the announcement of World Bank projects. A lack of local life satisfaction can lead to individual migration aspirations and be the initial trigger for migration flows—if met with the respective capabilities. Any change in (expected) life satisfaction—present or future—in the current place of residence might therefore also affect migration aspirations. To test this, we examine aid project *announcements*, which occur on the approval day of the World Bank's Executive Board. At approval, the Board signs project-related contracts, which also marks the start of the project implementation phase. For identification, we exploit the staggered rollout of the GWP during approximately four consecutive weeks per country wave in an event study approach. We match these survey windows with World Bank project announcements and compare individuals interviewed just before to those interviewed just after an announcement of a project in

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[4]See Parsons and Winters (2014) for a review of the literature on migration, trade, and aid.

[5]Capabilities may include financial resources, human capital, social capital, physical capabilities (such as sufficient health), and regular or irregular immigration opportunities at destination. They are thus considerably broader than financial means.

their country. In our most restrictive specification, we exploit individual variation in the same province-wave interviewed in a narrow window of just 15 days before and after a project announcement.

First, we document that the timing of approvals is positively correlated with local media coverage in aid recipient countries that includes the term "World Bank" in the days following a decision. Second, our results provide evidence of a sizeable negative effect of project announcements on migration preferences among the recipient-country population. This effect is more pronounced among young individuals, who are more likely to consider migration, and among those with internet access, arguably reflecting their better access to information. Importantly, our results show that this effect coincides with individuals reporting significantly more positive perceptions regarding the future. Together these results document a novel aid effectiveness channel, with World Bank project announcements reducing migration preferences in the short term by improving individual aspirations about their current place of residence.

In the second part of the empirical analysis, we shed light on the longer term causal effects of aid disbursements on migration preferences, individual attitudes and welfare outcomes, as well as realized migration flows that occur once World Bank projects are implemented. To this end, we exploit geolocated project-level disbursement schedules over time to measure the amount of aid receipts a subnational province receives over time. To tackle potential endogeneity concerns, we follow Kraay (2012 and 2014) and Andersen et al. (2022) and construct an instrumental variable for subnational disbursement flows of World Bank projects. While project design at approval is likely to be subject to *current* and *past* economic (and migration) shocks in the recipient country, they are unlikely to be correlated with *future* (migration) shocks unknown at the time of project approval. Hence, if loan disbursements follow pre-determined disbursement plans defined at project commitment, fluctuations in disbursements on projects approved in previous years will also be uncorrelated with contemporaneous macroeconomic shocks. Since no systematic data on planned disbursement schedules exist, we create *synthetic* disbursement schedules based on similar projects implemented in the same sector and geographic region as the project itself. We then use these synthetic disbursements to instrument for actual disbursements on the province level.

First, and consistent with the short-term announcement effects, we find that larger aid disbursements lead to lower migration preferences in aid recipient provinces in the short to medium term (up to three years after disbursement). Our baseline estimates indicate a decrease in reported preferences by around 0.3 percentage points for an annual aid allocation worth US$ 10 million (or 1.3 percent at the sample average). This effect is again more pronounced for younger and highly skilled individuals, occurs in all world regions with varying magnitudes (except for Asia where we find no effect), and is driven mainly by low-income countries and aid projects targeting the production sector (i.e.,

agriculture, industry, and mining). Importantly, this effect fades away after three years indicating no effect of aid on migration preferences in the longer term.

Second, and leveraging the wealth of individual-level data available from the GWP, we test how World Bank aid project disbursements affect individual attitudes and welfare outcomes in recipient provinces. In the short run, the negative effects of aid disbursements on migration preferences coincide with more positive perceptions about national government institutions. We do not find any effect on individual welfare outcomes such as poverty, income, or employment. We interpret this as evidence of increased aspirations about the current country of residence, while capabilities remain constant. However, in the longer term, between three and five years post-project implantation, we observe that the negative effect on migration preferences fades away and that individual attitudes regarding national institutions and expectations about the economy turn negative. Simultaneously, we find evidence that aid then leads to reduced individual poverty incidence and increases in income per capita. This can be interpreted as a simultaneous decline in local aspirations and increase in capabilities, which could translate into larger migration flows.

Finally, to identify where changes in migration preferences translate into changes in actual flows, we replicate our approach using data on bilateral migration and asylum seeker flows from the same sample of origin countries to the group of OECD countries. First, we find that the negative effects of aid on migration preferences do indeed trigger corresponding changes in regular migration and asylum seeker flows, as follows. In the short term, aid disbursements lead to a decrease in asylum seeker flows, which is persistent up to two years after disbursement and then vanishes. We find no effect on regular migration flows in the short term. In the longer term, however, we observe that disbursements have a positive effect on regular migration flows, which coincides with longer term decreases in local aspirations and increases in capabilities, as explained above. In summary, these results can be interpreted as follows: i) aid affects positively the attitudes and aspirations of people living in recipient areas, and this translates into short-term decreases in asylum seeker flows. ii) aid is effective in improving individual welfare in the longer term, which increases migration capabilities and, while local aspirations deteriorate, this translates into increases in regular migration flows, consistent with the "mobility transition" theory.

We proceed as follows. Section 3.2 provides a conceptual framework of how foreign aid can affect migration in the short and long term. In Section 3.3, we introduce the different data sources used and provide descriptive statistics. Section 3.4 presents the empirical approach and results for the event study design examining the short-term effects of aid project announcements. In Section 3.5, we present the empirical approach and results for the instrumental variable strategy investigating the local longer-term effects of aid disbursement and project implementation. We conclude in Section 3.6.

3.2 Conceptual Framework

Policymakers' promises to increase foreign aid to developing countries are often accompanied by claims that helping these countries develop will discourage their populations to emigrate. At first glance, this seems intuitive: If aid is effective in creating more jobs, better education, access to health care, and higher quality public institutions at home, *all else equal*, prospective migrants should be less inclined to leave their country.

Empirical studies' findings and economic theory offers diverging predictions regarding the development-migration nexus. Some empirical studies find that poor households are more likely to migrate as they face lower opportunity costs of migration (Jayachandran 2006, Mueller et al. 2014, Mastrorillo et al. 2016). However, a short-term increase in income might also enable migration of individuals that were formerly facing financial constraints (Cattaneo and Peri 2016, Bazzi 2017, Clemens and Postel 2018, Cai 2020). Even a small amount of additional income can thus trigger migration if individuals are severely credit constrained and are too close to subsistence to take any income risks, even if they can potentially reap large returns (Bryan et al. 2014).

Regarding the effect of longer-term income growth on emigration, which is highly correlated with other improvements in development outcomes, there is not yet a scientific consensus (Clemens 2014, Bencek and Schneiderheinze 2020, Czaika et al. 2021, Langella and Manning 2021). This is partly because the role of expectations and information in the migration decision-making process is still relatively poorly understood.[6] Information about the destination country can have ambiguous effects on migration. On the one hand, more information about a destination country might make this country appear more attractive as an ultimate destination and enable migrants to rely on existing networks (Dreher et al. 2019, Lanati and Thiele 2021). On the other hand, more information might create more realistic expectations about living conditions, including labor market access, and thus correct overestimated gains from migration downward (Bertoli et al. 2020, Tjaden and Dunsch 2021). This has important implications for the transmission mechanism of aid on migration decision-making. Furthermore, the most common migration models in economics, based on the neoclassical migration model, are highly simplistic when it comes to the motivation to migrate, thus overpredicting interest in emigration (see, e.g., Clemens 2022).

A useful model for understanding the relevant aspects of individuals' migration decision-making is the *aspirations-capabilities framework*, which splits up the migration decision into preferences and constraints.[7] The common approaches to model aspirations in eco-

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[6]Notable exceptions are Shrestha (2020) and Baseler (2023) who use an experimental approach to provide prospective individuals or their families with information about the risks and benefits of migration to assess the information channel.

[7]The framework was originally developed by qualitative social scientists (Carling 2002, Carling and Schewel 2018, De Haas 2021).

nomics is to assume that individuals draw utility from two separable sources: income and income relative to aspirations (Dalton et al. 2016). In the context of migration, it makes sense to widen the focus to broader living conditions (see Detlefsen et al. 2022). If individuals feel they cannot achieve their aspirations locally, they will start considering migration (De Haas 2021), which can be best measured through migration aspirations. Aid can thus decrease migration aspirations if it improves local conditions (or the perception thereof) and thus makes it more likely that individuals think they can achieve their life aspirations without migrating.

Aspirations are different from expectations (La Ferrara 2019). While rational expectations and revealed choices account for constraints, aspirations can be seen as a measure of preferences that are not impacted by real-world constraints.[8] A preference measure can be treated as separate and as unaffected by capabilities if the agent follows what Dalton et al. (2016) and La Ferrara (2019) call a behavioral solution, i.e., when aspirations are not updated based on constraints. The survey question we use to elicit preferences is hence formulated in a way that avoids the explicit incorporation of constraints (see Section 3.3).

To reflect the distinction between aspirations and capabilities, we conceptually separate our paper into two parts. In the first part of our paper, which analyzes the short-term effect of local aid announcements, we will analyze migration aspirations, which we interchangeably call migration preferences. In the second part, we will extend our analysis to longer-term effects and consider actual migration flows as well, thus also incorporating capabilities. The announcement of aid might have an immediate effect on the preference to stay, for example, by causing individuals to update their expectations of the future. By contrast, we expect that any effect of aid on actual living conditions and thus on capabilities is likely to take longer. As a consequence, migration preferences might react immediately upon announcement. Any effect caused by lifting constraints that could then affect realized migration via capabilities can be expected to take longer. If one were to focus solely on migration as the revealed outcome that distinction would not be possible and hence the resulting understanding of migration-decision making and aid's effects would be rather limited.

Aid projects can differ in their effects on migration aspirations and, in turn, migration flows, depending on the types of projects and the time horizon studied. Possible mechanisms are improved economic opportunities or living conditions. The latter include security, social protection, and, importantly, also improved health or education services, which are part of what has been called public amenities in the literature (see, e.g., Dustmann and Okatenko 2014). While perception can change rapidly when a new project is announced, the real effects, such as improved employment opportunities, may

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[8]In her paper, La Ferrara further distinguished aspirations from goals ("an objective, measurable target"), which she suggests are a lower-order variable than aspirations.

take longer.

In the short and medium term, aid projects can be expected to decrease migration aspirations by improving economic opportunities or living conditions and their perception. In the longer term, better education thanks to aid projects may increase individuals' or their parents' aspirations. This paper focuses on the short- and medium-term effects of aid on migration, so we expect this channel to be less relevant. Of course, projects might be ineffective at changing any of these relevant outcomes, either because they are not aimed at those margins in the first place or because they fail to deliver an impact. Whether the effects via income or amenities are more effective in the short run is an empirical question that depends on several factors, including the beneficiaries' utility function, the relative effectiveness of projects, and context.

In education-related approaches (La Ferrara 2019), low levels of aspirations, for example, due to a lack of positive role models, cause suboptimal effort. Policy recommendations have therefore rather focused on raising aspirations. This is different in the case of migration, where high emigration aspirations are seen as a problem by many destination-country policymakers. To protect domestic workers from outside competition, immigration to high-income countries is highly restricted. This results in a lack of capabilities to migrate legally for a majority of the population in poorer countries, even if they are able to pay for the direct costs of migration. As a consequence, people migrate irregularly and often take extremely high risks (Friebel et al. 2018, Bah et al. 2023). Lacking visa, these individuals often show up in the statistics as asylum seekers and make up a large part of those refused asylum. Reducing the asylum seeker flows, and the latter group in particular, is a key objective of the "root causes" strategy. We will therefore study not only regular migration, but also asylum seeker flows.

3.3 Data

3.3.1 Measuring Migration

We analyze both international migration preferences in developing countries and actual bilateral migration flows between developing countries and OECD countries. Separating migration preferences from actual flows allows us to study the effect of policies on individual aspirations to move abroad, independently of the major constraints that keep most people in low-income countries from migrating internationally. We then incorporate these constraints when we analyze realized migration flows.

To study international migration preferences, we use data from the Gallup World Poll (Gallup 2018). The GWP is an annually repeated cross-sectional survey, covering almost all countries worldwide, that provides data for more than 2.3 million individuals. For low- and middle-income countries, the dataset contains 1.5 million observations in total,

i.e., an average of 115,000 individuals per year. The sampling is probability-based and representative of the national resident population of 15 years and older—with only few exceptions due to staff safety and scarcely populated or poorly accessible areas. Questions are standardized around the world for all respondents and asked in the respective national language. GWP interviewers conduct surveys by telephone where telephone coverage exceeds 80 percent using random digit dialing or nationally representative telephone number lists. In all other regions, they conduct face-to-face interviews based on random routes procedures at different times of the day. A typical survey collects data from 1,000 individuals, varying with country population size.[9] GWP data are becoming widely used in economic research (e.g., Deaton 2008, 2018, Bjørnskov 2010, Kahneman and Deaton 2010, Stevenson and Wolfers 2013, Böhme et al. 2020, Guriev et al. 2022), including research on aid (Wellner et al. 2023) and migration (Dustmann and Okatenko 2014, Bertoli and Ruyssen 2018, Dao et al. 2018, Böhme et al. 2020, Aksoy and Poutvaara 2021).

GWP comes with great precision in time and space. It contains information on the specific date when the survey was carried out.[10] It also contains within-country geographic information that allows us to match individuals to first subnational administrative (ADM1) regions from the Database of Global Administrative Area (GADM).[11] ADM1 regions correspond to provinces, states, oblasts, among others, in the respective country's institutional setup, but we call them "provinces" in the following for simplicity. We successfully mapped provinces from 126 out of 140 non-high income countries covered in the GWP (listed in Appendix Table 3.A.1). From these countries, we successfully mapped 2,025 out of a total of 2,280 provinces.[12]

What we aspire to is often shaped, at least implicitly, by the constraints we face. To measure (stated) migration preferences cleanly in a way unaffected by constraints such as a lack of access to visas or finances, the survey question needs to be designed accordingly. In this article, we measure international migration preferences based on the following GWP question: "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" We code the answer as a binary indicator that takes a value of one if the individual responds with "Yes" and zero otherwise (i.e., "No," "NA," and "Don't Know"). This question inquires about

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an individual's preferences, providing a hypothetical scenario with the two clear options to move or to stay. The question has the advantage of clearly addressing the desirability of migration, irrespective of whether respondents view it as achievable or actively seek it (Carling 2019). A caveat is that the question clearly refers to *permanent* international migration. While the answer for those who indicate migration preference is clear, it is unclear whether denying the question indicates the absence of migration preferences or the presence of migration preferences *with the intention to return.* As we thus measure a migration preference for leaving the country permanently, we obtain a lower bound since the variable ignores short-term migration aspirations with the intention to return.

Our sample covers the period from 2008 to 2019 as the variable is not available in earlier and more recent Gallup survey waves. About a quarter of the respondents in our sample (23.2%) express migration preferences. As the world map in Figure 3.1 shows, we observe substantial variation across space. Migration preferences are highest in Latin America (31.1%), followed by Sub-Saharan Africa (30.3%) and lowest in Asia (12.8%). At the country level, Sierra Leone (61.5%), Albania (58.8%), and Liberia (58.8%) rank highest, whereas Indonesia (2.3%), Thailand (3.3%), and Uzbekistan (3.9%) show the lowest share of people aspiring to migrate. We also observe significant variation over time (Appendix Figure 3.A.1). From 2008 to 2019, the share of respondents that are willing to migrate increased from 17.9% percent to 30.9%.

Figure 3.1 – Average migration preferences by province, 2008–2019
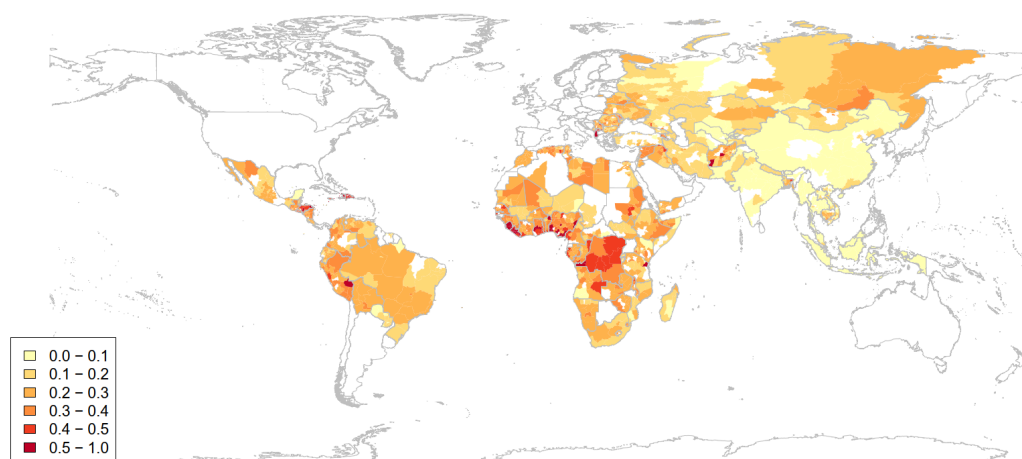
Notes: This figure shows the average share of interviewed individuals that indicate a preference to migrate by province (ADM1 region) between 2008 and 2019.

Table 3.A.3 provides the descriptive statistics for our final sample. 53.4% of the respondents are female. The average respondent's age is 38.5 years, and the average education level between 8 and 15 years. 37.4% live in an urban area, and 62.9% reports to have a child.

Turning to actual migration flows, we rely on the OECD International Migration Database, which covers bilateral flow data from 158 countries of origin into 25 OECD countries from 2000 onward (OECD 2020).[13] Data include both economic migration and asylum seeker applications. The flow data derive typically from national population registers or provided by national immigration offices.

Migration preferences correlate strongly with migration flows (Tjaden et al. 2019). Docquier et al. (2014) estimate the correlation between the average share of people who prefer to migrate internationally and the actual emigration rate in 138 countries of origin. In a linear regression, they estimate the coefficients for the less educated as 0.215 and for the college-educated as 0.942. This indicates that, among those individuals with access to finances and visa, there is almost a 1:1 elasticity between migration preferences and actual emigration ($R^2 = 0.97$), although many of those who report they want to migrate do not migrate in a given year due to constraints or changes in circumstances. Among the more constrained people with less education, a large share of the potential migrants becomes involuntarily immobile.

Migration preferences are also highly indicative of actual migration flows in the dataset we use (Appendix Table 3.A.2). A higher number of migrants flows into OECD countries in country-year pairs with higher migration preferences.[14] This correlation becomes stronger when restricting the sample to individuals who express migration preferences to one of the OECD countries and when we only consider individuals who also express concrete migration preparations. At the dyadic level (again using migration preferences for specific OECD locations), we also observe a positive correlation between migration preferences into specific OECD countries and actual migration flows into the same OECD countries.

3.3.2 World Bank Project Data

The World Bank is the World's largest international financial institution and has the mission to reduce global poverty and promote sustainable development. It provides financial and technical assistance to developing countries for development projects and programs that range from education and healthcare to agriculture and mining. The World Bank consists of two main branches: the International Development Association (IDA) and the International Bank for Reconstruction and Development (IBRD).[15] The IDA provides concessional loans and grants to the world's poorest countries, enabling them to undertake projects that may not be feasible through regular borrowing. The IBRD pro-

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[13]Since reliable worldwide annual bilateral migration data do not exist, we focus on OECD countries exclusively.

[14]For this, we aggregate eq. 3.2 to the country-year level and create weighted means of all variables.

[15]We specifically focus on the World Bank and not the World Bank Group, which also includes the International Finance Corporation (IFC), the Multilateral Investment Guarantee Agency (MIGA), and the International Centre for Settlement of Investment Disputes (ICSID).

vides loans and financial resources to middle-income and more creditworthy low-income countries. Decision-making at the World Bank is guided by its Board of Governors and the Board of Directors. The Board of Governors, composed of representatives from each member country, is the highest decision-making body and typically meets once a year to discuss strategic issues. We focus on the Board of Directors, which meets regularly and is responsible for day-to-day operations, including the approval of World Bank projects. It consists of 25 Executive Directors who represent the member countries or groups of countries and make decisions on projects, policies, and strategies. The President of the World Bank serves as the head of the institution and oversees its overall operations.

We use project-level data on World Bank grants and loans from two data sources, Aid-Data (2017) and the World Bank's Application Programming Interface (API).[16] Combined, these two data sets contain information on 6,728 projects, its implementing agency, project sector, and precise project location which we geocode to the ADM1 level.[17]

In addition to the project location data, we use an updated version of the World Bank project-level disbursement data from Kersting and Kilby (2016). The data contain information on monthly commitments, disbursements, repayments, fees, and interest payments of the World Bank. From the previous dataset, we possess disbursement schedules for 4,495 projects, totaling a commitment value of US$ 565 billion and disbursements amounting to US$ 364 billion. The availability of disbursement schedules allows us to track the development of single projects over time.[18] Figure 3.2 shows the total amount disbursed through these World Bank projects by province for all countries in our sample over the 2008–2019 period. The largest recipients of World Bank funds are Brazil, China, and India, with substantial amounts also disbursed to Argentina, Bangladesh, Pakistan, and Vietnam. Appendix Figure 3.A.2 shows the disbursed amounts by country and sector over the sample period.

3.4   Announcement Effects

3.4.1   Empirical Approach

To examine the short-term effects of World Bank project announcements on international migration preferences, we exploit information on the dates of project approvals by

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[16]We downloaded these data from the World Bank project level website `https://datacatalog.worldbank.org/search/dataset/0037800` on December 1st, 2020. Note that these data including the geocoordinates are no longer available on the website.

[17]We combine the two datasets by appending the additional information in the API dataset on the AidData dataset. In the resulting dataset, 3,357 projects (with 40,979 project locations) originate from the API; 3,371 projects (with 56,037 project locations) come from AidData. We only keep projects with a board approval date. Appendix Table 3.A.4 details how the World Bank categorizes project sectors.

[18]To allocate amounts across project locations, we equally split the disbursed amounts over the respective number of project locations by project provinces (e.g., Dreher and Lohmann 2015).

Figure 3.2 – World Bank disbursements by province, 2008–2019
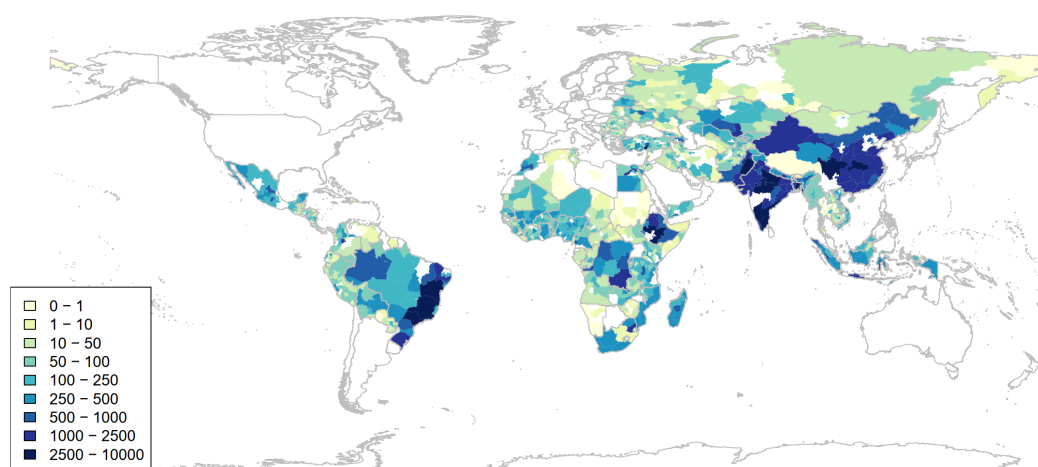
Notes: This figure shows total World Bank aid disbursements in million US$ by province (ADM1 region) between 2008 and 2019.

the World Bank's Board of Directors. Board approvals mark a central milestone in the progress of a World Bank project.[19] At this stage, the potential project has been identified, the required project documents (including financial plans, technical assessments, and environmental and social reports) have been drafted and the legal documents are being signed. In other words, the preparation phase ends and the implementation phase begins.[20]

We expect that the announcement of projects at the time of its approval affects individual migration decisions in aid recipient countries. For this to happen individuals in aid recipient countries need to (1) receive information about approved projects in due course and (2) change their migration calculus already at a time when the project's implementation phase is only about to begin and its completion might be years away.

Concerning (1), it is important to emphasize that board approvals are not only a standard administrative procedure, they are also accompanied by a substantial media coverage in the borrower country and elsewhere. Take the "Productive Partnerships in Agriculture Project" in Papua New Guinea as an example. The Board of Directors approved the project, which provides additional finance with the intention to improve yields, product quality, and ultimately the livelihoods of smallholder cocoa and coffee producers in the country, on Friday, February 28th, 2014. On the approval day, the World Bank issued a press release presenting the project and its benefits, followed by a blog post with footage from local farmers on March 1st, 2014. Already on the day

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[19] At first sight, it might seem that World Bank project closure dates would be an alternative relevant date for our analysis. However, closing dates have a rather administrative character indicating a date six months after project completion. It is thus unlikely that they can affect migration preferences.

[20] Figure 3.A.3 in the Appendix provides a detailed overview of the project life cycle.

following the approval decision, the news spread through the recipient country's national media, for example via the website of Papua New Guinea's commercial television channel EM TV.

We checked whether project coverage in recipient country news outlets in the days following the board approval is indeed common with two approaches. Using information collected by the Global Database of Events, Language, and Tone (GDELT) and Google news searches, we checked for a subset of projects whether we can find systematic media reporting. In a sample of 20 randomly selected World Bank projects, we were able to trace project-related coverage in the national media in the immediate days after the project event for five projects. Considering that the availability of English-language online newspapers limits this research, this is likely a lower bound. We thus decided to proceed with a more systematic analysis where we test whether board approvals lead to significantly higher World Bank coverage in recipient country media outlets in the day immediately following the board approval.

In the more rigorous approach to test whether news of the project approval travels into recipient countries, we use daily country-specific information on news reporting on the World Bank from the Global Flows of Political Information (GLOWIN) dataset (Parizek 2023), which is based on GDELT and covers 215 countries from 2018–2020.[21] We use these data to test whether World Bank board approval triggers media coverage on the World Bank in recipient countries. More precisely, we regress a binary variable indicating whether or not the World Bank was mentioned in the recipient countries' media on a binary variable measuring whether or not the World Bank approved a project in the same country on the previous day to allow the news to be picked up by the media. Appendix Table 3.B.1 reports the results gradually increasing the level of fixed effects. Our results show a significant increase in World Bank media coverage in recipient country media in the day after World Bank board approvals. There is a 11.7% likelihood that the World Bank is mentioned in any of the analyzed media outlets in the data, and this likelihood is increased by about 21% in the day immediately after a board approval. We thus conclude that World Bank board approvals are indeed meaningful dates that receive attention from the public in recipient countries.

Turning to (2), there are reasons to believe that the announcement can immediately affect individual migration decisions in the recipient country, i.e., at the start of the project's implementation phase when completion might still take several years. As discussed by Czaika et al. (2021), migration preferences are shaped by aspirational gaps between *real* life circumstances and and *desired* life circumstances. If the desired life circumstances cannot be envisioned in the real circumstances, the individual desires change.

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[21]For each day in the sample, GLOWIN uses a random subset of all GDELT entries, conditional on being in the top 500 views of at least one country in the world, totalling to around 10,000–15,000 news articles per day. It then uses hand-coding and supervised machine learning to identify reporting on international organizations.

One way how this change can be implemented is through migration. Individuals who intend to migrate hope to decrease the gap between their current life circumstances and their aspirations by finding a better life in a different location. Hence, individual migration preferences are a trade-off between the expected gains from moving relative to the counterfactual scenario of staying. Any change in current standards of living—or expected changes in future standard of living—can alter this equilibrium. While the announcement of a single World Bank project is unlikely to bear immediate effects on populations in recipient countries, it can raise expectations that life will improve in the future. This might change the underlying trade-off regarding the migration decision. When World Bank projects thus alter future perceptions, the announcement of a project might also have an immediate effect on migration preferences in recipient countries.

To estimate the short-term effects of aid announcements, we exploit the staggered roll-out of the GWP, which conducts specific country-wave interviews within a time frame of on average about four weeks. We match these survey windows with project-specific announcement (i.e., Board approval) dates implemented in the same country. This allows us to compare individuals interviewed before such an announcement date to individuals interviewed after the announcement date. Gallup World Poll interviews happen throughout the year and different countries are interviewed in different months of the same year.[22] Figure 3.A.4 shows the distribution of GWP interview dates by country of the exemplary year 2015, where interview dates are marked in light gray and interview dates after the announcement of an aid project in the country are marked blue. We only exploit variation between individuals of the same survey wave. The identifying assumption is that the timing of the World Bank announcements relative to the implementation of the GWP survey days is random, which is plausibly the case. There is no obvious reason why the interview dates of a U.S.-based private analytics company would systematically be related to the timing of decisions made by the World Bank's Board of Directors. Nevertheless, we take several measures to ensure that our control group (individuals interviewed just before an approval date) is comparable to our treatment group (individuals interviewed just after an approval date).

First, we only compare respondents interviewed across similar space and time. In our strictest specification province-year-month fixed effects, we exploit variation across individuals of the same province and survey wave.

Second, while the allocation of individuals before and after the event is random, sampling methods could result in an uneven distribution of individuals around the announcement date. One obvious concern is that individuals interviewed in later stages of a country-year spell are systematically different from those interviewed in early stages. We thus conduct balancing tests with individual-level characteristics (gender, age, education

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[22]The GWP is a quasi panel, which makes the usual difference-in-differences setup impossible as not all individuals are interviewed within the same time period.

level, binary variables for urban residence and parenthood) around the announcement dates and find that, overall, individuals in the treatment and control group are similar (Table 3.B.2). Individual characteristics are largely balanced across individuals interviewed before and after World Bank project announcements. The exception is the urban status of the respondent: on average, individuals interviewed after an announcement are more likely to live in rural areas. We thus control for urban residence (and the other listed individual-level characteristics) in all specifications below.

Third, we control for day-of-the-week-level confounders through fixed effects. Gallup interview and World Bank approval dates occur on all weekdays, however, with varying probability. Bank announcements are more likely on Tuesdays and Thursdays, while GWP interviews are more frequent on Saturdays.[23]

Fourth, we restrict the sample to individuals interviewed within 15 days before and after the World Bank approval date.[24] While this comes at the disadvantage of discarding a large share of the data, it further increases the comparability of our treatment and control group. Within the already strict provinces-year-month fixed effects, we now only compare individuals interviewed about two weeks before and after the announcement within the same month and province.

Based on these measures, it is arguably random if an individual is interviewed before or after project announcement, which allows us to interpret our results in a causal manner. Specifically, we estimate the following equation:

$$outcome_{ipcdy} = \beta post_{icdy} + \gamma X_{ipcdy} + \delta S_d + \zeta_{pcy} + \epsilon_{icpdy}, \tag{3.1}$$

where $outcome_{icdy}$ is our dependent variable being migration preferences or attitudes for individual i living in province p of country c interviewed on day d in year y. $post_{icdy}$ indicates whether or not an individual has been interviewed after a project event in the respondent's country.[25] X_{ipcdy} are individual-level control variables, which include a binary variable if the respondent is female, the respondent's age in years and its square, an education indicator, an urban area indicator, and whether or not the household has at least one child.[26] S_d represents a binary variable for the day of the week. ζ_{pcy} denotes province×year-month-fixed effects. Finally, ϵ_{ipcdy} is the error term. We cluster standard

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[23]See Figure 3.A.5 for the distribution of Gallup interview dates and Figure 3.A.6 for the distribution of World Bank board approval dates over time.

[24]We chose 15 days in our preferred specification as it roughly corresponds to half of the average survey length.

[25]17.5% of the individuals in our final sample are treated, as we report in Table 3.A.3.

[26]The education variable takes a value of one if the respondent has 1–8 years of schooling, a value of two for 8–15 years, and a value of three for 15 years or more. The urban area indicator defines if the respondent lives in a rural area (defined as rural area, village, or small town) or urban area (suburb of a large city or large city.) 53% of the respondents are female. The average respondent has an age of 39 years and an education level of 8–15 years. 37% live in an urban neighborhood, and 63% report to have at least one child.

errors at the country level.[27]

3.4.2   Results

Figure 3.3 presents the main results in an event study plot. It shows coefficients and 90 percent confidence intervals for interactions between the binary variable *Post* indicating whether the individual was interviewed after World Bank project announcement or not and 22 dummies for three-day periods included between 30 days (4 weeks) before and after the approval date. It is reassuring that we do not observe significant pre-trends. We find a significant decrease in migration preferences in the days after World Bank project approvals. Individuals are 1.9 percentage points less likely in the three days after project approval to express migration preferences than in the three days before project approval. This effect is sizable given the sample mean of 23.2%. The estimated effect remains negative with varying precision of the estimates throughout the entire month after project announcement.[28]

In Table 3.1, we estimate eq. 3.1 with a step-wise increasing number of controls. As indicated in the table, we start with only year fixed effects in column 1 and end with province-year-month fixed effects in column 4. Finally, in our preferred specification in column 5, we use the reduced sample with 15-day event windows around project announcements. Throughout, results show a consistent and statistically significant negative effect of World Bank announcements on migration preferences. Individuals interviewed after a project announcement are 0.8 percentage points less likely to express migration preferences. In light of the sample mean of 20.3%, this effect is sizeable. These results indicate a substantial and immediate effect of aid project announcements on preferences for international migration.

To understand whether this effect occurs locally or across the country, we analyze province-level effects in Table 3.B.9. For this, we interact the treatment variable with a binary variable taking the value one if the project is approved for a specific province. "Post×province" thus indicates the additional effect of a project announcement in the project province compared to the overall country effect. We do not find a significantly different impact of project announcements on migration preferences in the project province compared to other provinces. The announcement effect reducing respondents' preferences for international migration is thus not specific to the province where the project is implemented.[29]

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[27]As we show below, our results are robust against a variety of different levels of clustering standard errors.

[28]Note that the number of observations decreases mechanically when moving further away from the event, as the average survey wave length is 26 days, which explains the increase in the size of the standard errors.

[29]We further test whether the effect differs for the type of project announced. As we show in Table 3.B.10, the effect does not differ across IDA or IBRD projects, nor does it increase with the committed

Figure 3.3 – Migration preference and World Bank announcements, event study plot
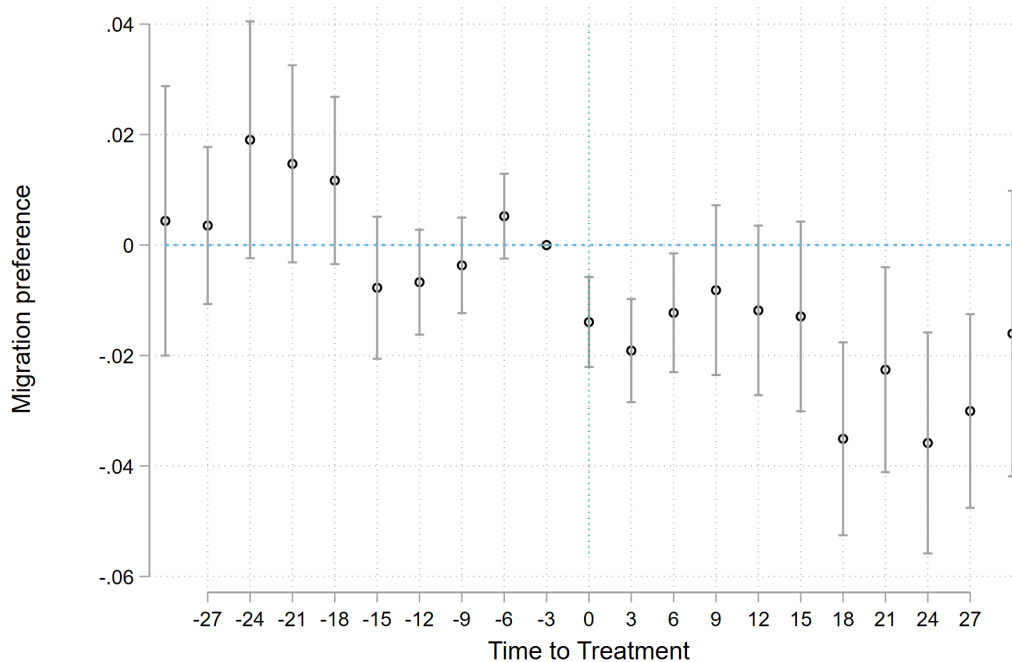
Notes: This figure plots the coefficients and 90 percent confidence intervals for a regression based on eq. 3.1 where we augment the specification with interactions between the binary variable indicating whether the individual was interviewed after World Bank project approval or not and 22 dummies for 3-day periods included between 30 days (4 weeks) before and after the approval date. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Standard errors are clustered at the country level.

We test the robustness of the results in several ways. First, we run a randomization inference test based on 999 Monte Carlo replications to show that our effect is not driven by any underlying features of our data structure that our control variables and fixed effects might not account for. We do this in a two-step-procedure. In the first step, we draw at random a number of country-year waves from the entire population of country-year waves of the GWP data. This simulates the random coincidence of GWP interviews and World Bank board approvals. We draw 265 country-year waves as this corresponds to the total number of treated waves in the baseline.[30] In the second step, we draw one random date as placebo treatment date for each of these 265 survey waves and divide the sample into treatment (post event) and control (pre event) in parallel to our main specification to run eq. 3.1 with these random events. Based on these random events, we conduct a randomization inference test based on 999 Monte Carlo replications. Figure 3.B.1 in the Appendix displays the distribution of the 999 coefficients, which center around zero. The

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financial size of the project.

[30]Placebo survey waves can thus include both country-year waves that are treated and non-treated in the baseline.

	(1)	(2)	(3)	(4)	(5)
	Migration preference				
Post	-0.05880***	-0.05601***	-0.00867*	-0.00939**	-0.00811**
	(0.01834)	(0.01543)	(0.00488)	(0.00365)	(0.00382)
Observations	952713	952713	952713	952336	172112
Individual controls	.	✓	✓	✓	✓
Country-year FE	.	.	✓	✓	✓
Day-of-the-week FE	.	.	✓	✓	✓
Year-month FE	.	.	✓	✓	✓
Province-year-month FE	.	.	.	✓	✓
15-day-window	.	.	.	.	✓
Mean DV	0.232	0.232	0.232	0.232	0.203

Notes: This table presents the results from eq. 3.1. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Post" is a dummy if the respondent was interviewed after the board approval of a World Bank project that will be implemented in the respondent's country. Individual level controls include gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

baseline estimate from column 6 of Table 3.1 is shown by the dashed vertical line. The p-value of 3.6 percent is calculated as the proportion of times that the absolute value of the t-statistic in the simulated data exceeds the absolute value of the original t-statistic. The results of this randomization test show that it is very unlikely that any omitted variables correlate with our World Bank project announcement variable in a way that would spuriously produce our main results.

Second, we show that our results are robust when altering the number of days we include in the window around the event. Our conclusions do not hinge on our choice of the length of the window as regression results for alternative event windows of 5, 10, 20, and 30 days show (Table 3.B.3). It is particularly reassuring that our results remain robust even when reducing the event window to 5 days around the event. We judge it as very unlikely that once we control for our set of control variables and fixed effects, other events could drive our results in a systematic manner or that our results are driven by sample composition mechanics within such a short time frame of only 10 days. What is more, our results remain robust when leaving out the event date (and the day before and/or after the event date) from our analysis (Table 3.B.4). Our results even become stronger, which is not surprising given that we cannot rule out that the news of a new World Bank project needs time to spread in the recipient country population.

Third, we check that our results are not driven by our decision to use individual-level data. We prefer to run our regressions on the individual level, as this allows us to fully exploit both individual-level information for our treatment and control variables, the individual-level heterogeneities, as well as the temporal variation of the data with

precise interview dates for our fixed effects and pre-trend analysis. One concern with this approach might be that the large number of observations artificially inflates the standard errors in our estimations. To address this concern, we show that our results are robust when aggregating the individual-level data to the country level, creating country-year averages for the period before and after the project announcement.[31] Our results are similar when using these aggregates instead of the individual-level data (Table 3.B.5).

Fourth, our baseline results are also robust to using an alternative outcome variable from the GWP, asking individuals "In the next 12 months, are you likely or unlikely to move away from the city or area where you live?" Since this question does not explicitly ask for *international* migration, it covers internal migration as well. Results remain very similar in magnitude and significance (Table 3.B.6). We further test if aid announcements affect individual level capabilities to migrate. Following our argument, we do not expect aid announcements to change individual level capabilities to migrate, as these announcements come with no immediate material benefits in recipient countries. To test this, we exchange the outcome variable to migration plans based on the question "Are you planning to move permanently to that country in the next 12 months, or not?" and migration preparations based on the question "Have you done any preparation for this move?"[32] As we show in Table 3.B.7, there is no systematic effect of aid announcements on migration plans and preparations after project approval.

Finally, results are robust to changing the level at which standard errors are clustered to the level of provinces, provinces and countries, countries and dates, and to country×dates (Table 3.B.8).

3.4.3 Mechanisms

It is unlikely that World Bank board approvals have any immediate impact on the individual's income, job, and well-being as any project-related disbursement typically only occurs several months after the decision. However, project announcements immediately provide respondents with new information that might affect how they assess the expected *future* relative benefits of staying versus migrating. To better understand the mechanisms behind the project announcement effect, we now analyze key effect heterogeneities and possible mediating variables.

We start with effect heterogeneities. We expect the effects of aid on migration preferences to be stronger among individuals who are more likely to benefit from these projects, in particular among younger individuals where future benefits of aid projects enter with

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[31]This forces us to alter the survey controls we use. We drop day-of-the-week fixed effects since we aggregate the data across different dates to the pre- and post-treatment period. To control for interview month, we take the month of the first interview pre-treatment, and month of the treatment for the post-treatment period.

[32]Note that these questions were only asked once the respondent indicated migration preferences.

a longer expected time horizon. In addition, we expect these effects to be stronger among individuals who are more likely to be exposed to national and local news. To test both hypotheses, we interact the *Post* dummy in eq. 3.1 with individual-level characteristics. Table 3.2 shows that as expected, the announcement effect is significantly stronger for individuals under the age of 30 and those with access to the internet. We do not find significantly different effects across gender, households with a family member abroad, or individuals who are unemployed.

Table 3.2 – Migration preference and World Bank announcements, individual-level heterogeneities

|	(1)	(2)	(3)	(4)	(5)
	Female	Under 30	Internet access	Member abroad	Unemployed
Post	-0.00582	-0.00091	-0.00415	-0.00976	-0.00915**
	(0.00469)	(0.00480)	(0.00417)	(0.00694)	(0.00394)
Characteristic	-0.04353***	0.00791**	0.03858***	0.12640***	0.07719***
	(0.00444)	(0.00347)	(0.00404)	(0.00976)	(0.00523)
Interaction	-0.00701	-0.02038**	-0.01749***	-0.00804	0.00054
	(0.00769)	(0.00904)	(0.00646)	(0.01458)	(0.00987)
Observations	952336	952336	942836	104936	909862
Mean DV	0.232	0.232	0.232	0.218	0.234

Notes: This table presents the results from eq. 3.1, where we interact the independent variable and the instrument with individual level characteristics as indicated in column titles. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Post" indicates a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, whether or not the household has a child as well as province-year-month fixed effects and day-of-the-week fixed effects. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

We now turn to the suggested mediating variable and test whether aid announcements lead to expected improvements of future living conditions in the country of origin. For this, we use two variables from the GWP that measure the individual's perception of their life today and their life in the future. 'Life today' asks the respondents to imagine a ladder with steps numbered from 0 at the bottom to 10 at the top and indicate on which ladder they feel like standing today. The top of the ladder represents the best possible life and the bottom of the ladder means the worst possible life. The second question, 'Life in the future', asks which step of the ladder the respondents think they will stand on in five years. We use these two variables in three different ways to ensure that definitions do not drive our results. First, we create a binary variable indicating that the respondent thinks life in the future will be better than today. Second, we calculate the difference between the two variables subtracting life today from life in the future. Third, we use life in the

future as the outcome variable, controlling for life today.[33]

Figure 3.4 shows the results in an event study plot. Again, this figure plots coefficients and 90 percent confidence intervals for interactions between the binary variable that indicates whether the individual was interviewed before or after World Bank project announcement and 22 dummies for three-day periods included between 30 days (4 weeks) before and after the approval date. While we do not observe significant pre-trends, there is a significant increase in the respondent's perception of the future in the days after World Bank project approvals.

Figure 3.4 – Perceptions of the future and World Bank announcements, event study plot
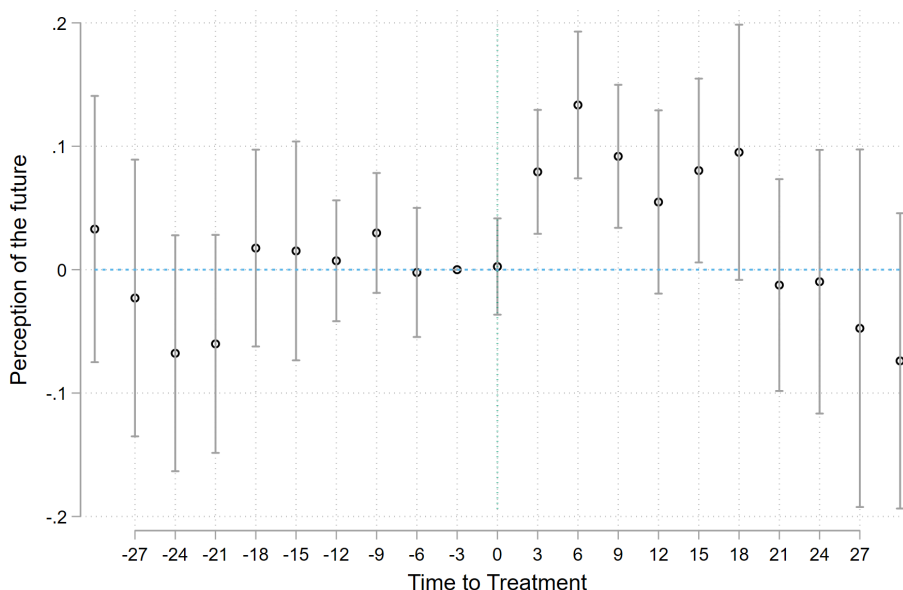
Notes: This figure plots the coefficients and 90 percent confidence intervals for a regression based on eq. 3.1 where we augment the specification with interactions between the binary variable indicating indicating whether the individual was interviewed after World Bank project board approval or not and 22 dummies for 3-day period included between 30 days (4 weeks) before and after the approval date. Outcome variable is how the individual perceives her life in the future relative to her life today. Standard errors are clustered at the country level.

We present the detailed results in Table 3.3. We find a strong positive effect of World Bank project announcements on individual perceptions of the future. While this effect is strongest for the relative measure in columns 3 and 4, it persists when controlling for perceptions of life today in columns 5 and 6. In line with our argument, project announcements give citizens a more optimistic outlook of the future, while at the same time decreasing the number of individuals expressing migration preferences.

Finally, we test if the effect of project announcements on migration preferences runs *through* perceptions of the future. As we argue, the only possible channel on how aid

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[33]The descriptive statistics on these variables are also provided in Table 3.A.3.

Table 3.3 – World Bank announcements and perceptions of the future

	(1)	(2)	(3)	(4)	(5)	(6)
	Future better		Relative future		Life in the future	
Post country	0.01491***	0.01402**	0.06184***	0.05893***	0.03576*	0.03232*
	(0.00564)	(0.00554)	(0.02283)	(0.02227)	(0.01898)	(0.01851)
Observations	853422	154041	853422	154041	853422	154041
Individual controls	✓	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓	✓	✓
15-day-window	.	✓	.	✓	.	✓
Life today	.	.	.	.	✓	✓
Mean DV	0.710	0.738	1.618	1.669	6.623	6.592

Notes: This table presents the results from eq. 3.1, where we change the outcome variable to perceptions of the future. For this, we use the questions on perceptions of the respondent's life today ("Please imagine a ladder with steps numbered from 0 at the bottom to 10 at the top. Suppose we say that the top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time, assuming that the higher the step the better you feel about your life, and the lower the step the worse you feel about it? Which step comes closest to the way you feel?") and the respondent's life in the future (..."On which step of the ladder would you say you will stand on in the future, say about 5 years from now?") Columns 1 and 2 use a dummy variable indicating if life in the future is expected to be better than life today, columns 3 and 4 use the difference between life in the future and today, and columns 5 and 6 use life in the future as outcome, controlling for life today. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

announcement can alter migration preferences in recipient countries in the days immediately following the project announcement is by improving perceptions of the future. We thus take project announcement as an instrument in a 2SLS regression of migration preferences on perceptions of the future. As we show in Table 3.B.11, there is a significant positive effect of an improvement of the perception of the future (instrumented by project announcement) on migration preferences.[34]

We see three main takeaways from the analysis of information flows and mechanisms. First, individuals in recipient countries seem to be aware of aid announcements. Second, they seem to expect a positive effect of aid projects on local opportunities in the future. Third, when forming a preference for migration (or not), they consider these future opportunities created by aid projects and are less likely to develop migration aspirations when projects in their country are announced. In what follows, we investigate whether and how these short-term effects evolve over time once aid projects are being implemented.

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[34]Since the instrument does not achieve the standard levels of the F-statistic, we must be cautious in interpreting these results.

3.5 Disbursement Effects

3.5.1 Empirical Approach

We now move from World Bank announcements to World Bank disbursements. As we argued in the previous section, announcements constitute national events that are echoed in the recipient country media and received by its population, in particular among young and well-informed individuals. We argue that the receipt of this news increases individual perceptions of the future and consequently reduces demand for migration. At project approval, however, relatively little concrete information are spread about the project details, and even less is known about the concrete implementation and the future effects of these projects. As we show, the effect of project announcement is not restricted to specific provinces in recipient countries, but rather a nation-wide effect.

As projects advance to the implementation stage, more information becomes available to individuals in the vicinity to project sites. Every month, the World Bank transfers funds into specific locations in the recipient country that increase local demand for goods, jobs, and services. With our data, we are able to precisely measure the specific month of these financial flows for each World Bank project. As compared to announcement effects, project disbursements are not single-standing events but occur in regular frequency throughout the project life, which is why we regress our individual-level migration preferences variable on georeferenced World Bank disbursements at the province-year level by means of the following estimation equation:

$$migration_{ipcmy} = \beta aid_{pcmy} + \delta X_{ipcmy} + \zeta_{cy} + \eta_p + \epsilon_{ipcmy}, \qquad (3.2)$$

where $migration_{ipcmy}$ represents different outcomes capturing migration preferences and development outcomes for individual i living in province p of country c in month m of year y. aid_{pcmy} is our variable of interest, World Bank aid disbursements in millions of constant 2014 US dollars in a given province p of country c in the month before the interview month m in year y. As described in Section 3.3, we follow Kersting and Kilby (2016) and construct province-level World Bank aid disbursements by aggregating the project-level amounts disbursed over time following the exact project-level disbursement schedules. The availability of monthly disbursement observations allows us to aggregate at any frequency down to the month prior to each individual's GWP interview date.[35] X_{ipcmy} are the same individual-level control variables as in eq. 3.1. ζ_{cy} denotes country-year fixed effects, and η_p denotes province fixed effects. ϵ_{ipcmy} is the error term. In our preferred specification, we cluster standard errors at the country level, but show robustness of our results to alternative other cluster decisions below.

Exploiting province-level variation in aid disbursements over time allows us to rely on a

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[35]Table 3.A.3 provides the descriptive statistics on these disbursements.

tight set of fixed effects which absorb any potential confounding variables at the country-year level (e.g., macroeconomic shocks, national conflicts, or changes in migration policies) and time-invariant factors at the level of each recipient province within a given country (e.g., geography, availability of natural resources, or the aggregate level of development). This accounts for a wide range of potential confounders and represents a clear advantage over most of the empirical approaches used in the existing literature on the topic that have typically relied upon country-year variations in foreign aid allocation.

Nonetheless, there are remaining threats to identification to consider. These include time-varying factors at the province level as long as these factors are correlated with both aid disbursements and changes in migration and development outcomes simultaneously. This could be the case with provincial conflict episodes and subsequent reconstruction efforts, local development interventions, or *precisely* when donors target specific provinces due to increasing emigration rates. This may introduce an omitted variable bias when estimating eq. 3.2.

To address such endogeneity concerns, we implement an instrumental variable strategy motivated by recent contributions of Kraay (2012, 2014) and Andersen et al. (2022). Our instrument exploits variation in the time lag between the commitment and the disbursement of World Bank projects. More precisely, we construct *synthetic* disbursement schedules by interacting the initial commitment value of each project at approval with the average disbursement share of all projects from the same sector and same world region. Using synthetic disbursements to instrument for actual disbursements provides plausibly exogenous variation in the amount of project funding inflows over time since the former are, by construction, unaffected by province-time-specific confounders as discussed above.

Some institutional background helps to understand the reasoning behind this instrumental variable strategy. World Bank lending is organized by project. Individual projects are designed and set up following a consultation process with the borrower (again, see Figure 3.A.3 for the typical project life cycle). The borrower and the World Bank then conclude an agreement on the envisioned amount of spending and set up a loan agreement that fixes the lending conditions, including the committed financial amount, the terms and conditions of the loan, as well as the intended disbursement plan. A typical project takes several years from the board approval day to the last disbursement and completion of the project.

Unsurprisingly, not every World Bank project is disbursed as initially planned. Project disbursements are subject to World Bank staff discretion, project progress, technical and procurement issues, borrower performance, or any shock in the recipient country. For example, Kersting and Kilby (2016) show evidence of faster disbursement of World Bank investment project loans when countries are politically aligned with the United States and this effect becomes even stronger before competitive executive elections. All this can create substantial deviations in actual disbursements from the planned disbursement

laid out at project approval. The instrumental variable approach we implement follows the idea that, while the project design at approval might be correlated with *past* and *contemporaneous* shocks in the recipient country or province, the instrumental variable is uncorrelated with *future* shocks unknown at the time of approval. In other words, if project disbursements follow the initial disbursement plan set up at approval, then fluctuations in disbursements on projects approved in previous years will be uncorrelated with contemporaneous shocks.

Data on planned project disbursement schedules at the time of project approval are not publicly available. In the absence of such information, for each World Bank project, we create *synthetic* disbursement schedules based on comparable projects. We define *comparable* projects as those implemented in the same sector and world region and create synthetic disbursement schedules based on the average schedules of the sample of comparable projects.[36] For each project, this provides us with an average disbursement schedule that indicates the monthly disbursement share from project approval to the last disbursement.[37] We then multiply the initial loan commitments with these average loan disbursement shares to create synthetic disbursements. Finally, we aggregate the synthetic disbursements to the province-month-year level to match them with the GWP data.

Conditional on eq. 3.2, synthetic disbursements are thus uncorrelated with the error term and hence satisfy the exclusion restriction. Our baseline approach thus uses synthetic disbursements in a Two-Stage-Least Squares (2SLS) regression framework, replacing aid_{cpmy} in eq. 3.2 with $\widehat{aid}_{pcmy}$, based on the following first-stage regression:

$$aid_{pcmy} = \beta \widehat{aid}_{pcmy} + \delta_{ipcmy} + \zeta_{cy} + \eta_p + \epsilon_{ipcmy}, \tag{3.3}$$

where $\widehat{aid}_{pcmy}$ denotes the sum of all synthetic disbursements of World Bank aid in province p of country c in the month before interview month m of year y. We thus compare outcomes across individuals of different provinces within a country receiving different amounts of World Bank aid over time, during the month(s) prior to the interview.

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[36]We only include projects with available information on the project location and projects where a significant part of the initial commitment is disbursed (≥ 50 percent). Project sectors follow the sector II definitions as presented in Table 3.A.4. World regions follow the World Bank categorization: East Asia & Pacific, Europe & Central Asia, Latin America & Caribbean, Middle East & North Africa, North America, South Asia, and Sub-Saharan Africa.

[37]Figure 3.C.1 shows the uniform average disbursement pattern for all projects, as well as the sector specific disbursement pattern. We exclude disbursements made in the commitment year as they may potentially be endogenous to anticipated shocks at project approval date. With respect to the spatial distribution of aid disbursements for multi-location projects, we calculate average disbursements by project location by dividing total disbursements per project and time by the number of project locations.

3.5.2 Results

We begin by analyzing the aggregate effect of World Bank aid disbursements in the month before the interview on migration preferences based on an estimation of eq. 3.2 on the full sample of 952,713 individual observations from the GWP. The baseline results are reported in Table 3.4. Panel A provides the OLS results with columns 1 through 5 reflecting different variations of the estimation equation including individual controls and an increasingly tight set of fixed effects, with the final column being our preferred specification. From left to right, the OLS results show statistically significant negative point estimates (columns 1–3), which turn zero in our preferred specification (column 5).

Table 3.4 – Migration preferences and World Bank disbursements

	(1)	(2)	(3)	(4)	(5)
			Migration preferences		
Panel A: OLS					
Disbursements	-0.00241**	-0.00231**	-0.00233**	-0.00012	0.00001
	(0.00105)	(0.00093)	(0.00092)	(0.00014)	(0.00010)
Panel B: First stage					
Synthetic disbursements	0.88936***	0.88973***	0.89530***	0.88978***	0.98580***
	(0.04904)	(0.04873)	(0.04857)	(0.05927)	(0.13441)
Panel C: Second stage					
Disbursements	-0.00976***	-0.00923***	-0.00860***	-0.00112*	-0.00268***
	(0.00297)	(0.00265)	(0.00243)	(0.00058)	(0.00073)
Observations	952713	952713	952713	952713	952713
Individual controls	.	✓	✓	✓	✓
Year FE	.	.	✓	✓	✓
Country FE	.	.	.	✓	✓
Country-year FE	.	.	.	.	✓
Province FE	.	.	.	.	✓
Mean DV	0.232	0.232	0.232	0.232	0.232
F-stat	328.9	333.3	339.8	225.4	53.8

Notes: This table presents the results from eq. 3.2. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank financial amounts disbursed in the month prior to the interview month in million, constant 2014 US$. This variable is the outcome variable in Panel B ("First stage"). "Synthetic disbursements" are aggregated, synthetic project level disbursements in million, constant 2014 US$ based on disbursement schedules of projects in the same sector and world region. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors are clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Given the risk that the simple OLS estimates could be affected by an omitted variable bias, in what follows, we focus on the estimations from our instrumental variable approach. In the first stage (panel B), the coefficient of the instrumental variable is positive as expected and highly statistically significant. Depending on the specification, a one

dollar increase in synthetic disbursements translates into a 88.9–98.6 US cent increase in actual disbursements. The second-stage results are reported in panel C. The first-stage F-statistic reaches a value of between 54 and 340, signaling that the instrumental variable is highly relevant in the given context. The point estimate for our outcome of interest—migration preferences—is negative and highly significant, providing evidence that aid reduces migration preferences at the provincial level. The point estimate in our preferred specification in column 5 indicates a decrease of 2.7 percentage points for an increase in foreign aid receipts to the specific province worth 10 million US\$. Comparing the point estimates in column 5 between the OLS specification (panel A) and the IV second stage (panel C) suggests that the OLS coefficient is biased upwards. This would occur if omitted variables were positively correlated with both migration preferences and aid disbursements at the same time. This is a plausible scenario in the case of provincial economic shocks and natural or man-made disasters, which would typically be spurring migration and aid inflows to the affected province simultaneously. We are hence confident that the IV approach is successful in tackling this endogeneity problem and also that the baseline results provide evidence of a negative short-term causal effect of foreign aid on migration preferences.

We repeat this analysis for longer-term lags of aid disbursements between 1 to 5 years to explore the dynamics of this effect over time. To be precise, Table 3.5 replaces our right-hand side variable of interest, aid disbursements in the month prior to the interview date, by cumulative disbursements in the 12 months preceding the interview date (column 1) and its first, second, third, and fourth lag, respectively (columns 2–5). The point estimate with the first yearly lag of disbursements in column 1 is again negative and highly significant, with the magnitude being somewhat larger than the one implied by the one-month lag.[38] The coefficient indicates a decrease in migration preferences of 0.3 percentage points for an additional annual disbursement of 10 million US\$, on average. Comparing the coefficient across columns shows that the effect is most pronounced in the first year after the aid disbursement. Analyzing longer lags, we still observe negative and statistically significant estimates, but their magnitudes are decreasing (column 2–3) and become indistinguishable from zero in the fourth year after disbursement. Taken together, these results suggest that the negative effect of aid disbursements on migration preferences occurs in the short run, but disappears in the longer run.

In what follows, we explore various dimensions of heterogeneity regarding the effects on migration preferences. First, we analyze potential mediation variables. Following our approach for aid announcements above, we interact the treatment variable with individual-level characteristics (Appendix Table 3.C.5). In line with the findings on aid announcements, the effect of aid disbursements is stronger among the young and highly

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[38]To compare this coefficient to the one from the monthly specification reported in Table 3.4, we divide it by 12 (months). This yields an implied coefficient of 0.00022.

Table 3.5 – Migration preferences and World Bank disbursements, longer-term effects

	(1)	(2)	(3)	(4)	(5)
	Migration preferences				
12 months, lagged	**0-12**	**13-24**	**25-36**	**37-48**	**49-60**
Disbursements	-0.00029***	-0.00034***	-0.00023*	-0.00007	0.00002
	(0.00007)	(0.00010)	(0.00012)	(0.00009)	(0.00008)
Observations	952713	952713	952713	952713	952713
Individual controls	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓
Mean DV	0.232	0.232	0.232	0.232	0.232
F-stat	129.5	61.0	62.6	50.5	25.4

Notes: This table presents the results from eq. 3.2, using longer aggregates and lags. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures lagged aggregated World Bank financial amounts disbursed in the 12 months prior to the interview month (or lags of this variable, as indicated in column titles) in million, constant 2014 US$. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors are clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

skilled individuals. Arguably, the young and highly educated have a longer time horizon, are less established economically and hence more flexible regarding new employment opportunities, and are better equipped for local labour market opportunities. We do not observe a differential effect by gender, urban residence, or parenthood. In addition, individuals with a more positive view of their future are less likely to express migration preferences with higher levels of aid disbursements.

Second, we study the heterogeneity across world regions (Appendix Table 3.C.6). The results suggest that the short-term effect of aid disbursements on migration preferences is systematically negative and significant across all regions of the world, but Asia and Sub-Saharan Africa. For Asia, the point estimates is close to zero, indicating no effect, while for SSA it is negative yet noisily estimated. The latter is due to strong heterogeneity within Africa with respect to state fragility. In unreported regressions, we split the African sample into fragile and non-fragile states and we find that the point estimate is strongly negative and statistically significant for the subgroup of non-fragile states, while it is close to zero for fragile ones.[39] This suggests that aid reduced migration preferences in African countries that do not suffer from instability, while this effect disappears in the fragile nations. This is likely driven by a lack of aid impact on development outcomes in these places, which we explore below.

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[39]To do so we rely on data from the State Fragility Index (SFI) to categorize countries with respect to their fragility. On a scale between 0 and 120, with larger values indicating more fragility, we define states with an FSI above 90 as fragile, based on data from 2020. https://fragilestatesindex.org/.

Third, we also analyze recipient country heterogeneity by splitting the sample in different classes of the World Bank Income Classification (Appendix Table 3.C.7). The results provide evidence that the negative effect of aid disbursements on migration preferences exists everywhere, but that it is most pronounced in low-income countries.

Finally, we investigate project-level heterogeneity. For this, we split World Bank projects into the three broad sectors: Social Infrastructure and Services, Economic Infrastructure and Services, and Production Sectors (see details in Appendix Table 3.A.4). To be able to translate eq. 3.2 to the sectoral level, we aggregate disbursements and synthetic disbursements by sector and province-year-month (columns 1–3 of Appendix Table 3.C.8). The negative effect of aid disbursements on migration preferences is prevalent across all three broad sectors in the short to medium term. The magnitude is strongest in the Production Sector (i.e., agriculture, forestry, and fishing, as well as industry and mining), where an additional US\$ 10 million spent during the last 12 months reduces migration preferences by 0.7 percentage points, which is approximately twice as large as our baseline estimate and those for the other target sectors.

3.5.3 Robustness Checks

We test the robustness of our baseline estimates in several ways and report the results from these exercises in Appendix B. First, we test the relevance of the instrument. For this, we augment eq. 3.3 with four leads and lags of the synthetic disbursement variable to show that changes in pre-determined synthetic disbursements also translate into changes in actual disbursements. We find that the synthetic disbursements directly translate into actual disbursements at time t, while there is no significant effect on past or future disbursements (Appendix Figure 3.C.2).

Second, we test our main identifying assumption, i.e., exogeneity of the instrumental variable. As planned project disbursements are a function of past project approvals, synthetic disbursement schedules must not be correlated with shocks occurring during the disbursement stage of projects. We follow Kraay (2012, 2014) and use a reduced form specification to show that there is no significant relation between synthetic disbursements and migration preferences (other than through actual disbursements). We augment the reduced form of our instrumental variable approach by four leads and lags of the synthetic disbursement variable (Appendix Figure 3.C.3). Reassuringly, neither past nor future synthetic disbursements are correlated with migration preferences. This provides evidence that our identifying assumption is plausible.

Third, we conduct a randomization inference test to show that our results are not driven by spurious correlations. The test randomizes the amount of World Bank aid disbursements and the corresponding instrumental variable across different countries and years in the sample (Appendix Figure 3.C.4). All coefficient estimates from this exercise

are concentrated around zero. According to an exact Fisher test, the coefficient from our province-level-aggregated estimate above (depicted by the vertical dashed line) is significantly different from the randomized coefficients (p-value of 0.037). This also holds when we modify the time dimension and randomize the entire time series between countries, years within countries, and countries within years. In summary, the results of our randomization test show that it is very unlikely that any omitted variables correlate with our variables of interest in a way that would spuriously produce our main results.

Fourth, we provide evidence that our results are robust to different empirical choices related to the construction of synthetic disbursements (Appendix Table 3.C.1). Our baseline results and the F-statistic remain almost identical when calculating the synthetic disbursements based on disbursement schedules of all projects, projects in the same sector, or projects in the same world region. They remain also robust when excluding the project itself, or all projects from the same country when calculating the synthetic disbursement schedules.

Fifth, we also test for the robustness of our baseline results regarding the definition of our independent variable of interest, foreign aid disbursements. While our preferred definition is using absolute monetary values of aid disbursements for ease of interpretation, we also use alternative ones including aid disbursements per province capita, or when using the log of aid in per capita values (see Appendix Table 3.C.2).

Sixth, we show that our baseline results hold when dropping the survey sampling weights and when aggregating the data at the province-year level, both when using weighted means and unweighted means (Appendix Table 3.C.3).

Finally, in Table 3.C.4, we alternate the level at which standard errors are clustered. The instrumental variable results are robust when clustering standard errors at the level of provinces, country×year, and province×year. They are also robust to clustering standard errors at the precise interview date, country×date, and province×date.

3.5.4 Mechanisms

Leveraging the wealth of individual-level data available from the GWP, we analyze how changes in aid lead to changes in migration preferences. Specifically, we test how World Bank aid project disbursements affect individual attitudes and welfare outcomes in recipient provinces. This constitutes one of our main contributions with respect to the aid effectiveness debate and further helps explaining the mechanisms through which aid changes migration preferences. Motivated by the existing literature and the World Bank's own development goals, we select the following dependent variables: poverty, income, unemployment, public amenities, confidence in national institutions, perceptions of the

future, and perceptions of the economy.[40] We test the effects of aid on these variables by altering the outcome variables in eq. 3.2.

The main results are reported in Table 3.6. For the readers' convenience, column 1 reports again the baseline results for migration preferences using lags of aid disbursements between 1 month and 5 years. One of the most frequently declared objectives of foreign aid—and at the core of the World Bank's mission—is poverty reduction. In column 2, we therefore present results on the effect of aid disbursements on extreme poverty, measured with a binary variable that takes a value of one if the respondent reports a per capita income of below US$ 2.15. Column 3 uses the (logged) reported US$ value of the individual's income per capita as dependent variable. The estimated coefficients for both outcome variables are basically zero in the short to medium term, suggesting no effect on welfare measures. However, at lags of four and five years, respectively, the coefficients become statistically significant with a negative sign for extreme poverty and a positive sign for income per capita. The point estimates on the five-year lag suggest an average poverty reduction of 0.4 percentage points and an increase in income per capita of 0.4 percent for annual aid disbursements worth 10 million US$ at the province level. These results thus provide evidence of positive individual welfare effects of World Bank aid project disbursements in the longer term, between four to five years after disbursement.Column 4 reports the results for unemployment, using a binary dependent variable indicating whether the individual reports being unemployed or not. Comparing the point estimates across panels shows that they are close to zero at any time, indicating no effect on unemployment whatsoever.

In columns 5 and 6, we explore aid effects on respondents' satisfaction with public amenities and confidence in national institutions. We measure the former with an index from GWP that ranges between zero and one, with higher values reflecting greater levels of satisfaction.[41] We measure the latter with another GWP index that captures the respondents' confidence in key public institutions of the country of residence.[42] While the resulting coefficients on public amenities are close to zero and statistically insignificant across all lags, the coefficients on trust in national institutions are positive and significant

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[40]The World Bank uses so-called development *scorecards* as internal achievement measures. They report the World Bank's and client performance relative to a set of quantitative goals and performance measures. See https://scorecard.worldbank.org (accessed 27 September 2023). Again, we provide descriptive statistics in Table 3.A.3.

[41]The index is based on seven questions that all start with "In the city or area where you live, are you satisfied or dissatisfied with" and then ends with "the public transportation systems," "the roads and highways," "the quality of air," "the quality of water," the "availability of good affordable housing," "the educational system or the schools," and "the availability of quality healthcare." The index is the share of subquestions to which the respondent replies with "satisfied".

[42]This index is based on the question "Do you have confidence in each of the following, or not?" and then computes the share of institutions for which the respondent replies with "yes" out of the following: the judicial system and courts, the military, the national government, and the honesty of elections. The index has a minimum value of 0 and a maximum of 100, with higher values indicating a more positive view of the national institutions.

in the short to medium term, i.e., up to two years after disbursement. The point estimate suggests an increase in approximately 0.4 index points for additional annual aid disbursements of US\$ 10 million. This effect, however, disappears in the medium term and then turns negative and significant in the longer term (five years after disbursement). This result is consistent with an increase in confidence regarding national institutions in the short term, when individual first learn about new aid projects, and later disappointment regarding those projects.

In the remaining columns 7 and 8, we analyze respondents' perceptions of their own future and that of the national economy.[43] The coefficients in column 7 are consistently close to zero across panels, indicating no effect. Turning to the perceptions about the national economy, despite lacking statistical power, we observe a similar pattern as in column 6: the point estimates are positive and relatively large in the short term, with the point estimate for the 12 month lag indicating a 0.44 index point increase for aid disbursements of 10 million US\$ (i.e., translating into a 20% increase at the mean). This is consistent with our results regarding the short-term announcement effects. However, starting at lag three, coefficients switch signs and increase in magnitude in the longer term, indicating more negative perceptions.

Summing up, in line with the announcement effects, the negative effects of aid disbursements on migration preferences coincide with more positive perceptions about national government institutions and those about the national economy in the short run. This can be interpreted as increased local aspirations, while capabilities as measured by selected welfare outcomes, remain constant. However, as the negative effect on migration preferences disappears in the longer term, this coincides with more negative perceptions about the future and positive individual welfare outcomes, as measured by reductions in extreme poverty and income increases. Based on our conceptual framework, this can be interpreted as a simultaneous decline in local aspirations and increase in capabilities, which could translate into larger migration flows, which we analyze in the next section.

3.5.5 Migration and Asylum Seeker Flows

Having analyzed the effect of aid on individual migration preferences as a measure of aspirations as well as individual attitudes and welfare outcomes as a measure of capabilities, we now turn to realized migration flows. For this, we rely on annual bilateral flow data of migration and asylum seekers from the OECD International Migration Database.

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[43]For perceptions of the future, we use the "future better" variable from the announcement effect analysis above, indicating that an individual expects a better life in the future than today. Perceptions of the national economy are based on GWP indices, both ranging from zero to one. "National economy" is based on two questions: "Right now, do you think that economic conditions in this country, as a whole, are getting better or getting worse?" and "How would you rate your economic conditions in this country today –as excellent, good, only fair, or poor?" The index has a minimum value of -100 and a maximum of 100, with positive values indicating a positive view of the economy.

Table 3.6 – Disbursement effects on individual welfare and perceptions

	(1) Migration preference	(2) Extreme poverty	(3) Income (per capita)	(4) Un-employment	(5) Public amenities	(6) National institutions	(7) Future better	(8) National economy
Panel A: Month 0-1								
Disbursements	-0.00268***	-0.00009	0.00105	0.00010	-0.00043	0.24964*	0.00120	0.24195
	(0.00073)	(0.00021)	(0.00188)	(0.00029)	(0.00095)	(0.14504)	(0.00113)	(0.19585)
Observations	952713	895349	895349	910219	738804	803125	853826	529223
Mean DV	0.232	0.072	7.099	0.073	0.576	51.342	0.710	-2.252
F-stat	53.8	50.3	50.3	46.7	54.9	43.9	50.8	17.9
Panel B: Month 0-12								
Disbursements	-0.00029***	0.00001	0.00015	0.00003	-0.00000	0.03729**	0.00021	0.04379
	(0.00007)	(0.00003)	(0.00028)	(0.00003)	(0.00014)	(0.01770)	(0.00014)	(0.03303)
Observations	952713	895349	895349	910219	738804	803125	853826	529223
Mean DV	0.232	0.072	7.099	0.073	0.576	51.342	0.710	-2.252
F-stat	129.5	115.9	115.9	99.5	89.3	131.5	122.8	172.8
Panel C: Month 13-24								
Disbursements	-0.00024***	0.00000	0.00029	0.00002	-0.00003	0.02007*	0.00011	0.02738
	(0.00006)	(0.00002)	(0.00037)	(0.00003)	(0.00011)	(0.01131)	(0.00010)	(0.02443)
Observations	952713	895349	895349	910219	738804	803125	853826	529223
Mean DV	0.232	0.072	7.099	0.073	0.576	51.342	0.710	-2.252
F-stat	131.6	113.9	113.9	107.2	167.6	114.6	127.4	613.0
Panel D: Month 25-36								
Disbursements	-0.00018**	-0.00002	0.00041	0.00001	-0.00003	0.00587	0.00008	-0.00657
	(0.00009)	(0.00003)	(0.00038)	(0.00003)	(0.00012)	(0.01029)	(0.00007)	(0.02080)
Observations	952713	895349	895349	910219	738804	803125	853826	529223
Mean DV	0.232	0.072	7.099	0.073	0.576	51.342	0.710	-2.252
F-stat	282.6	259.9	259.9	272.5	271.6	256.2	254.7	224.0
Panel E: Month 37-48								
Disbursements	-0.00005	-0.00003	0.00048*	0.00000	-0.00002	-0.00677	0.00010	-0.01022
	(0.00007)	(0.00003)	(0.00029)	(0.00002)	(0.00011)	(0.01001)	(0.00008)	(0.01139)
Observations	952713	895349	895349	910219	738804	803125	853826	529223
Mean DV	0.232	0.072	7.099	0.073	0.576	51.342	0.710	-2.252
F-stat	628.2	701.3	701.3	608.1	493.7	542.5	620.8	217.7
Panel F: Month 49-60								
Disbursements	0.00001	-0.00004**	0.00042**	-0.00000	-0.00006	-0.01367	0.00009	-0.02059
	(0.00005)	(0.00002)	(0.00017)	(0.00001)	(0.00008)	(0.00876)	(0.00007)	(0.01392)
Observations	952713	895349	895349	910219	738804	803125	853826	529223
Mean DV	0.232	0.072	7.099	0.073	0.576	51.342	0.710	-2.252
F-stat	439.9	402.9	402.9	391.4	369.0	377.6	403.4	40.9

Notes: This table presents the results from eq. 3.2, where we change the outcome variable to test the different mechanisms. "Migration preference" indicates if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Extreme Poverty" indicates whether or not the individual's income is below the World Bank poverty line of US$ 2.15 per capita per day, or not. "Income (per capita)" measures logged per-capita income of the respondent in constant US$. We winsorize this variable one-sided at the 99-percent level. "Unemployment" indicates if the individual indicates to be unemployed. "Public amenities" measures satisfaction with public amenities based on a GWP index. "National institutions" measures confidence in national institutions based on a GWP index. "Future better" is a dummy variable indicating if life in the future is expected to be better than life today. "National economy" measures perceptions of the national economy based on a GWP index. "Disbursements" measures aggregated World Bank financial amounts disbursed in the months indicated prior to the interview month in million, constant 2014 US$. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

These data are available for 96 countries in our sample over the study period (2008–2019). This allows us to estimate the effects of aid on flows of migrants and asylum seekers from recipient countries. For this, we aggregate our disbursement dataset to the country-year level and merge it with the bilateral migration data. We estimate the following regression equation:

$$flow_{cdy} = \beta aid_{cy-l} + \delta X_{cy} + \kappa_{cd} + \lambda_{dy} + \epsilon_{cy}, \tag{3.4}$$

where $flow_{cdy}$ is the logged number of migrants or asylum seekers, respectively, from country of origin c to country of destination d in year y. aid_{cy-t} is our variable of interest, World Bank aid disbursements in millions of constant US dollars to the country of origin c in year $y - l$, with l representing different monthly or yearly lags. We instrument this variable with "synthetic disbursements" as outlined above but aggregate it to the country-year level. X_{cy} is a vector of control variables and includes the country-year weighted averages of our individual-level controls in eq. 3.2. κ_{cd} denotes bilateral origin-destination fixed effects, and λ_{dy} denotes destination-year fixed effects. Note that the bilateral fixed effects included in this regression equation absorb any time-invariant factors of specific migration corridors accounting, for example, for travel distance, linguistic similarities, colonial ties, or bilateral migration networks. Destination-year fixed effects absorb potentially confounding changes at the level of the destination country, such as changes in immigration policies, as well as time-varying global shocks that affect all origin countries alike. ϵ_{cy} denotes the error term and we cluster standard errors at the country level.

Table 3.7 presents the baseline results from estimating eq. 3.4 with different lags l of aid disbursements (panels A–E). Columns 1 to 3 report the results for regular migration flows, which include mostly economic migrants. Columns 4 to 6 then report the estimates for asylum seeker flows, as reported by OECD member states' national registries. As reported in panel A, in the short term, flows of asylum seekers respond negatively to aid disbursements, i.e., we observe a decrease in asylum seeker outflows from aid receiving provinces. We do not observe any effect for regular migrants. The point estimate from our preferred specification in column 6 indicates a decrease of around 0.6 percent in flows for additional World Bank disbursements worth 10 million US$ at the country level. This effect persists up to the second year after disbursement and then vanishes. Note that this effect is consistent with the short-term announcement and disbursement effects on migration preferences, and the corresponding improvements in individual attitudes, described above. Taken together, this suggests that decreases in migration preferences actually translate into short-term reductions in emigration, and affect particularly asylum seekers.

As reflected in the estimates reported in columns 1–3, in the medium term, starting

between two to three years after disbursements (panel B and C), we observe that aid causes increases in regular migration flows. The point estimate in column 3 (panel C) indicates an increase of 0.5 percent for additional aid disbursements worth US$ 10 million. This effect is persistent over time. It is reassuring that this effect overlaps with our longer term disbursement effects on migration preferences, and the corresponding improvements in individual welfare. This suggests that positive aid effects as measured by reductions in poverty and improvements in income per capita translate into increases in migration flows. These longer term changes in migration flows materialize particularly through regular migration. This can be interpreted through the lens of the mobility transition theory in which aid increases individual capabilities to migrate, which then leads to increases in regular migration.

We conduct a range of heterogeneity analyses on the regular migration flow results using our preferred specification. First, we compare the findings across world regions (Appendix Table 3.D.1). Regarding the short-term decrease of asylum seeker flows to the OECD, we find this effect to be driven by Latin American origin countries as well as the Middle East and North Africa. Asylum seeker flows from Latin America react relatively quickly to aid disbursements (lags 1 and 2 indicate sizable effects), but this effect is short-lived. In the Middle East and North Africa the effect only occurs with a delay of one year but is persistent up to 5 years after disbursement. For Sub-Saharan Africa, we find this effect to occur only in the subsample of non-fragile states (Appendix Table 3.D.2). This is consistent with our earlier findings on negative aid effects on migration preferences in non-fragile African countries. The lack of aid effectiveness in the subsample of fragile states in Sub-Saharan Africa aligns well with the null effect on asylum seeker flows (and regular migration) in fragile settings.

By contrast, the longer term increase in regular migration flows to the OECD is driven by emigration from Latin America, Sub-Saharan Africa, and—to a lesser extent—Europe & Former USSR. In line with the results on asylum seeker flows for Sub-Saharan Africa, these effects on regular migration flows are again driven by the sample of non-fragile states exclusively (Appendix Table 3.D.3).

Summing up, we present evidence suggesting that the effects of aid on migration preferences, described in Section 3.5.2, trigger corresponding changes in emigration from aid receiving countries, through regular migration channels and asylum seeker flows. These changes are consistent with the effects on attitudes and welfare outcomes discussed in Section 3.5.4. Our two main findings are: (i) Aid affects positively the attitudes and aspirations of people living in recipient areas, and this translates into short-term decreases in asylum seeker flows. (ii) Aid is effective in improving individual welfare in the longer run, which increases migration capabilities, and this results in increasing regular migration flows, consistent with the "mobility transition" theory.

Table 3.7 – Migration flows and World Bank disbursements

	(1) Regular migrants	(2) Regular migrants	(3) Regular migrants	(4) Asylum seekers	(5) Asylum seekers	(6) Asylum seekers
Panel A: Lag 1 year						
Disbursements	0.00022	0.00017	0.00017	-0.00061**	-0.00063**	-0.00062**
	(0.00020)	(0.00021)	(0.00021)	(0.00025)	(0.00025)	(0.00025)
Observations	16504	16405	16405	19072	18995	18995
F-stat	27.7	28.2	27.8	26.6	26.9	26.4
Panel B: Lag 2 years						
Disbursements	0.00040	0.00050**	0.00045*	-0.00057**	-0.00064**	-0.00063**
	(0.00026)	(0.00024)	(0.00023)	(0.00026)	(0.00030)	(0.00030)
Observations	16504	16405	16405	19072	18995	18995
F-stat	21.6	22.5	22.3	24.3	24.2	23.8
Panel C: Lag 3 years						
Disbursements	0.00046*	0.00059**	0.00053**	-0.00025	-0.00036	-0.00036
	(0.00027)	(0.00027)	(0.00025)	(0.00030)	(0.00034)	(0.00034)
Observations	16504	16405	16405	19072	18995	18995
F-stat	26.7	26.6	26.3	23.3	23.1	22.7
Panel D: Lag 4 years						
Disbursements	0.00045	0.00060**	0.00054**	0.00016	0.00008	0.00009
	(0.00028)	(0.00029)	(0.00027)	(0.00033)	(0.00037)	(0.00036)
Observations	16504	16405	16405	19072	18995	18995
F-stat	18.2	17.8	17.8	18.1	18.1	17.9
Panel E: Lag 5 years						
Disbursements	0.00037	0.00049*	0.00045*	0.00032	0.00030	0.00031
	(0.00025)	(0.00025)	(0.00023)	(0.00024)	(0.00025)	(0.00025)
Observations	16504	16405	16405	19072	18995	18995
F-stat	12.7	12.4	12.6	15.3	15.1	15.0
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Country-destination FE	.	✓	✓	.	✓	✓
Destination-year FE	.	.	✓	.	.	✓

Notes: This table presents the results from eq. 3.4. Columns 1-3 report the results using the number of regular migrants as outcome variable, columns 4-6 report results for the number of asylum seeker applications as outcome variable. "Disbursements" measures one year lagged, annual, aggregated World Bank financial amounts disbursed in million, constant 2014 US$, or further lags of this variable as indicated in panel titles. Individual controls include weighted means of the variables gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

3.6 Conclusions

Governments of high-income countries promote foreign aid as a tool to reduce irregular migration from developing countries. Our results show that foreign aid has an effect on migration and development outcomes in aid-receiving provinces and therefore can play a role in curbing irregular migration, but it is far from a panacea.

Our short-term results indicate that the mere announcement of a World Bank aid project significantly decreases migration preferences. We find similar effects for project disbursements, which also reduce asylum seeker flows to the OECD in the short run. This reduction seems related to enhanced optimism about the economic prospects in aid recipient provinces and improved confidence in national institutions.

In the longer run, aid projects increase incomes and alleviate poverty. However, the negative effect of aid on asylum seeker numbers fades out, and regular migration increases. That is consistent with the disappearance of positive short-run effects on migration aspirations and longer-run improvements in capabilities. There is thus no evidence in our study that targeting the "root causes" of migration through aid on average increases irregular migration or asylum seeker numbers. This decrease in irregular and increase in regular migration is consistent with the key objective of high-income country policymakers, who want to "manage" migration, and it should thus be counted as a sign of success of the "root causes" approach.

The effect of aid comes with considerable heterogeneity that needs to be considered when designing future policies. While we obtain similar results with both of our identification strategies, the event study and the instrumental variables approach, the causal effect is not homogeneous across space. In the short run, aid projects reduce migration preferences and asylum seeker flows to the OECD from Latin America, MENA, and non-fragile Sub-Saharan African countries. However, we do not find a significant effect in fragile countries of Sub-Saharan Africa, which are an important source of irregular migration to Europe. For policymakers, a key takeaway from our study is that aid projects do not keep people from migrating from the most hostile environments, but they can be effective in more stable environments.

It is essential to acknowledge that the World Bank projects we analyze are not designed to reduce migration, such as for example those implemented by the EU Trust Fund for Africa, and thus it is reasonable to interpret our results as conservative. Aid projects that are designed to target the "root causes" of migration specifically may have a more pronounced effect on migration outcomes. Still, when merely considering the dollars spent per asylum seeker or refused asylum seeker who do not arrive in a (high-income) host country, the price tag of this policy will remain high.

That somewhat cynical simplification aside, when judging alternatives to aid, the welfare consequences and cost effectiveness of the mechanisms that different policies target

must be considered. Generally, there are two ways of decreasing migration with a policy: reducing migration aspirations or reducing capabilities. The root causes approach attempts to reduce migration aspirations, making more people stay in their country of origin voluntarily because their welfare at home improves. By contrast, policies such as stricter border enforcement reduce migration by decreasing capabilities, which merely increases involuntary immobility. The latter type of policy thus does not improve welfare in origin countries but, if anything, reduces it. Aid reducing the "root causes" of migration thus offers a more ethical strategy than merely restricting mobility in general would.

The "root causes" strategy should not be seen in isolation, but as part of a broader policy toolbox. Donor governments frequently use aid as a political tool to buy policy concessions from source and transit countries of migrants (Dreher et al. 2019). These concessions may include more restrictive border controls for emigrants and more cooperation in repatriating rejected asylum seekers.

Instead of using aid as a tool to reduce migration in the short term and likely being disappointed by limited effects on asylum seeker numbers, donor governments should consider ways in which aid *and* migration can be combined effectively to improve the medium- and long-term development of origin countries. That should involve legal pathways for more student migration and labor migration of low- and medium-skilled people, which can benefit labor markets in destination countries and, through the transfers of money, skills, and values by migrants back to their origin countries, reduce root causes of migration.

<div align="right">

Appendices

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3.A   Data Appendix

**Figure 3.A.1** − Migration trends by world region, 2008–2019
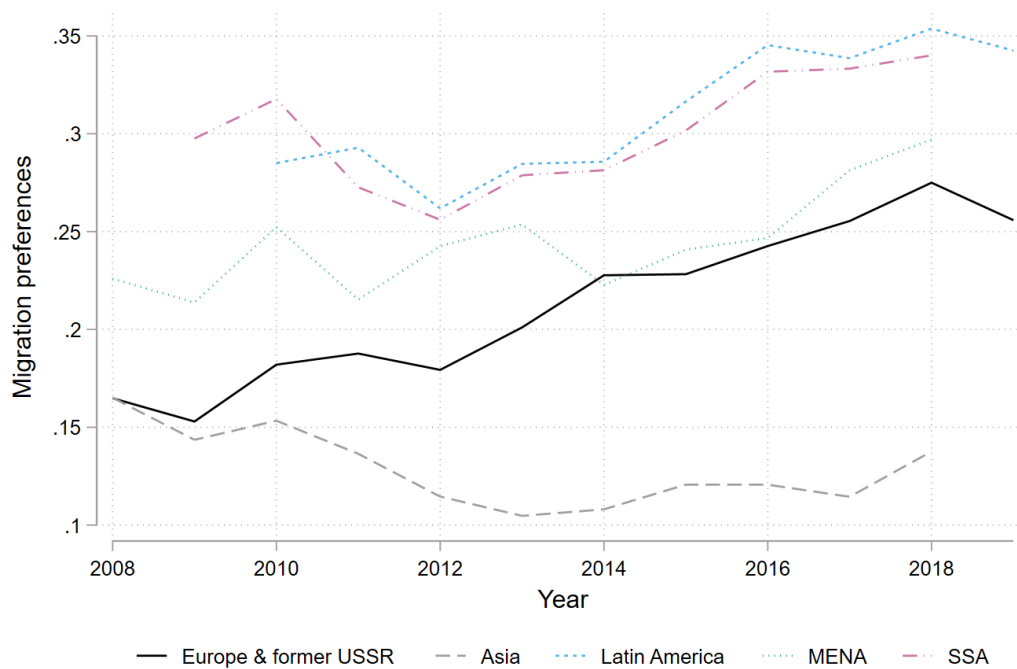
Notes: This figure shows the average migration preference by world region. From the individual-level data of the GWP, we first create weighted country-year means and then simple means by continent and year.

Figure 3.A.2 – World Bank disbursements by broad sector and recipient country, 2008–2019
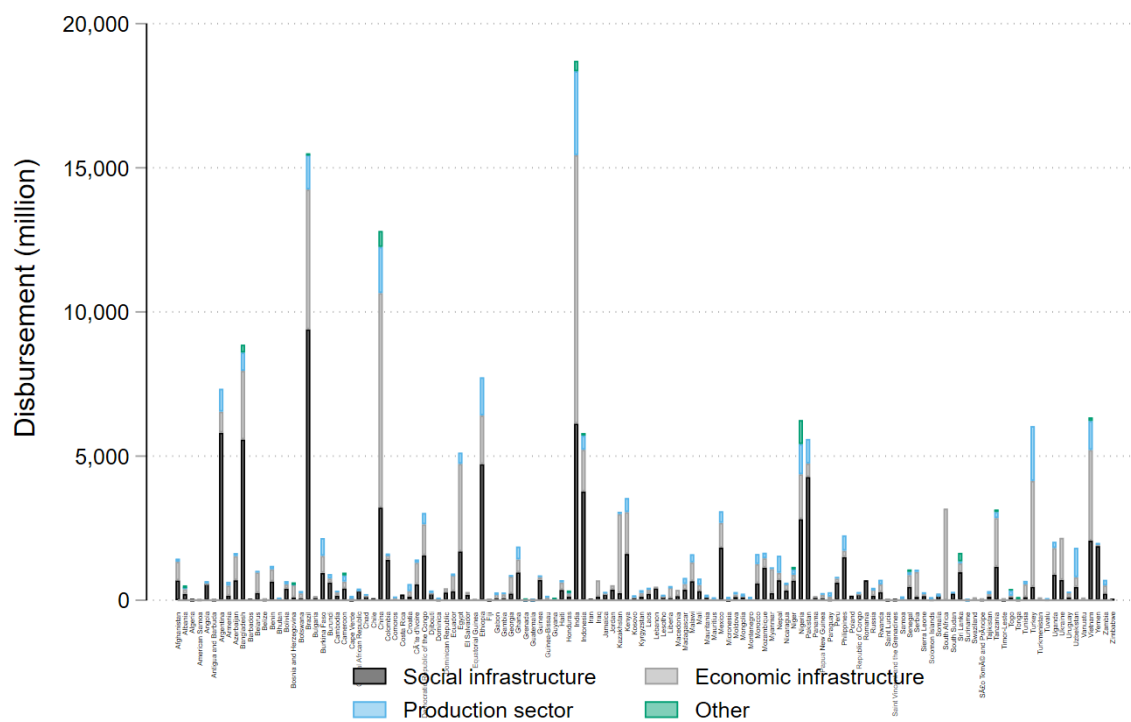
Notes: This figure shows the total amount in US$ million disbursed through World Bank projects between 2008 and 2019 by aid sector.

Figure 3.A.3 – World Bank project cycle
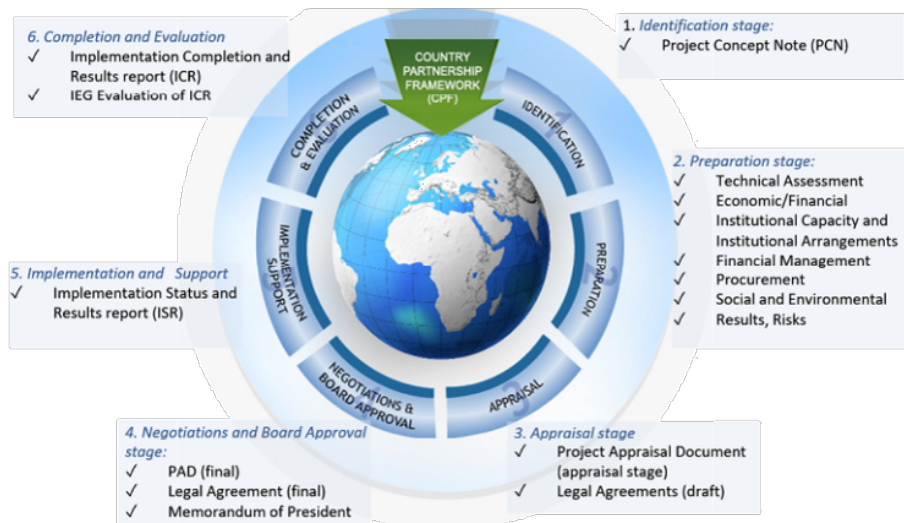
Notes: This figure presents an illustration of the typical the World Bank project cycle.[44] It provides an overview of the procedure from project initiation to completion and includes the project approval date in the project cycle.

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[44]This figure is published by the World Bank at https://www.worldbank.org/en/projects-operations/products-and-services/brief/projectcycle.

Figure 3.A.4 – Gallup World Poll interview date distribution by country in 2015
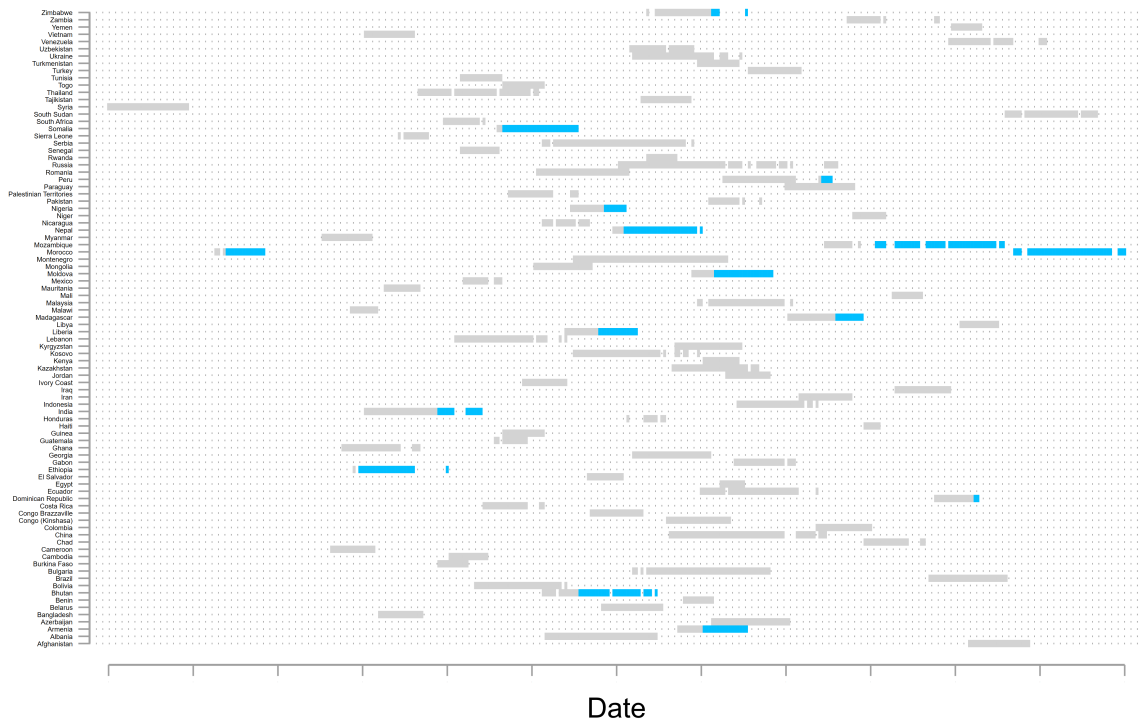
Date

Notes: This table presents the distribution of Gallup World Poll interview dates by country for our final sample in 2015. GWP interview dates are colored gray. GWP interview dates after project approval in the respective country are colored blue.

Figure 3.A.5 – Gallup World Poll interview date distribution
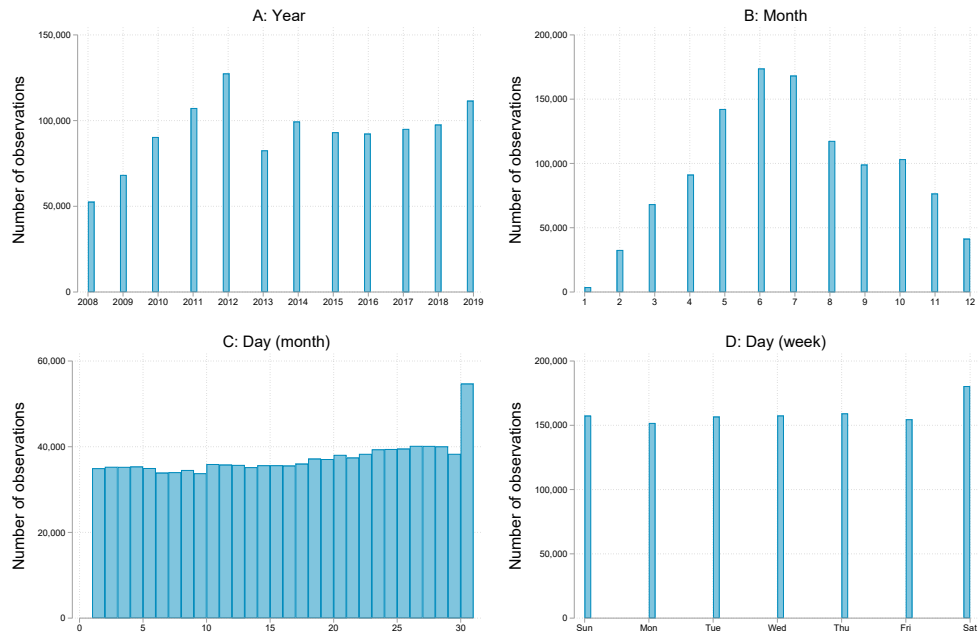
Notes: This table presents the distribution of Gallup World Poll interview dates by year (A), month (B), day of the month (C), and day of the week (D).

Figure 3.A.6 – World Bank project announcement date distribution
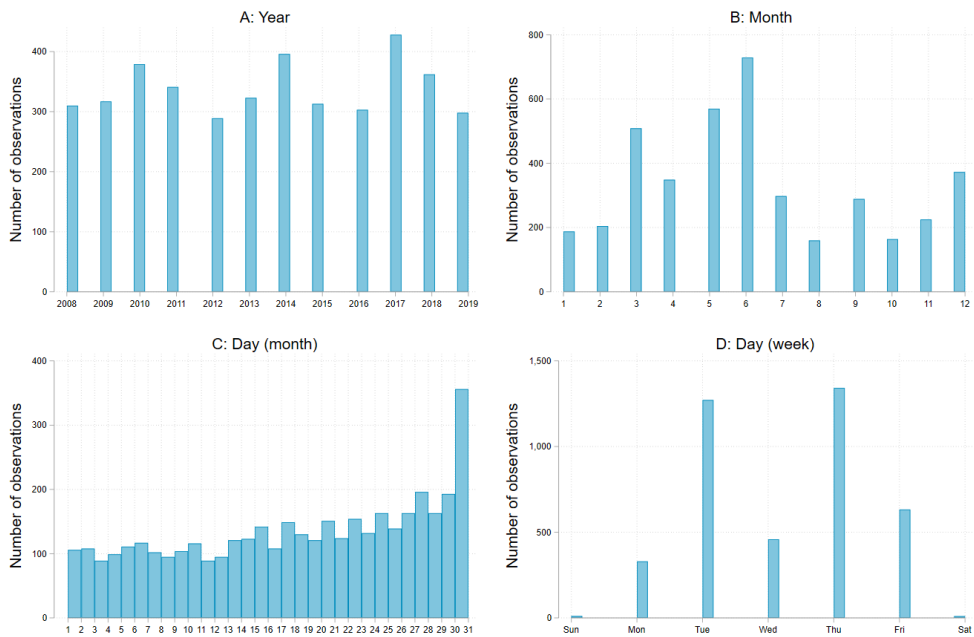
Notes: This table presents the distribution of World Bank board approval dates by year (A), month (B), day of the month (C), and day of the week (D).

Table 3.A.1 – List of countries

Afghanistan	Guatemala	Nigeria
Albania	Guinea	Pakistan
Algeria	Haiti	Palestinian Territories
Angola	Honduras	Paraguay
Armenia	India	Peru
Azerbaijan	Indonesia	Congo Brazzaville
Bangladesh	Iran	Romania
Belarus	Iraq	Russia
Belize	Jamaica	Rwanda
Benin	Jordan	Senegal
Bhutan	Kazakhstan	Serbia
Bolivia	Kenya	Sierra Leone
Brazil	Kosovo	Somaliland region
Bulgaria	Kyrgyzstan	Somalia
Burkina Faso	Laos	South Africa
Burundi	Lebanon	South Sudan
Cambodia	Lesotho	Sudan
Cameroon	Liberia	Suriname
Central African Republic	Libya	Eswatini
Chad	Madagascar	Syria
China	Malawi	Tajikistan
Colombia	Malaysia	Tanzania
Comoros	Mali	Thailand
Costa Rica	Mauritania	Togo
Ivory Coast	Mauritius	Tunisia
Congo (Kinshasa)	Mexico	Turkey
Djibouti	Moldova	Turkmenistan
Dominican Republic	Mongolia	Ukraine
Ecuador	Montenegro	Uzbekistan
Egypt	Morocco	Venezuela
El Salvador	Mozambique	Vietnam
Ethiopia	Myanmar	Yemen
Gabon	Namibia	Zambia
The Gambia	Nepal	Zimbabwe
Georgia	Nicaragua	
Ghana	Niger	

Notes: The table lists all 106 countries and territories included in our regression analysis.

Table 3.A.2 – Explaining migration flows with migration aspirations

	(1)	(2)	(3)	(4)	(5)	(6)
			Outcome: Number of OECD migrants			
	Preference	Preparation	Preference OECD	Preparation OECD	Preference OECD	Preparation OECD
Migration intention	0.72132***	3.36677**	0.60861*	4.33170**	1.37632***	2.15194***
	(0.26259)	(1.50077)	(0.35253)	(1.91672)	(0.02470)	(0.06424)
Observations	776	776	776	776	16504	16504
Mean DV	0.247	0.007	0.159	0.004	0.793	0.142
Country FE	✓	✓	✓	✓	✓	✓
Region-year FE	✓	✓	✓	✓	✓	✓

Notes: This table present results from a country level regression of migration aspirations on logged actual migration flows into OECD countries. In columns 1–4, we use country-year level data. In columns 5–6, we use the dyadic data following eq. 3.4. All specifications include weighted country-year means of the individual controls gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.A.3 – Migration preferences and World Bank projects, descriptive statistics

	Count	Mean	Sd	Min	Max
Migration preference	952713	0.232	0.422	0	1
Female	952713	0.534	0.499	0	1
Age	952713	38.492	16.656	13	99
Age squared	952713	1759.054	1499.377	169	9801
Education	952713	1.687	0.661	1	3
Urban	952713	0.374	0.484	0	1
Has a child	952713	0.629	0.483	0	1
Post	952713	0.175	0.380	0	1
Future better	853826	0.710	0.454	0	1
Relative future	853826	1.618	2.142	-10	10
Life in 5 years	858569	6.627	2.492	0	10
Life today	935791	4.970	2.304	0	10
Disbursements (1m)	18809	0.972	5.729	-12	465
Synthetic disbursements (1m)	18809	1.018	2.865	0	43
Disbursements (12m)	18809	11.621	35.882	-21	1127
Synthetic disbursements (12m)	18809	12.301	34.046	0	491
Poor	895349	0.072	0.259	0	1
Income p.c.	895349	7.099	1.834	0	10
Unemployed	910219	0.073	0.259	0	1
Public amenities	738805	0.576	0.304	0	1
National Institutions Index	803125	51.342	37.134	0	100
Economic Confidence Index	529223	-2.252	70.884	-100	100

Notes: This table presents the descriptive statistics for the final sample used to estimate announcement and disbursement effects.

Table 3.A.4 – World Bank project sector overview

Sector level I	Sector level II	Sector level III	Details
Social Infrastructure	Education	Education, level unspecified	General education sector, Public administration (Education)
Social Infrastructure	Education	Basic education	Adult literacy/non-formal education, Pre-primary and primary education
Social Infrastructure	Education	Secondary education	Secondary education, Vocational training
Social Infrastructure	Education	Post-secondary education	Tertiary education
Social Infrastructure	Health	Health	Health
Social Infrastructure	Health	Health, general	Compulsory health finance, Public administration (Health)
Social Infrastructure	Water	Water supply and sanitation	General water, sanitation and flood protection sector, Hydropower, Public administration (Water), Sewerage, Solid waste management, Wastewater Collection and Transportation, Wastewater Treatment and Disposal, Water supply
Social Infrastructure	Government	Government and civil society	Central government administration, General industry and trade sector, General public administration sector, Law and justice, Sub-national government administration
Economic Infrastructure	Other Social Infrastructure	Other social infrastructure	Compulsory pension and unemployment insurance, Other social services
Economic Infrastructure	Transport	Transport and storage	Aviation, General transportation sector, Ports, waterways and shipping, Public administration (Transportation), Railways, Roads and highways, Rural and Inter-Urban Roads and Highways, Urban Transport
Economic Infrastructure	Communications	Communications	General information and communications sector, Information technology, Media, Postal services, Public administration- Information and communications, Telecommunications
Economic Infrastructure	Energy	Energy generation and supply	Energy efficiency in Heat and Power, General energy sector, Other Renewable Energy, Power, Public administration- Energy and mining, Renewable energy, Thermal Power Generation, Transmission and Distribution of Electricity
Economic Infrastructure	Banking	Banking and financial services	Banking, Capital markets, Credit Reporting and Secured Transactions, General finance sector, Housing finance, Micro- and SME finance, Microfinance, Non-compulsory health finance, Non-compulsory pensions and insurance, Other non-bank financial intermediaries, Payments, settlements, and remittance systems, Public administration(Financial Sector), SME Finance
Production Sector	Agriculture, Forestry, Fishing	Agriculture, forestry, fishing	General agriculture, fishing and forestry sector
Production Sector	Agriculture, Forestry, Fishing	Agriculture	Agricultural extension and research, Animal production, Crops, Irrigation and drainage, Petrochemicals and fertilizers, Public administration- Agriculture, fishing and forestry
Production Sector	Agriculture, Forestry, Fishing	Forestry	Forestry
Production Sector	Industry and Mining	Industry	Agro-industry, Agro-industry, marketing, and trade, Other industry
Production Sector	Industry and Mining	Mineral resources and mining	Coal Mining, Mining and other extractive, Oil and gas, Other Mining and Extractive Industries
Production Sector	Trade	Trade policy and regulations	Other domestic and international trade, Public administration- Industry and trade
Other	Other	General environmental protection	Flood protection
Other	Other	Other	Housing construction
Other	Other	Unallocated/ unspecified	Unspecified

Notes: This table provides details on how AidData (2017) categorizes World Bank activities by sector. In this paper, we use the sector divisions under columns 1 and 2. Columns 3 and 4 serve to illustrate the type of project included in each sector category.

3.B   Announcement Effects

3.B.1   Robustness

Figure 3.B.1 – Migration preferences and World Bank announcements, Monte Carlo regressions
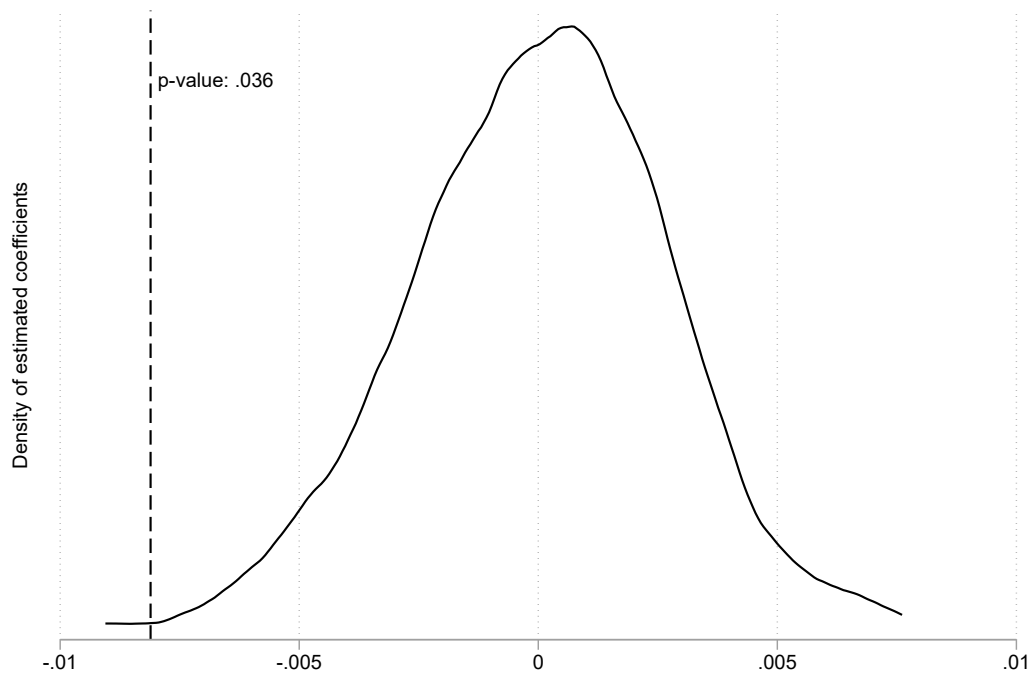
Notes: The figure shows the randomization inference test based on 999 Monte Carlo replications for the event study analysis. For this, we first draw an equal number of country-year waves (265) at random from the GWP, and then draw one random date from each of these windows that we use as placebo treatment. The original estimate from column 6 of Table 3.1 is shown by dashed vertical lines. The p-value of 3.6 percent is calculated as the proportion of times that the absolute value of the t-statistic in the simulated data exceed the absolute value of the original t-statistic.

Table 3.B.1 − World Bank announcements and media coverage

	(1)	(2)	(3)	(4)	(5)
	World Bank covered in recipient country media				
	2018–2020				
	Dummy	Dummy	Dummy	Dummy	Number
Board approval $_{t-1}$	0.06287***	0.03334***	0.02906***	0.02524**	0.02991**
	(0.01656)	(0.01021)	(0.00998)	(0.00977)	(0.01241)
Observations	313470	313470	313470	313470	313470
Year FE	✓	✓	✓	✓	✓
Country-year FE	.	✓	✓	✓	✓
Month FE	.	.	✓	✓	✓
Day-of-the-week FE	.	.	✓	✓	✓
Year-month FE	.	.	.	✓	✓
Mean DV	0.117	0.117	0.117	0.117	0.145

Notes: This table presents the the results from regressing World Bank media coverage in recipient countries on World Bank board approval dates. The analysis is undertaken on the country-date level. The outcome variable in columns 1–4 indicates weather or not on a given day, the World Bank is mentioned in the news of the recipient country. In column 5, we use the number of news articles reporting on the World Bank in the recipient country. The analysis is limited to the years 2018–2020 as news coverage data is only available for this time period. In columns 1–5, the treatment variable indicates whether or not a World Bank board approval occurred on a given day, lagged by one day. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.2 − Migration preferences and World Bank project announcement, balance test

	(1)	(2)	(3)	(4)	(5)
	Gender	Age	Edu	Urban	Has child
Post	-0.00630	0.14651	-0.01080	-0.05155***	-0.00590
	(0.00694)	(0.17088)	(0.00913)	(0.01695)	(0.00421)
Observations	952336	952336	952336	952336	952336
Individual controls	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓	✓
15-day-window	.	.	.	.	.
Mean DV	0.534	38.491	1.687	0.374	0.629

Notes: This table presents results from the balance test. Outcome variables are the individual level control variables as indicated in column titles. "Post" is a dummy if the respondent was interviewed after the board approval of a World Bank project that will be implemented in the respondent's country. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.3 – Migration preferences and World Bank announcements, alternative event windows

	(1) 5 days	(2) 10 days	(3) 15 days	(4) 20 days	(5) 30 days
Post	-0.00935*	-0.00918**	-0.00811**	-0.00861**	-0.00885**
	(0.00481)	(0.00392)	(0.00382)	(0.00368)	(0.00377)
Observations	85444	138305	172112	195501	224187
Individual controls	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓	✓
Event-window	✓	✓	✓	✓	✓
Mean DV	0.213	0.208	0.203	0.197	0.193

Notes: This table presents the results from eq. 3.1, where we chose different bandwidths relative to the project event, as indicated by column title. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.B.4 – Migration preferences and World Bank announcements, dropping event days

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Event day		Event day $+-1$ day		Event day + day after		Event day + day before	
Post country	-0.01258***	-0.01101**	-0.01986***	-0.01856***	-0.01420***	-0.01246***	-0.01788***	-0.01683***
	(0.00404)	(0.00417)	(0.00530)	(0.00548)	(0.00437)	(0.00456)	(0.00503)	(0.00503)
Observations	943796	163573	926746	146523	935598	155376	934945	154721
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
15-day-window	.	✓	.	✓	.	✓	.	✓
Mean DV	0.232	0.203	0.232	0.201	0.232	0.203	0.232	0.202

Notes: This table presents the results from eq. 3.1, where we leave out the event day (and surrounding days). In columns 1 and 2, we leave out the event day. In columns 3 and 4, we leave out the event day plus the day before and after the event. In columns 5 and 6, we leave out the event day plus the day after the event. In columns 7 and 8, we leave out the event day plus the day before the event. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.5 – Migration preferences and World Bank announcements, aggregates

	(1)	(2)	(3)	(4)
Post	-0.01337**	-0.01401**	-0.01475***	-0.01327**
	(0.00594)	(0.00576)	(0.00557)	(0.00628)
Observations	1060	1060	1060	438
Individual controls	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Year FE	.	✓	✓	✓
Month FE	.	.	✓	✓
Country-year FE	.	.	.	✓
Mean DV	0.247	0.247	0.247	0.229

Notes: This table presents the results from eq. 3.1, where we aggregate the individual level data on the country-year level for the pre and post treatment period. We weight individuals using probability weights before we aggregate. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.B.6 – Migration preferences and World Bank announcements, alternative outcome

	(1)	(2)	(3)	(4)	(5)
	Likelihood to move away from place of residence
	*2008–2019*
Post	-0.01433	-0.01526	-0.00959**	-0.00847*	-0.00722*
	(0.00982)	(0.00928)	(0.00407)	(0.00429)	(0.00412)
Observations	871406	871406	871406	871033	158421
Individual controls	.	✓	✓	✓	✓
Country-year FE	.	.	✓	✓	✓
Day-of-the-week FE	.	.	✓	✓	✓
Year-month FE	.	.	✓	✓	✓
Province-year-month FE	.	.	.	✓	✓
15-day-window	.	.	.	.	✓
Mean DV	0.232	0.232	0.232	0.232	0.205

Notes: This table presents the results from eq. 3.1, changing the outcome variable to whether or not individuals are likely to move their current place of residence based on the question "In the next 12 months, are you likely or unlikely to move away from the city or area where you live?" Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.7 – Migration preferences and World Bank announcements, capabilities

	(1)	(2)	(3)	(4)
	\multicolumn{2}{c}{Migration plan}	\multicolumn{2}{c}{Migration preparation}
	\multicolumn{2}{c}{*2010–2015*}	\multicolumn{2}{c}{*2010–2015*}
Post	-0.00480*	-0.00463	-0.00188	-0.00160
	(0.00289)	(0.00294)	(0.00194)	(0.00196)
Observations	566448	96751	566448	96751
Individual controls	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓
15-day-window	.	✓	.	✓
Mean DV	0.218	0.193	0.218	0.193

Notes: This table presents the results from eq. 3.1, changing the outcome variable. Outcome variable in columns 1 and 2 is a binary indicator if the respondent plans to migrate based on the question "Are you planning to move permanently to that country in the next 12 months, or not?" Outcome variable in columns 3 and 4 is a binary indicator if the respondent prepares to migrate based on the question "Have you done any preparation for this move?" Both variables are only available for the 2010–2015 period. Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.8 – Migration preferences and World Bank announcements, alternative cluster

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Province		Counry+Province		Country	Date	Country*Date	
Post	-0.00939**	-0.00811*	-0.00939**	-0.00811**	-0.00939**	-0.00811**	-0.00939**	-0.00811**
	(0.00417)	(0.00421)	(0.00365)	(0.00382)	(0.00367)	(0.00383)	(0.00371)	(0.00377)
Observations	952336	172112	952336	172112	952336	172112	952336	172112
Mean DV	0.232	0.203	0.232	0.203	0.232	0.203	0.232	0.203
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Country*year FE	✓	✓	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓	✓	✓	✓	✓
Country-year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
15-day-window	.	✓	.	✓	.	✓	.	✓

Notes: This table presents the results from eq. 3.1. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Treatment is a dummy indicating if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered as indicated in column title: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

3.B.2 Additional Results

Table 3.B.9 – Migration preferences and World Bank announcements, project provinces

	(1)	(2)	(3)	(4)	(5)
Post	-0.06915***	-0.06307***	-0.00728	-0.01094**	-0.00956**
	(0.02039)	(0.01663)	(0.00525)	(0.00429)	(0.00456)
Post*project province	0.04249*	0.02895	-0.00511	0.00490	0.00456
	(0.02316)	(0.01848)	(0.00781)	(0.00816)	(0.00863)
Observations	952713	952713	952713	952336	172112
Individual controls	.	✓	✓	✓	✓
Country-year FE	.	.	✓	✓	✓
Day-of-the-week FE	.	.	✓	✓	✓
Year-month FE	.	.	✓	✓	✓
Province-year-month FE	.	.	.	✓	✓
15-day-window	.	.	.	.	✓
Mean DV	0.232	0.232	0.232	0.232	0.203

Notes: This table presents the results from eq. 3.1, where we interact the treatment with an indicator for project provinces, that is, provinces where projects are implemented. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.10 – Migration preferences and World Bank announcements, project-level heterogeneities

	(1)	(2)	(3)	(4)
Post	-0.01119	-0.01524**	-0.00872**	-0.00669
	(0.00682)	(0.00718)	(0.00413)	(0.00428)
Post*IDA	0.00252	0.00691		
	(0.00780)	(0.00810)		
Post*commitment value			-0.00001	-0.00002
			(0.00002)	(0.00002)
Observations	952336	177251	322609	172112
Individual controls	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓
Day-of-the-week FE	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓
Province-year-month FE	✓	✓	✓	✓
15-day-window	.	✓	.	✓
Mean DV	0.232	0.204	0.197	0.203

Notes: This table presents the results from eq. 3.1, where we interact the treatment with an dummy indicating that a project is undertaken by the IDA (as compared to the IBRD) or the commitment value of the respective project. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" Treatment is a dummy if the respondent was interviewed after the World Bank project announcement. All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.B.11 – World Bank announcements, perceptions of the future, and migration preferences

	(1) Future better	(2) Relative future	(3) Life in the future
Panel A: first stage			
Post	0.01491***	0.06184***	0.03576*
	(0.00564)	(0.02283)	(0.01898)
Mean DV	0.710	1.618	6.623
Panel B: second stage			
Perception of future	-0.69927*	-0.16856*	-0.30728
	(0.37332)	(0.08885)	(0.19454)
Observations	853422	853422	853422
Individual controls	✓	✓	✓
Country-year FE	✓	✓	✓
Day-of-the-week FE	✓	✓	✓
Year-month FE	✓	✓	✓
Province-year-month FE	✓	✓	✓
15-day-window	.	.	.
Mean DV	0.236	0.236	0.236
F-stat	6.986	7.336	3.549

Notes: This table presents the results from an instrumental variable approach, where we instrument the three different measures for the perception of the future as indicated by column titles with the post dummy indicating if the respondent was interviewed after the World Bank project announcement as first stage regression in panel A. Column 3 controls for perception of life today. In panel B, we show the second stage regressions, regressing the perceptions of the future variables on a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" All specifications include the individual level controls gender, age, age^2, education, an urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

3.C Disbursement Effects

3.C.1 Robustness

Figure 3.C.1 – World Bank disbursements, disbursement shares by sector
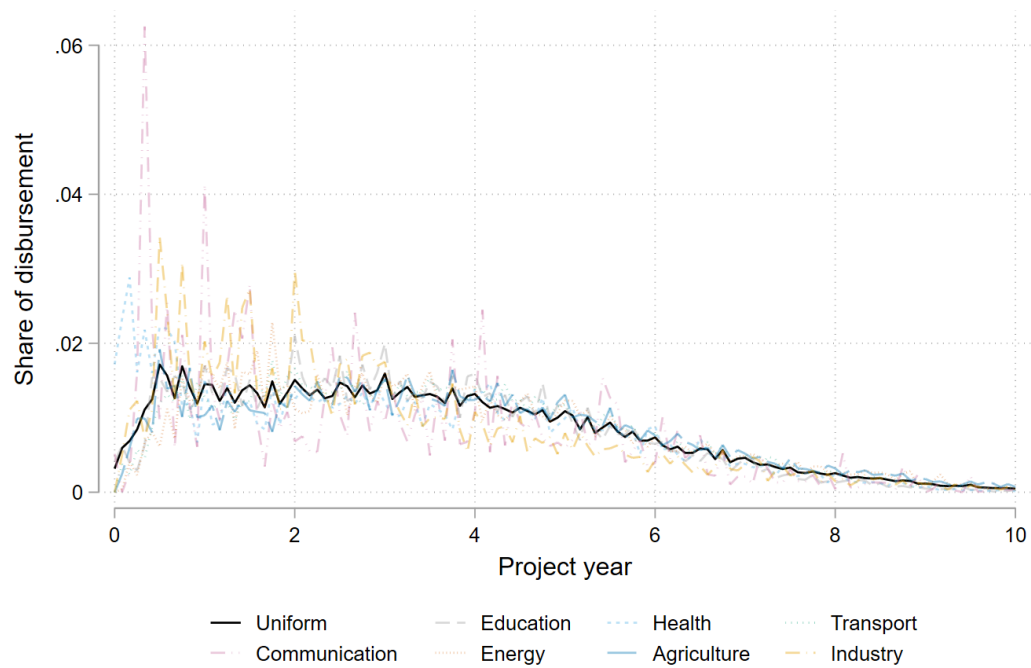
Notes: This table presents the share of project disbursements by project year over the lifetime of the project. "Uniform" presents the average disbursement share for all projects. The other lines present the average disbursement share by sector.

Figure 3.C.2 – Migration preferences and World Bank disbursements, augmented first stage

First stage, 1 month
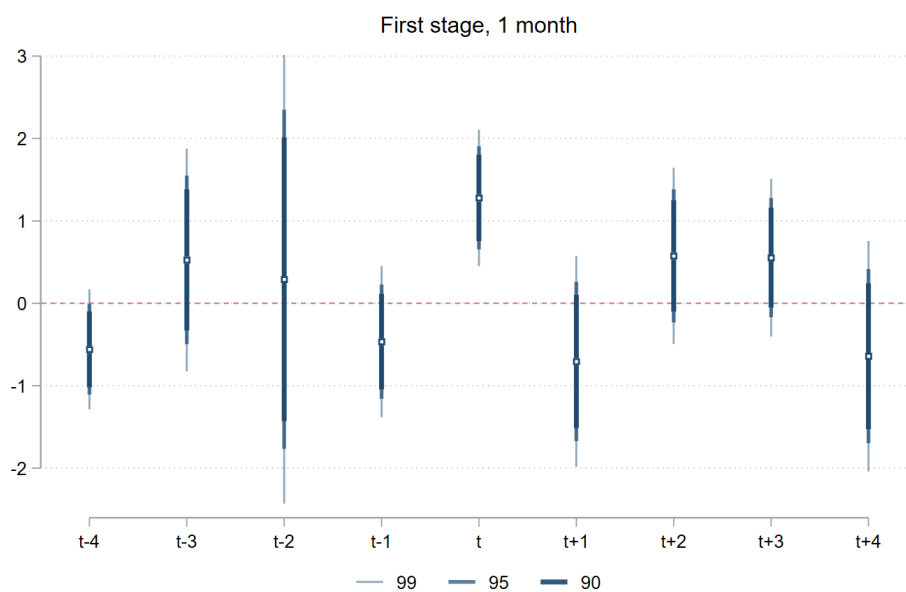
Notes: This table presents the results from the first stage from eq. 3.3, augmented with four leads and four lags of the synthetic disbursement variable.

Figure 3.C.3 – World Bank disbursements, augmented reduced form

Reduced form, 1 month
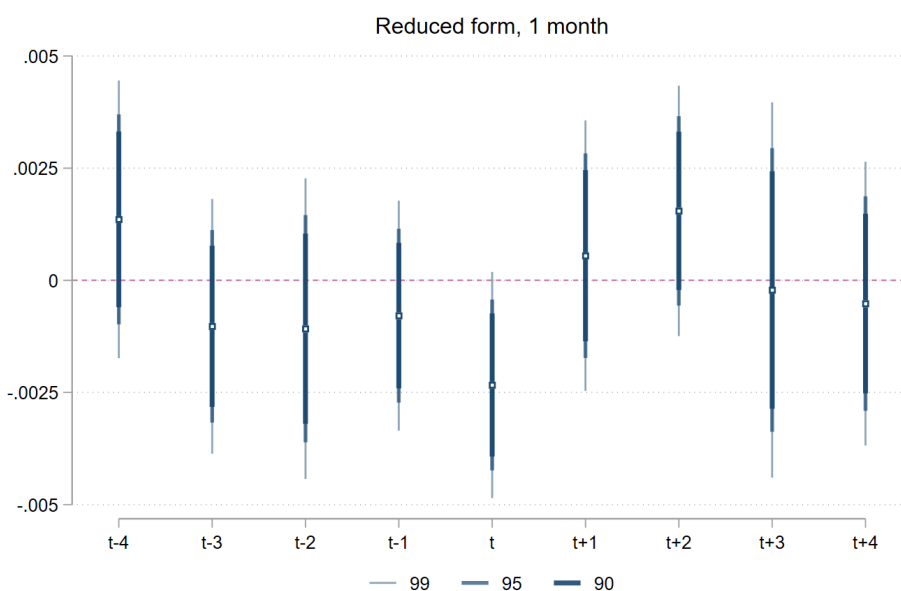
Notes: This table presents the results from the reduced form from eq. 3.2, augmented with four leads and four lags of the synthetic disbursement variable.

Figure 3.C.4 – Migration preferences and World Bank disbursements, Monte Carlo simulations
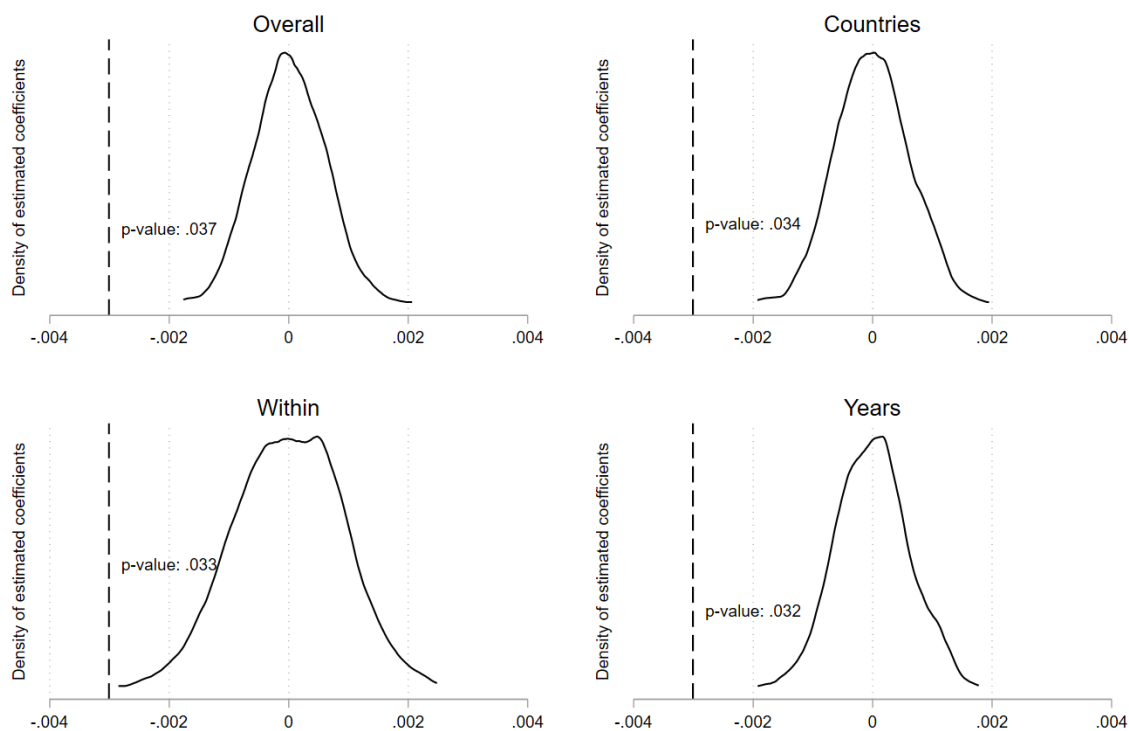
Notes: The figure shows the distribution of point coefficients of the disbursement of World Bank projects based on 999 Monte Carlo replications under different randomization inference tests. For this, we use province level aggregated data. The dotted line therefore depicts the coefficient estimated in Table 3.C.3 column 3 (0.00301). "Overall" swaps the number of projects completed and the instrument for all observations, "Countries" swaps the entire time series between countries, "Within" swaps years within countries, and "Years" swaps countries within years. The original estimate from column 1 of Table 3.4 is shown by dashed vertical lines. The p-values are calculated as the proportion of times that the absolute value of the t-statistics in the simulated data exceed the absolute value of the original t-statistic.

Table 3.C.1 – Migration preferences and World Bank disbursements, alternative synthetic disbursement schedules

	(1)	(2)	(3)	(4)	(5) Sector & region, leave-out project	(6) Sector & region, leave-out country
	All	Sector	Region	Sector & region		
Disbursements	-0.00341***	-0.00260***	-0.00362***	-0.00268***	-0.00262***	-0.00276***
	(0.00104)	(0.00093)	(0.00105)	(0.00073)	(0.00073)	(0.00080)
Observations	952713	952713	952713	952713	952713	952713
Individual controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Mean DV	0.232	0.232	0.232	0.232	0.232	0.232
F-stat	72.5	46.3	70.0	53.8	52.3	41.6

Notes: This table presents the results from eq. 3.2, where we change the definition of the synthetic disbursement schedules. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank disbursements in million, constant 2014 US\$. Synthetic disbursements are calculated based on disbursement schedules of all projects (column 1), projects in the same sector (column 2), projects in the same world region (column 3), projects in the same sector and world region (column 4). Column 5 repeats column 4, but we exclude the respective project when calculating the synthetic disbursement schedules following a leave-one-out logic. Similarly, column 6 calculates the synthetic disbursement schedules only based on projects from other countries. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.C.2 – Migration preferences and World Bank disbursements, alternative definition of aid

	(1) Million	(2) By population	(3) Log (by population)
Disbursements	-0.00268***	-0.01146*	-0.04136***
	(0.00073)	(0.00628)	(0.01430)
Observations	952713	952713	952713
Individual controls	✓	✓	✓
Year FE	✓	✓	✓
Country FE	✓	✓	✓
Country-year FE	✓	✓	✓
Province FE	✓	✓	✓
Mean DV	0.232	0.232	0.232
F-stat	53.8	11.0	140.7

Notes: This table presents the results from eq. 3.2, where we change the definition of the synthetic disbursement schedules. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank disbursements. Disbursements are measured in in million, constant 2014 US\$ in column 1, in million, constant 2014 US\$ divided by population in column 2, and in then in million, constant 2014 US\$, divided by population and logged in column 3. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.C.3 – Migration preferences and World Bank disbursements, alternative weights and aggregates

	(1)	(2)	(3) Aggregates (weighted)	(4) Aggregates (unweighted)
	Baseline	No weights		
Disbursements	-0.00268***	-0.00263***	-0.00336*	-0.00276
	(0.000733)	(0.000604)	(0.00193)	(0.00172)
Observations	952,713	952,713	13,011	13,011
R-squared	0.045	0.045	0.046	0.048
Individual controls	✓	✓	✓	✓
Country*year FE	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
F-stat	53.79	66.38	10.13	10.12

Notes: This table presents the results from eq. 3.2, where we change weights and use aggregates instead of using the individual level data. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank disbursements in million, constant 2014 US$. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Column 1 presents our baseline. Column 2 presents the baseline without probability weights. Columns 3 and 4 present results when aggregating data on the province-year level, using probability weights to create means in column 3, and no weights to create means in column 4. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.C.4 – Migration preferences and World Bank disbursements, alternative cluster

	(1) Country	(2) Country × year	(3) Province	(4) Province × year	(5) Date
Disbursements	-0.00268***	-0.00268***	-0.00268***	-0.00268***	-0.00268***
	(0.00073)	(0.00060)	(0.00081)	(0.00075)	(0.00040)
Observations	952713	952713	952713	952713	952713
Individual controls	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓
Mean DV	0.232	0.232	0.232	0.232	0.232
F-stat	53.8	34.0	21.4	21.2	133.2

Notes: This table presents the results from eq. 3.2, where we change the level standard errors are clustered on. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank disbursements in million, constant 2014 US\$. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered as indicated in column titles: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

3.C.2 Additional Results

Table 3.C.5 – Migration preferences and World Bank disbursements, heterogeneous effects

	(1) Female	(2) Under 30	(3) High skilled	(4) Urban	(5) Has child	(6) Future better
Panel A: months 0–1						
Disbursement	-0.00262***	-0.00084	-0.00170**	-0.00286***	-0.00246***	-0.00193**
	(0.00093)	(0.00108)	(0.00082)	(0.00098)	(0.00078)	(0.00091)
Characteristic	-0.04427***	0.01362***	0.02689***	0.02767***	-0.00039	0.00470
	(0.00506)	(0.00503)	(0.00657)	(0.00312)	(0.00245)	(0.00327)
Interaction	-0.00012	-0.00485***	-0.00197*	0.00041	-0.00037	-0.00081*
	(0.00093)	(0.00149)	(0.00116)	(0.00086)	(0.00034)	(0.00047)
Observations	952713	952713	952713	952713	952713	853826
Mean of characteristic	0.534	0.364	0.576	0.374	0.629	0.710
F-stat	27.0	26.9	27.1	27.2	27.4	25.4
Panel B: months 0–12						
Disbursements	-0.00030***	-0.00014	-0.00020**	-0.00029***	-0.00027***	-0.00024***
	(0.00007)	(0.00010)	(0.00009)	(0.00008)	(0.00007)	(0.00009)
Characteristic	-0.04447***	0.01399***	0.02746***	0.02817***	-0.00034	0.00464
	(0.00509)	(0.00514)	(0.00662)	(0.00313)	(0.00245)	(0.00327)
Interaction	0.00001	-0.00043***	-0.00019*	0.00000	-0.00004	-0.00006
	(0.00008)	(0.00012)	(0.00010)	(0.00007)	(0.00003)	(0.00004)
Observations	952713	952713	952713	952713	952713	853826
Mean of characteristic	0.534	0.364	0.576	0.374	0.629	0.710
F-stat	64.8	64.8	64.3	65.4	64.9	60.9

Notes: This table presents the results from eq. 3.2, where we interact the independent variable and the instrument with individual level characteristics as indicated in column titles. "Female" indicates whether or not the respondent is female. "Under 30" indicates whether or not the respondent is under the age of 30. "High skilled" indicates whether or not the respondent has an education of more than 8 years. "Urban" indicates whether or not the respondent lives in an urban area. "Has child" indicates whether or not the respondent has a child. "Future better" indicates whether life in the future is expected to be better than life today. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank financial amounts disbursed in the months indicated prior to the interview month in million, constant 2014 US$. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.C.6 – Migration preferences and World Bank disbursements by world region and sector

	(1) Europe & Former USSR	(2) Asia	(3) Latin America	(4) Middle East & North Africa	(5) Sub-Saharan Africa
Panel A: All sectors					
Disbursements	-0.00937***	-0.00114	-0.00375***	-0.00373**	-0.00350
	(0.00108)	(0.00074)	(0.00037)	(0.00131)	(0.00309)
Observations	183323	209935	139686	147471	272298
F-stat	54.9	15.4	199.9	285.0	47.8
Panel B: Social infrastructure					
Sector Disbursements	-0.01878	-0.00208	-0.00207***	0.00071	-0.02080***
	(0.01485)	(0.00154)	(0.00035)	(0.01379)	(0.00533)
Observations	183323	209935	139686	147471	272298
F-stat	2.6	8.3	93.7	52.2	35.4
Panel C: Economic infrastructure					
Sector Disbursements	-0.00844***	-0.00140	0.05499**	-0.00570*	-0.00122
	(0.00127)	(0.00159)	(0.02571)	(0.00289)	(0.00187)
Observations	183323	209935	139686	147471	272298
F-stat	43.6	19.9	4.2	14.6	30.0
Panel D: Production sector					
Sector Disbursements	-0.02868**	-0.00338***	-0.03443	-0.01645***	-0.01778
	(0.01322)	(0.00047)	(0.09577)	(0.00048)	(0.03911)
Observations	183323	209935	139686	147471	272298
F-stat	11.7	26.5	5.5	21067.1	23.1
Individual controls	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Country-year FE	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓
Mean DV	0.204	0.104	0.288	0.243	0.314

Notes: This table presents the results from eq. 3.2, where we split the sample by world region and show the results by project sector. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" In panel A, "Disbursements" measures aggregated World Bank financial amounts disbursed in the 12 months prior to the interview month in million, constant 2014 US\$. Panel B subsets disbursements to all disbursements in the sector social infrastructure. Panel C subsets disbursements to all disbursements in the sector economic infrastructure. Panel D subsets disbursements to all disbursements in the sector production sector. We also subset disbursements accordingly when creating the synthetic disbursement schedules. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.C.7 – Migration preferences and World Bank disbursements by income group and sector

	(1) Low income	(2) Lower middle income	(3) Upper middle income
Panel A: All sectors			
Disbursements	-0.00540**	-0.00141*	-0.00374***
	(0.00251)	(0.00071)	(0.00096)
Observations	222680	356372	373661
F-stat	67.0	12.3	101.0
Panel B: Social infrastructure			
Sector Disbursements	-0.00535	-0.00290	-0.00265***
	(0.01119)	(0.00234)	(0.00085)
Observations	222680	356372	373661
F-stat	30.2	6.9	35.2
Panel C: Economic infrastructure			
Sector Disbursements	-0.00780*	-0.00242**	-0.00564
	(0.00393)	(0.00093)	(0.00557)
Observations	222680	356372	373661
F-stat	17.7	14.3	3.3
Panel D: Production sector			
Sector Disbursements	-0.00336***	-0.00413***	-0.01680***
	(0.00115)	(0.00103)	(0.00215)
Observations	222680	356372	373661
F-stat	580.7	15.0	605.6
Individual controls	✓	✓	✓
Year FE	✓	✓	✓
Country FE	✓	✓	✓
Country-year FE	✓	✓	✓
Province FE	✓	✓	✓
Mean DV	0.288	0.224	0.205

Notes: This table presents the results from eq. 3.2, where we split the sample by World Bank income group and show the results by project sector. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" In panel A, "Disbursements" measures aggregated World Bank financial amounts disbursed in the 12 months prior to the interview month in million, constant 2014 US\$. Panel B subsets disbursements to all disbursements in the sector social infrastructure. Panel C subsets disbursements to all disbursements in the sector economic infrastructure. Panel D subsets disbursements to all disbursements in the sector production sector. We also subset disbursements accordingly when creating the synthetic disbursement schedules. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.C.8 − Migration preference and World Bank disbursements by project sector

	(1) Social Infrastructure (SI)	(2) Economic Infrastructure (EI)	(3) Production Sector (PS)	(4) Education (SI 1)	(5) Health (SI 2)	(6) Water (SI 3)	(7) Government (SI 5)	(8) Transport (EI 1)	(9) Energy (EI 2)	(10) Agriculture, Forest, Fishing (PS 1)	(11) Industry & Mining (PS 2)
Panel A: Month 0-1											
Sector Disbursements	-0.00277***	-0.00379*	-0.00689***	-0.00307	-0.01764	-0.00665**	-0.00183***	-0.00547	-0.00308	-0.00490***	-0.00283**
	(0.00089)	(0.00200)	(0.00262)	(0.00446)	(0.01528)	(0.00326)	(0.00059)	(0.00374)	(0.00267)	(0.00152)	(0.00117)
Observations	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713
F-stat	10.0	9.1	14.6	28.2	6.4	26.3	61.6	4.2	36.9	40.0	11.9
Panel B: Month 0-12											
Sector Disbursements	-0.00039***	-0.00032**	-0.00062***	-0.00040	-0.00069	-0.00052	-0.00034***	-0.00037*	-0.00030	-0.00037**	-0.00069**
	(0.00009)	(0.00013)	(0.00015)	(0.00074)	(0.00092)	(0.00039)	(0.00010)	(0.00020)	(0.00018)	(0.00018)	(0.00031)
Observations	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713
F-stat	25.8	23.7	28.9	34.9	30.9	9.9	182.6	10.4	94.4	92.5	80.4
Panel C: Month 13-24											
Disbursements	-0.00150***	-0.00054**	-0.00268	-0.02338	-0.01042*	-0.01410	-0.00235***	-0.00075**	-0.00280	-0.01825	-0.00984
	(0.00049)	(0.00021)	(0.00166)	(0.01613)	(0.00593)	(0.03293)	(0.00075)	(0.00035)	(0.00178)	(0.02424)	(0.00821)
Observations	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713
F-stat	11.5	25.8	2.0	2.6	6.9	0.2	7.8	12.3	2.9	0.6	1.5
Panel D: Month 25-36											
Disbursements	-0.00136	-0.00031*	-0.00340	-0.00423	-0.00794	-0.01014	-0.00302	-0.00041*	-0.00166*	-0.07290	-0.02307
	(0.00093)	(0.00017)	(0.00308)	(0.00417)	(0.00548)	(0.02472)	(0.00227)	(0.00025)	(0.00098)	(0.78231)	(0.02865)
Observations	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713
F-stat	5.6	48.9	0.8	1.5	5.0	0.2	3.3	23.0	3.6	0.0	0.8
Panel E: Month 37-48											
Disbursements	-0.00055	-0.00008	-0.00128	-0.00490	-0.00321	-0.00397	-0.00087	-0.00010	-0.00045	0.00379	-0.00808
	(0.00070)	(0.00012)	(0.00157)	(0.00802)	(0.00482)	(0.01069)	(0.00114)	(0.00016)	(0.00054)	(0.01343)	(0.01519)
Observations	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713
F-stat	8.6	22.1	0.4	2.9	7.1	0.1	7.3	13.0	3.5	0.2	2.9
Panel F: Month 49-60											
Disbursements	0.00015	0.00003	0.00048	0.00080	0.00094	0.00052	0.00042	0.00004	0.00015	-0.00088	0.00235
	(0.00058)	(0.00010)	(0.00223)	(0.00293)	(0.00345)	(0.00197)	(0.00161)	(0.00012)	(0.00060)	(0.00278)	(0.00785)
Observations	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713	952713
F-stat	9.6	12.8	0.3	4.8	4.5	1.8	3.1	8.4	3.9	0.4	9.3

Notes: This table presents the results from eq. 3.2, where we look at subsets of aid projects by sector. Columns 1–3 report results for the three main sectors social infrastructure (SI), economic infrastructure (EI), and production sector (PS). Columns 4–10 report results for the main sub-sectors of the three main sectors: Education, health, governance (all SI), transport, energy (all EI), and agriculture, forestry, and fishing, and industry and mining (all PS). We also subset disbursements accordingly when creating the synthetic disbursement schedules. Outcome variable is a binary indicator if the respondent prefers to migrate based on the question "Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?" "Disbursements" measures aggregated World Bank financial amounts disbursed in the respective sector in the month(s) prior to the interview month as indicated in panel titles in million, constant 2014 US\$. Individual controls include gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

3.D   Migration Flows

Table 3.D.1 – Migration flows and World Bank project disbursements by world region

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Europe & Former USSR		Asia		Latin America		Middle East & North Africa		Sub-Saharan Africa	
	Migration flows 2008–2019	Asylum seeker 2008–2019	Migration flows 2008–2019	Asylum seeker 2008–2019	Migration flows 2008–2019	Asylum seeker 2008–2019	Migration flows 2008–2019	Asylum seeker 2008–2019	Migration flows 2008–2019	Asylum seeker 2008–2019
Panel A: Lag 1										
Disbursements	0.00071	-0.00056	-0.00009	-0.00041	-0.00010	-0.00076***	0.00017	0.00003	-0.00032	-0.00032
	(0.00071)	(0.00081)	(0.00070)	(0.00086)	(0.00006)	(0.00025)	(0.00018)	(0.00020)	(0.00045)	(0.00043)
F-stat	6.8	6.7	6.1	5.3	189.8	280.1	30.5	28.4	47.4	44.9
Panel B: Lag 2										
Disbursements	0.00084	-0.00081	0.00044	0.00006	0.00025	-0.00072	-0.00013	-0.00075**	-0.00001	-0.00003
	(0.00067)	(0.00110)	(0.00082)	(0.00053)	(0.00024)	(0.00044)	(0.00025)	(0.00031)	(0.00035)	(0.00022)
F-stat	6.5	5.7	4.6	4.4	33.8	34.6	33.2	32.5	39.6	33.6
Panel C: Lag 3										
Disbursements	0.00072	-0.00023	0.00105	0.00108	0.00036*	-0.00010	-0.00035	-0.00104**	0.00056***	0.00026
	(0.00042)	(0.00088)	(0.00099)	(0.00075)	(0.00017)	(0.00039)	(0.00033)	(0.00036)	(0.00018)	(0.00032)
F-stat	13.2	13.0	5.0	4.7	220.1	111.1	51.3	53.7	67.1	44.4
Panel D: Lag 4										
Disbursements	0.00042**	0.00023	0.00180	0.00222	0.00036**	0.00033	-0.00046	-0.00110*	0.00071***	0.00069**
	(0.00019)	(0.00084)	(0.00173)	(0.00209)	(0.00015)	(0.00025)	(0.00037)	(0.00052)	(0.00014)	(0.00027)
F-stat	22.3	19.8	1.5	1.5	366.6	217.6	37.5	37.4	41.7	58.9
Panel E: Lag 5										
Disbursements	0.00033	0.00079	0.00176	0.00227	0.00022**	0.00025	-0.00063	-0.00125	0.00069***	0.00084***
	(0.00020)	(0.00090)	(0.00159)	(0.00177)	(0.00009)	(0.00014)	(0.00041)	(0.00071)	(0.00020)	(0.00024)
F-stat	19.1	16.6	1.9	2.6	2453.2	1839.1	28.2	26.7	31.7	24.9

Notes: This table presents the results from eq. 3.4, where we split the sample by world region, as indicated in column titles. "Disbursements" measures one year lagged, annual, aggregated World Bank financial amounts disbursed in million, constant 2014 US$, or further lags of this variable as indicated in panel titles. Individual controls include weighted means of the variables gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.D.2 – Migration flows World Bank project disbursements, by state fragility status, Sub-Saharan Africa

	(1)	(2)	(3)	(4)
	Non-Fragile		Fragile	
	Migration flows *2008–2019*	Asylum seeker *2008–2019*	Migration flows *2008–2019*	Asylum seeker *2008–2019*
Panel A: Lag 1				
Disbursements	-0.00060***	-0.00046	0.00001	-0.00014
	(0.00019)	(0.00028)	(0.00052)	(0.00049)
F-stat	70.4	80.7	12.2	8.6
Panel B: Lag 2				
Disbursements	-0.00020	0.00015	0.00011	-0.00024
	(0.00014)	(0.00023)	(0.00049)	(0.00046)
F-stat	59.7	90.8	83.4	34.3
Panel C: Lag 3				
Disbursements	0.00065***	0.00069***	0.00025	-0.00025
	(0.00009)	(0.00023)	(0.00043)	(0.00034)
F-stat	71.3	56.8	239.6	515.7
Panel D: Lag 4				
Disbursements	0.00091***	0.00107***	0.00049**	0.00036
	(0.00012)	(0.00018)	(0.00023)	(0.00031)
F-stat	264.7	342.3	90.6	94.9
Panel E: Lag 5				
Disbursements	0.00110***	0.00094***	0.00034	0.00058
	(0.00015)	(0.00018)	(0.00024)	(0.00047)
F-stat	235.8	194.2	232.7	357.7

Notes: This table presents the results from eq. 3.4, where we split the sample by state fragility. We subset the sample to Sub-Saharan Africa. Columns 1–2 report results for non-fragile states, columns 3–4 report results for fragile states. "Disbursements" measures one year lagged, annual, aggregated World Bank financial amounts disbursed in million, constant 2014 US\$, or further lags of this variable as indicated in panel titles. Individual controls include weighted means of the variables gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.D.3 – Migration flows and World Bank project disbursements, by state fragility status

	(1)	(2)	(3)	(4)
	Non-Fragile		Fragile	
	Migration flows *2008–2019*	Asylum seeker *2008–2019*	Migration flows *2008–2019*	Asylum seeker *2008–2019*
Panel A: Lag 1				
Disbursements	0.00029	-0.00052**	-0.00079	-0.00098
	(0.00021)	(0.00026)	(0.00073)	(0.00085)
F-stat	26.6	24.8	6.3	6.3
Panel B: Lag 2				
Disbursements	0.00070***	-0.00043	-0.00064	-0.00104
	(0.00024)	(0.00032)	(0.00065)	(0.00073)
F-stat	17.8	19.0	12.0	11.5
Panel C: Lag 3				
Disbursements	0.00073***	-0.00010	-0.00061	-0.00138
	(0.00027)	(0.00038)	(0.00073)	(0.00095)
F-stat	25.7	21.2	11.9	12.4
Panel D: Lag 4				
Disbursements	0.00070**	0.00031	-0.00018	-0.00070
	(0.00031)	(0.00040)	(0.00044)	(0.00063)
F-stat	16.4	16.1	46.3	46.0
Panel E: Lag 5				
Disbursements	0.00052*	0.00039	-0.00006	0.00001
	(0.00027)	(0.00025)	(0.00040)	(0.00055)
F-stat	11.4	13.4	80.8	81.9

Notes: This table presents the results from eq. 3.4, where we split the sample by state fragility. Columns 1–2 report results for non-fragile states, columns 3–4 report results for fragile states. "Disbursements" measures one year lagged, annual, aggregated World Bank financial amounts disbursed in million, constant 2014 US\$, or further lags of this variable as indicated in panel titles. Individual controls include weighted means of the variables gender, age, age^2, education, urban dummy, and whether or not the household has a child. Standard errors clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1.

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229

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4

Author Contributions

Chapter 1: Can Aid Buy Foreign Public Support?

This chapter is co-authored with Axel Dreher, Andreas Fuchs, Austin Strange, and Bradley Parks. I am first author of this paper. We equally contributed to the conceptualization. Austin Strange and I undertook the data collection. I undertook all data preparation and analysis for this paper. All coauthors contributed to the writing, with Axel Dreher taking the lead.

Chapter 2: Bureaucrats, Ideological Biases, and the Policies of International Organizations

This chapter is co-authored with Valentin Lang and Alexandros Kentikelenis. All of us contributed to the conceptualization, with Valentin Lang contributing most. We undertook the data collection together. Most of the data preparation and analysis were carried out by myself. All of us contributed to the writing, with Valentin Lang and Alexandros Kentikelenis taking the lead.

Chapter 3:The Effect of Foreign Aid on Migration and Development

This chapter is co-authored with Andreas Fuchs, André Gröger, and Tobias Heidland. All of us contributed to the conceptualization. Most of the data preparation and analysis were carried out by myself. We equally contributed to the writing.

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5
Abstracts

The Impact and Implementation of International Development Finance

This dissertation examines the pivotal role of international development finance amid the halfway point of the 2030 Agenda and the Sustainable Development Goals. Against the backdrop of formidable challenges, including the Covid-19 pandemic, increasing geopolitical tensions, and climate change, this dissertation investigates how diverse actors strategically utilize international development finance to achieve their objectives. With a specific focus on China's development finance and its impact on global perceptions, the first chapter scrutinizes the instrument's efficacy as a soft power tool. The second chapter delves into the influence of political ideologies within international organizations, shedding light on the role of individual ideas in shaping global governance. The third chapter investigates the impact of international development finance from institutions like the World Bank on international migration and livelihoods, contributing nuanced insights to the ongoing debate on aid effectiveness and how to reduce migration pressures in the developed world. Based on new, fine-grained data, and robust identification approaches, this dissertation offers a comprehensive analysis, aiming to inform future policies addressing current and future global economic, social, and political challenges.

Die Auswirkungen und Ausführung Internationaler Entwicklungsfinanzierung

Diese Dissertation untersucht die Rolle von internationalen Entwicklungsfinanzierung anlässlich des Halbzeitpunkts der Agenda 2030 und der Ziele für nachhaltige Entwicklung. Vor dem Hintergrund erheblicher Herausforderungen, darunter die Covid-19-Pandemie, zunehmende geopolitische Spannungen und der Klimawandel, untersucht diese Dissertation, wie diverse Akteure die internationale Entwicklungsfinanzierung strategisch nutzen, um ihre Ziele zu erreichen. Mit einem speziellen Fokus auf die Entwicklungsförderung Chinas und deren Auswirkungen auf die globale Wahrnehmung analysiert das erste Kapitel die Wirksamkeit dieses Instruments als Soft-Power-Werkzeug. Das zweite Kapitel geht auf den Einfluss politischer Ideologien innerhalb internationaler Organisationen ein und beleuchtet die Rolle individueller Ideen bei der Gestaltung der globalen Governance. Das dritte Kapitel untersucht die Auswirkungen der internationalen Entwicklungsfinanzierung von Institutionen wie der Weltbank auf die internationale Migration und Lebensbedingungen in Entwicklungsländern. Es liefert differenzierte Einblicke in die laufende Debatte über die Wirksamkeit von Entwicklungshilfe und die Reduzierung von Migrationsdruck in der entwickelten Welt. Basierend auf neuen, feingliedrigen Daten und robusten Identifikationsstrategien bietet diese Dissertation eine umfassende Analyse mit dem Ziel, zukünftige Politikmaßnahmen zur Bewältigung aktueller und zukünftiger globaler wirtschaftlicher, sozialer und politischer Herausforderungen zu informieren.