Geography, Economics, and Power: Global Assessments of Development with Geo-Referenced Data

Dissertation

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To Coximo and Fegli.
Of Ecuador, by Latin-America, for the Species.

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Introduction

Geography, Economics, and Power: Post-modern globalization is an intricate phenomenon. It not only comprises several dimensions of human sociability, namely economic, political, and cultural interactions, but also influences and is influenced by such dimensions (Dreher, 2006; Tonkiss, 2006). The tangible epiphenomena of globalization are then, unsurprisingly, multiple—e.g., trade agreements, diplomatic arrangements, and the exchange of cultural traditions (Urata, 2002; L'Etang, 2009; Jensen et al., 2011). Each epiphenomena, moreover, has implications on international, national, and local narratives (Sassen, 1999). While, for instance, processes of trade liberalization can foster growth at national levels (Vamvakidis, 1999), for some subnational regions such liberalization might carry more harm than benefit (Otero, 2011; Salamanca et al., 2009). Similarly, the literature poses other potential consequences on the political and cultural arena as globalization may systematically ostracize or benefit particular countries and regions (see Holton, 2000 and Appadurai, 1990).

Overall, the specialized literature has often had an ambivalent tone. Depending on the particular social dimension of globalization studied, the scope of countries under examination, or the level of aggregation analyzed, one might find works aggrandizing globalization's benefits or its shortcomings. For example, processes of cultural integration have shown to be beneficial for particular entertainment industries of the Global North, yet, at the same time, such integration has most likely displaced similar ventures in the Global South (Mirrlees, 2013). While the standardization of global, intellectual property protection has benefited trademark security, some argue that it has systematically benefited the pharmaceutical industry above others (Rodrik, 2018). Similarly, while trade integration has brought overall benefits for the agricultural sector of developed countries, it has negatively affected that of emerging nations (Otero, 2011; Salamanca et al., 2009). Furthermore, the adoption of economic policy-making paradigms of western international organizations has often been associated with the increase of inequality in both developing and developed regions (Stiglitz, 2015; Piketty, 2015, 2020). These economic effects have, as mentioned, in turn influenced several epiphenomena in the political and cultural sphere, with processes of political radicalization being the latest, most salient examples of such political and cultural expressions (Ozer, 2020; Varaine, 2019). Brexit, the appearance of Trump's anti-multilateral ethos, and the rise of national-populist movements around the world are just some of the many instances of such epiphenomena.

Knowing the ambivalent nature of globalization, one could argue that the topic is subject to discursive manipulation and, indeed, it has been. In such a context then, it is then worth asking: what is the road forward in terms of globalization? I pose empirical evidence holds the key here. I argue that such evidence must weigh the positive and negative effects at different aggregation levels to fully assess the real dimension of globalization's effects. Political agency, however, has a part to play, too. The debate on policy-making should be constructed based on rigorous scientific research. My dissertation aims to contribute to the existing scientific research with the inclusion of economic factors into the equation and political and cultural influences, in hopes of inspiring its use as the base for future public policy. More specifically, my work here is especially aimed at policymakers focused on the construction of policy associated with Western and non-Western aid, trade agreements, and political favoritism on development. Such work will be subsequently explained in preliminary detail.

Level of Analysis: Lately, one of the patterns of scientific advancement has been the study of phenomena at increasingly disaggregated levels to explore the relation of such micro-levels with already "understood" macro-phenomena. In the world of elemental physics, for instance, we now know that quarks are subatomic building blocks of hadrons, while hadrons are in turn the building blocks of the already "understood" atoms. In social sciences we have increasingly recognized the need for the study of subnational mechanisms of development to reassess already "understood" macro-phenomena like globalization (Hutzschenreuter et al., 2020).

All chapters of the dissertation deal directly with micro-level analyses of macro-level phenomena, as one can preliminarily see in Figure I. On the one hand, however, the chapters on aid (*Chapter 1*) and trade agreements (*Chapter 2*) explicitly contrast the impact of such international policy at national and local levels of development. On the other hand, the chapter on favoritism (*Chapter 3*) only includes the impact analysis at the subnational level, as such work focuses on the distribution inefficiencies of a, while universal (Hodler and Raschky, 2014), rather national phenomenon—leaders' predispositions to somehow favor their home regions.

Chapter 1 studies the relationship between development aid and health. The two phenomena share a global nature and have remained part of the international debate in tandem and as standalone issues. Therefore, my coauthors and I have found that an accurate description of the effects of such development finance would have to analyze particular elements of development (as health) and combine such analyses at different levels of geographic aggregation. Thus, using a well-known theory from aid literature, i.e., fungibility, we argue one could better understand the macro-effects of such international policy by contrasting its results at both the local and national level.

While, with some consensus, trade is often associated with general growth, this strand of literature usually also portrays the shortcomings of such trade when its gains are presented as having poor redistribution equilibria. Several interest groups (economic, political, mediatic, etc.) can have a say in the distribution of the gains of trade in exchange for support in their different spheres (Bombardini and Trebbi, 2012). The identification of such groups may be difficult to address, yet, I pose they can indeed be objectively proxied by, for example, subnational land characteristics. The identification of the particular distribution of trade agreement gains among interest groups subnationally is an exercise that I delve into in *Chapter 2* by using remote-sensing local data.

Poor distribution equilibria is a recurrent theme in Political Economy; for instance, pork-barrel politics (Hodler and Raschky, 2014) is often thought of as the main mechanism behind the regional disparity in rather homogeneous (resource-wise) subnational areas. Stepping away from the study of international policies, we look into unequal development in local regions of Latin American countries in *Chapter 3* and relate it to

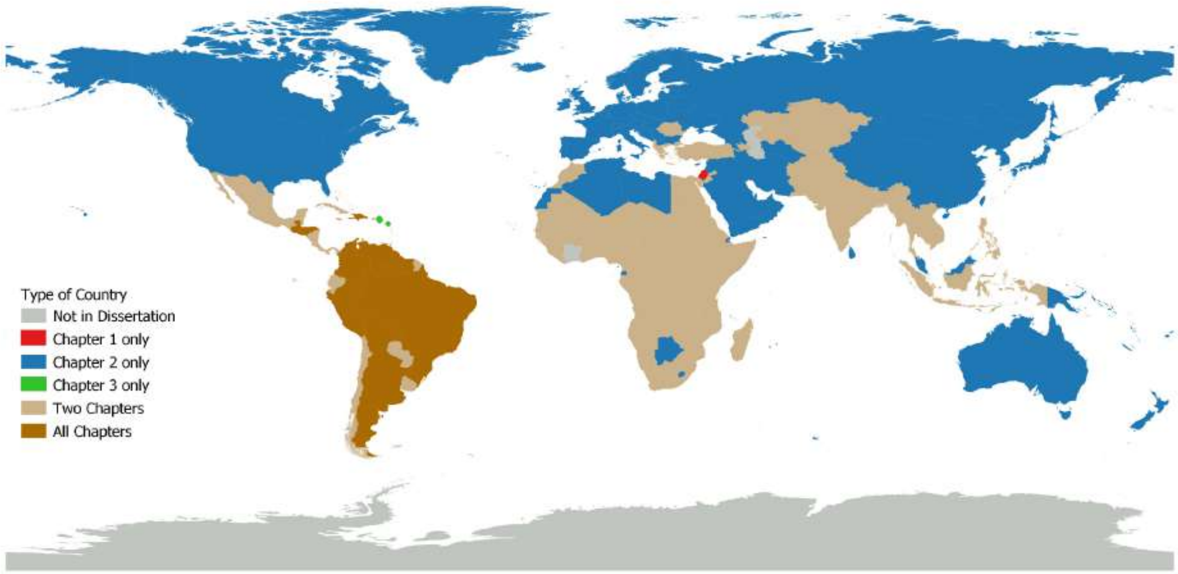


Figure I Countries under study

Notes: Figure I shows a World map that categorizes countries and autonomous territories by their presence in the chapters of this dissertation. In total, data from 219 countries/autonomous territories are used to perform the analysis present in this work. 93 countries are analyzed at least in two chapters, and 13 are analyzed in all. *Chapter 1* distinctively analyzes one sovereign state: Palestine, while *Chapter 2* does the same for 110. Finally, *Chapter 3* uniquely assesses Montserrat, and the Virgin Islands.

regional favoritism enacted by (parliamentary) leaders.

Data: The different works of this dissertation combine the use of data at different levels of aggregation and, therefore, use diverse well-regarded data sources. On the country level, we rely heavily on well-known data; for instance, for aid we use the information generated by a research lab at William and Mary: AidData. Similarly, for the data on trade agreements and trade flows we draw information from the work of Dür et al. (2014) and the World Bank's World Development Indicators, respectively. Conversely, the local level indicators, while mostly well-regarded by spatial economists, rely heavily on remote-sensing data, e.g., local GDP and human development index (Kummu et al., 2018) or land cover (ESA, 2017). First-hand data were also compiled to construct local level indicators needed in the third project, namely variables related to Latin American parliamentary leaders' birthplaces.

Methods: The main goal of this dissertation is to advance economic research in the intertwined subject of the epiphenomena of globalization and figures of development at both the micro- and macro-level, as one can initially see in Figure II. To do that, I have attempted to take into account potential sources of unobserved variable bias. In particular, apart from using time-series data in all chapters, which may already address some part of the unobserved bias, I use several identification strategies—e.g., instrumental variables and difference-in-differences—to identify plausibly exogenous variation and claim causal identification.

In two of the three chapters we exploit plausibly exogenous variation by interacting national with local level variables. In detail, and following Dreher et al. (2019a) and Lang (2020), among others, *Chapter 1* interacts aid donors' proxies of liquidity with subnational recipients' probabilities to receive such aid to construct an instrumental variable for aid at the local level. In *Chapter 2*, within a difference-in-differences strategy, I exploit local variation of national trade agreements by making use of subnational land cover characteristics that proxy different productive activities. Finally, inspired by the work of Hodler and Raschky (2014), in *Chapter 3* I exploit local and year variation by comparing development indicators of regions in the vicinity of parliamentary leaders' birthplaces to those far away from the birth areas.

While we mostly analyze causal mechanisms supported in long-established identification strategies, the plausibly random nature of our variables of interest—especially for *Chapter 2* with its land cover data and *Chapter 3* with its birth region data—allows us to further support our claim of causal identification.

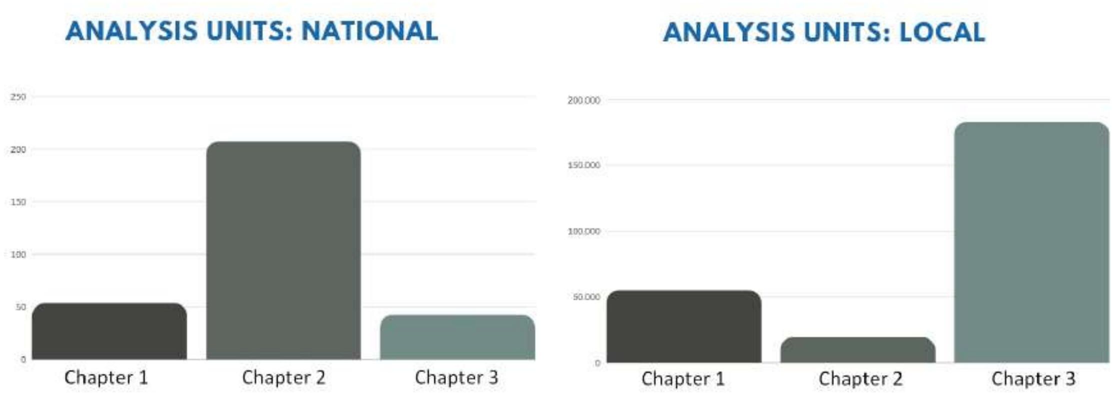


Figure II Units of analysis

Notes: Figure II details the number of distinct units of analysis for each chapter while distinguishing between local- and national-level analysis. Thus, *Chapter 1* has 54,946 and 53 distinct local- and national-level units of analysis, respectively. *Chapter 2* has 19,033 and 207. Finally, *Chapter 3* has 183,082 distinct local-level units, and 45 at the national-level.

Findings: *Chapter 1* focuses on health-related outcomes to study development. Specifically, we explore the effects of Chinese development finance on infant mortality rate between 2000 and 2014. We combine the advantages of sub-national data with those of country-level analysis, comparing results at different levels of aggregation. The comparison allows us to highlight a subnational mechanism connecting aid to development: fungibility (Dijkstra and White, 2003). We also compare the effects of Chinese aid to those of the World Bank, which the literature considers being very different (e.g., Gehring et al. 2019). In general, we expect China's non-interference aid-giving policy to make its aid particularly fungible. Indeed, we find indirect evidence of such fungibility: Chinese aid seems to allow recipients to use the "liberated" government budget to fund something else, e.g., other sectors, such as military projects (Langlotz and Potrafke, 2019), other health sectors, such as malaria treatment (Anshan, 2011), or use it somewhere else, as suggested by Seim et al. (2020). Thus, we find that Chinese aid increases infant mortality at the local level. We argue that such results are most likely driven by the worsening of health provision quality, as, for instance, both the number of births attended by skilled staff and the education of health staff in general decrease after aid comes into the region. In parallel, we also expand the debate on Chinese aid effectiveness as we find that, on average, Chinese aid actually improves infant mortality rates at national levels, which contrasts with fundamentalist perspectives on the matter portraying China as a "rogue donor" (Naim, 2007). The local- and national-level results suggest that the local positive effects of the "liberated" government projects dominate the negative effects of the Chinese ones on average, which nevertheless results in the Chinese aid being, in fact, beneficial for the nations which the aid targets. However, the development impacts of globalization are not only reflected through the distribution and politics of aid.

The distribution and politics of trade can also channel globalization's impacts. My single-authored project in *Chapter 2* studies how local factors can distort the gains of free trade agreements in most countries of the world. This paper analyzes the effects of Free Trade Agreements (FTAs) on various measures of local development in 207 countries over the 1990-2015 period. Using a difference-in-differences approach, I exploit spatial and time variation by comparing regions with (exogenously determined) exploitable and non-exploitable land in different FTAs' activation periods. I show that FTAs have a limited yet positive impact on a region's human development (as measured by the Human Development Index). My results also indicate that this limited impact can be explained by the positive effects of Free Trade Agreements on economic activity (night lights and GDP), together with the lack of repercussions of the same agreements on patterns of inequality (distribution of night lights among the population). Finally, I also show that FTAs' impact on human development is stronger for urbanized regions. Conversely, there is neither strong evidence of a weaker positive effect if trade partners belong to the Global North nor if the agreements include arrangements beyond the elimination of tariffs and quotas. Policies can shape patterns of development, yet are not enough to understand the distribution of development.

Chapter 3, while still using high spatial-resolution data like in the previous two chapters, focuses on a specific supra-region: Latin America and the Caribbean. Most formal institutions are stable in Western countries, yet those in Latin America and the Caribbean (LAC) tend to be less so. In this context, although less obvious, patterns of favoritism and rent-seeking are observable among particular elites. The chapter explores the degree to which the development of subnational regions is affected by their proximity to parliament leaders' birthplaces, and how this might arise from the *de facto* influence provided by the unstable *de jure* frameworks of LAC countries. We collected data on 366 political leaders and 238 distinct birth locations over the 1992–2016 period and constructed a panel of approximately 183,000 uniformly distributed subnational micro-regions across 45 countries and autonomous territories of the LAC region. Our results show that parliament leaders hold significant power to divert resources to regions in the vicinity of their birthplaces, as measured by increases in night light emissions and World Bank aid. The effect is overall informed by the degree of influence given by the peculiar constitutional frameworks of LAC countries.

Summary: The following chapters portray how, in the context of globalization, some social, political, and economic mechanisms can differentially explain local, national, and international development in the world. While processes of globalization such as aid and trade agreements do convey gains expressed in improved figures of human and economic development, they do not operate in a political and social vacuum. The lessons on the politics of redistribution visited in all chapters—but more heavily expressed in *Chapter 3*—speak loudly in favor of the latter. Globalization and the construction of welfare is a multi-layered process, dependent not only on international and national policy-making, but also on local factors that shape the numbers of *winners* and *losers* around the world. I hope that this thesis contributes to the latest, increasing array of work in charge of sustaining effective national- and local-level policy-making in the fields of aid, trade, and leaders' regional influence on specific and more general forms of development.

Chapter 1

Aid and Health

Joint work with Axel Dreher and Johannes Matzat

Abstract

We investigate whether and to what extent Chinese development finance affects infant mortality, combining 92 demographic and health surveys (DHS) for a maximum of 53 countries and almost 55,000 sub-national locations over the 2002-2014 period. We address causality by instrumenting aid with a set of interacted variables. Variation over time results from indicators that measure the availability of funding in a given year. The cross-sectional variation results from a sub-national region's "probability to receive aid." Controlled for this probability in tandem with fixed effects for country-years and provinces, the interactions of these variables form powerful and excludable instruments. Our results show that Chinese aid increases infant mortality at sub-national scales, but decreases mortality at the country-level. In several tests, we show that this stark contrast likely results from aid being fungible within recipient countries.

1.1 Introduction

Much has been written about foreign aid. According to its critics, donors allocate aid to ensure privileged access to the recipients' natural resources, create export markets for their goods, and reward strategic allies (Alesina and Dollar, 2000; Dreher et al., 2009). Whether and to what extent aid increases recipient countries' economic growth is highly debated (Werker, 2012; Galiani et al., 2016; Doucouliagos, 2019). Economic growth depends however on a large number of factors that are in turn affected by aid in different directions. The lack of robust evidence is then perhaps unsurprising. What is more, in many countries the aid is geographically highly concentrated. Even if aid affects outcomes in the regions it is given to, the effects might be insufficiently substantial to be measurable at the country-level. This observation has led recent studies to focus on sub-national regions, investigating the effects of aid on development at the province or district level, or even finer sub-national scales (Dreher and Lohmann, 2015; Bitzer and Goeren, 2018; Isaksson and Kotsadam, 2018; Gehring et al., 2019; Maseland and Minasyan, 2019). However, subnational analyses can be misleading if aid displaces other funds across space which is likely within but not across countries.

In this study, we focus on health-related outcomes as they are more tangible than overall measures of development such as economic growth. We combine the advantages of sub-national data with those of country-level analysis, comparing results at different levels of aggregation.[1] Such comparison allows us to uncover indirect benefits from aid to areas not directly targeted by it. Specifically, we explore the effects of Chinese development finance on infant mortality—the probability that a newborn baby will die before it reaches the age of one—as children's health has remained especially problematic at the subnational level of developing countries (Burstein et al., 2019).[2] China is the only bilateral provider of development finance for which geocoded data

[1]We are the first to investigate the causal effect of aid on health at the sub-national level for a large number of countries spreading across different continents. Sub-national studies, we are aware of, focus on infant mortality in Nigeria (Kotsadam et al., 2018) and Côte d'Ivoire (Wayoro and Ndikumana, 2019), health outcomes and perceived healthcare quality in Malawi (De and Becker, 2015, Marty et al., 2017), and the disease burden and severity in Uganda (Odokonyero et al., 2018). Most closely related to this paper is Martorano et al. (2020) who investigate the effect of Chinese aid on household welfare, focusing on 13 countries in Sub-Sahara Africa, and report positive correlations between Chinese aid and lower infant mortality in a difference-in-differences framework. Greßer and Stadelmann (2019) provide conditional correlations between the sub-national presence of World Bank projects and health-related outcomes in a sample of almost 40 countries. They find a positive correlation between projects and health quality. A large number of papers investigates the correlation between aid and health at the country-level, with mixed results (e.g., Williamson, 2008; Chauvet et al., 2009; Sonntag, 2010; Dietrich, 2011; Nunnenkamp and Öhler, 2011; Wilson, 2011; Doucouliagos et al., 2019; Kaplan et al., 2019).

[2]We use the terms "development finance" and "aid" for Official Development Assistance (ODA) and Other Official Finance (OOF) alike. During the time of our study, ODA was defined as financial flows that mainly aim at promoting the welfare and development of the recipients and have a grant element of at least 25 percent. OOF are official transactions that do not meet at least one of these criteria.

are available for a large number of recipient countries. Consequently, other bilateral donors or lenders cannot be included in this study. We however compare the effect of Chinese aid to those of the World Bank, for which geocoded data are equally available. This allows comparing two donors that the literature considers to affect development very differently—one perceived as being rather selfish and thus potentially harmful for recipient-country development, the other being a multilateral organization with much-appraised standards for allocating its aid (e.g., Gehring et al., 2019). We expect China's non-interference and "on-demand" policy to make its aid particularly fungible: To the extent that China responds to recipients' requests for aid without any policies in place that would ensure the aid is additional, recipient governments can easily use it to finance projects it would have funded anyway. The World Bank, to the contrary, has policies in place that aim to ensure the additionality of its aid, carefully monitoring the use of its funds (Dreher et al., 2021b).

According to Dijkstra and White (2003, p. 468), fungibility is "the idea that aid pays not for the items which it is accounted for but for the marginal expenditure it makes possible."[3] Chinese aid might replace government expenditures or other donors' aid in two ways.[4] First, Chinese aid might finance the exact same project that the recipient would have built absent the aid. The recipient could use the funds that are now available in its budget either to finance something entirely different, such as military expenditures, or—to the extent that the aid creates leeway in budgets related to health expenditures only—rather a similar project, in the same geographic region or elsewhere. Second, China might fund a project similar to but different from what the recipient government or other donors would have financed without China's support. The recipient government might then prefer to not implement a related project at all, or implement it in a different geographic area. This would prevent the financing of two similar projects, in one location, at the same time. The project with goals different from those of the government is, likely, less effective in achieving the recipient's goals and would thus make the aid appear less effective than the similar government's project, at regional scales. This is because outcomes would be more likely to improve in regions that do not receive Chinese aid compared to those that do. At the aggregate, country-level, effects of the government-financed project would however register. Aid can therefore appear less effective at the subnational than at the country-level.

As we explain in section 1.2, our sub-national analyses test the effect of aid on infant mortality reported by surveyed households in the area within a radius of 0.5, which amounts to roughly 55 km at the equator. To this end, we merge health indicators from 92 demographic and health (DHS) surveys on 53 developing countries over the 2002-2014 period and combine them with a geocoded dataset on Chinese development finance. Our first regressions follow the method of Isaksson and Kotsadam (2018), comparing the effect of projects that have disbursed aid ("active" projects for short)

[3]As cited in Leiderer (2012, p. 4).

[4]Hernandez (2017) shows that the World Bank competes with China by reducing the number of conditions in its aid programs when China is also present in a country. Zeitz (2021) also shows that the World Bank is responsive to Chinese competition.

to those that have not ("inactive" projects). Comparing observations that have been selected as project-sites at a certain point in time to others that have been selected as well (but are as yet inactive) reduces the omitted-variables bias compared to a simple cross-sectional specification. We thus compare the effect of active projects on outcomes relative to no aid projects, to the effect of inactive projects on outcomes relative to no aid projects in a region. We refine our basic regressions to address causality further, using an instrumental variable (IV) approach, at the local- and country-levels. Our instruments for aid follow Dreher et al. (2021a,b), and Bluhm et al. (2020), who suggest yearly production volumes of physical inputs into (tied) Chinese aid projects and the availability of foreign currency reserves as proxies for the availability of aid at any point in time. As they explain in some detail, China produces substantially more steel, cement, and other production materials than it can use domestically. It then uses them as inputs in its foreign aid projects. When production is high, the supply of aid becomes cheaper and should thus increase. Along similar lines, China uses its aid projects to earn interest on its foreign currency reserves. At times when reserves are higher, the supply of aid should also be higher. We interact these indicators of supply with the probability that a country or region receives smaller or larger shares of the year-to-year fluctuations in aid. China could be expected to expand its aid program beyond its traditional clients in years with abundant supply. Alternatively, regions that have received China's aid in the past might receive more of it when supply is high. The latter effect has been shown to prevail (Dreher et al., 2021a,b), as seems to be the case for bilateral donors more broadly (Werker et al., 2009; Nunn and Qian, 2014; Dreher and Langlotz, 2020).

The intuition of our IV regressions follows those of a difference-in-differences approach. We exploit the differential effect of China's production volumes of physical inputs into aid projects and the availability of foreign reserves on Chinese health projects in regions with a high probability of receiving aid compared to those with a low probability of receiving aid. The identifying assumption is that health outcomes in sub-national regions with differing probabilities of receiving aid will not be affected differently by changes in the supply of the inputs into aid projects and reserves, other than via the impact of aid, controlled for the probability to receive aid, country-year-fixed effects, province fixed effects (or country- and year-fixed effects in the country-year setting), and the other variables in the model. In other words, as in any difference-in-differences setting, we rely on a treatment that is (conditionally) exogenous and the absence of different pre-trends across groups. Controlling for a set of fixed effects that capture the direct effects of these variables, currency reserves, and physical inputs cannot be correlated with the error term and are indeed (conditionally) exogenous to aid. For different pre-trends to bias our coefficients, patterns across regions with a high compared to a low probability of receiving aid would have to vary in tandem with year-to-year changes in these supply factors. We test this possibility below and find no evidence of such a threat to our identification strategy.[5]

We report our main results in section 2.3. They show that Chinese health aid *increases* infant mortality at the local-level, but *decreases* mortality when focusing on countries instead. We argue that these differences can best be explained by fungibility, and test this channel in a number of ways. First, we show that the availability of clinics in the vicinity of Chinese aid projects is not affected by aid, potentially indicating that recipient governments channel the aid to build facilities they would have financed themselves or with aid from other donors absent of Chinese support. Second, we find that the number of deliveries in health clinics is reduced by aid. This can explain how fungibility renders the effect of aid at the sub-national level negative rather than just null. Aid-financed facilities specialize in areas that are less effective in reducing infant mortality than the facilities replaced by aid, or alternatively, poach skilled staff from an existing health clinic. Service provision thus decreases both in terms of quantity and quality (also see Deserranno et al., 2020). Third, and in line with this interpretation, we show that the number of births attended by skilled health staff is reduced by aid, while more births are attended by traditional health staff in turn. Fourth, the turnover of health staff at existing clinics increases, and average (educational) quality declines as a consequence of aid. It thus seems that aid-financed projects poach staff from existing clinics. Fifth, the effect of aid on mortality is larger when governments are already allocating relatively high levels of domestic public expenditures to the health sector prior to the receipt of aid and when more aid has been received from Western donors. This is in line with the interpretation that fungibility should play a larger role when alternative sources of funding are available. More directly, at the local-level, we also find that World Bank aid for health is reduced as a consequence of Chinese support to the same localities. Sixth, we test whether results in a sector of particular interest to China—but not necessarily to the recipients of its aid—improve as a consequence of aid, which would be the case if total funds focus more strongly on these sectors as a consequence of China's interventions. In line with this expectation, we find that Chinese health aid increases the probability that women took anti-malaria pills during pregnancy (with the fight against malaria being a major goal of Chinese operations).[6]

We compare our results for China to those of World Bank aid (in section 1.4) and test a potential alternative explanation for our results as well as their robustness (in section 1.5). Given that the World Bank carefully chooses which projects to fund and monitors its projects throughout the implementation period, fungibility should be lower

[5]Our empirical strategy can also be seen in relation to an evolving literature on shift-share instruments, such as Goldsmith-Pinkham et al. (2020). We discuss this in section 1.5.

[6]The three-year plan resulting from the Forum on China-Africa Cooperation in 2009, for instance, states in one out of the three points addressing public health: "The two sides noted with pleasure the deepening health cooperation between the two sides. In particular, the hospitals and anti-malaria centers that China has undertaken to build will play a positive role in improving the health care level and protecting people's health in African countries." The ongoing Chinese anti-malaria campaigns have also not gone unnoticed in the media. Reuters for example headlined in 2009 "China adopts "Malaria diplomacy" as part of Africa push" (see https://www.focac.org/eng/zywx_1/zywj/t626387.htm, https://www.reuters.com/article/idUSSP503140, accessed April 17, 2020).

when compared to China.[7] In line with this expectation, we do not find evidence in support of the hypothesis that the World Bank's health-related aid is fungible. Just like Greßer and Stadelmann (2019) we even find that World Bank projects reduce infant mortality at local scales. Regarding an alternative interpretation of our results for China, a skeptical reader might think that the composition of women giving birth is affected by aid. To the extent that aid differentially affects the number of mothers from vulnerable populations who give birth, increases in mortality could be the consequence of compositional differences. Our results indeed show that aid affects fertility and mortality differentially for different age groups and ethnicity of the mothers. However, when we hold these differences constant, our key results remain unchanged.

On balance, our results thus seem to indicate that fungibility makes Chinese aid appear to be negative at sub-national scales, while the overall effect of aid is positive. This observation bears important implications for policy-makers and research alike (which we discuss in the concluding section 3.5). To the extent that donors of aid are interested in outcomes in a particular locality, the incentives of recipient governments to re-direct their own funds or aid from other donors to alternative sectors or areas would also need to be considered. While fungibility is a problem many donors seem duly aware of, it is usually understood in a way less subtle than the mechanisms uncovered here. For example, Langlotz and Potrafke (2019) show that aid increases recipients' military expenditures, implying that part of the aid is used for entirely different purposes. In this paper, we show that fungibility can be important even if aid is spent in the same sector but in different geographic areas, or in the same area in a related sub-sector. Our study has implications for the recent trend in aid effectiveness studies that focuses on small sub-national areas rather than broader regions or countries. While the sub-national analysis of data bears important advantages—in terms of a greater number of observations and more rigorous identification—the effects of aid reflected at this level might turn out inconsequential or negative even if the aggregate effect is positive.

1.2 Data and Methods

The literature on the effectiveness of foreign aid below the country-level is scarce, mainly due to a lack of geo-coded data on aid, relevant outcomes, and important control variables. In this study, we rely on data for more than 2,000 Chinese aid project locations in the years 2000-2014 that have been geo-coded in Bluhm et al. (2020).[8] For comparison, we also use data on geo-coded World Bank projects that have been

[7] Also see Van de Walle and Mu (2007).

[8] The number of projects made available by Dreher et al. (2021b) is substantially larger (3,485 projects). This is because Bluhm et al. (2020) geo-code only those projects that were completed or in implementation during the period their sample covers, and due to the lack of sufficiently precise geographic information for some of the projects.

approved in the health sector over the same period—as preliminarily see in Figure 1.1.[9] Though not free from geopolitical interference of its donors (Dreher et al., 2009, 2019b), the World Bank has rigorous standards for the ex-ante evaluation of potential projects. In contrast, China gives substantial leeway to recipient countries on where to spend its aid (Dreher et al., 2018, 2019a). These two donors are thus very different in terms of how they exercise control over the recipients' use of aid, allowing us to compare the effects of aid for a donor that is particularly lenient to one that is particularly stringent among the set of all official donors of aid (Gehring et al., 2019).[10]

a) China **b)** World Bank

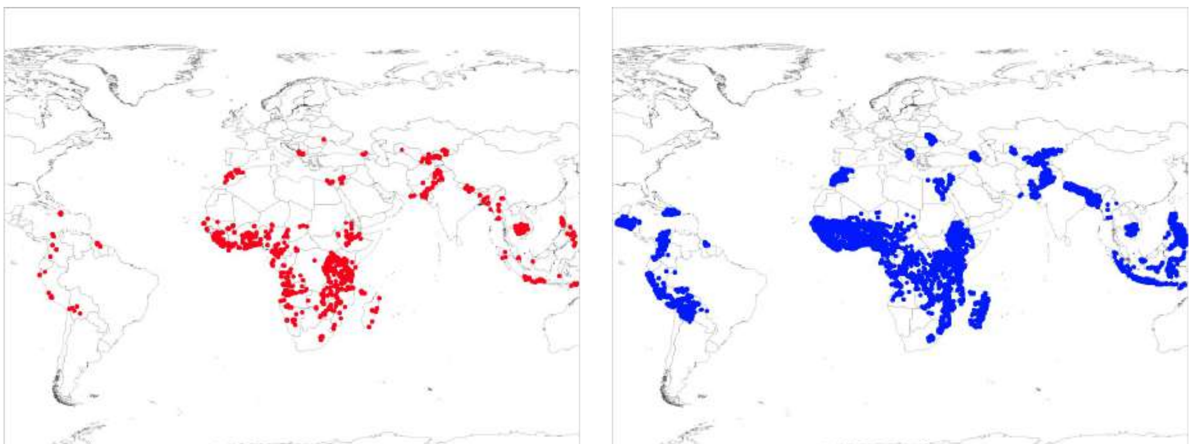


Figure 1.1 Chinese and World Bank aid in the World

Notes: The projects depicted have geo-coordinates that (i) correspond to an exact location, or (ii) are within 25 km of an exact location (i.e., AidData's precision codes 1-2).

We combine the data on aid with health indicators from 92 demographic and health surveys in a maximum of 53 countries over the 2002-2014 period. There are two main types of DHS—Standard and Interim. Standard surveys cover between 5,000-38,000 households per country and are typically conducted every five years. Interim surveys include fewer indicators and smaller populations. Both DHS types are however nation-

[9] AidData (2017) provides these data in collaboration with the World Bank. For both Chinese and World Bank aid, we only include projects where geo-coordinates (i) correspond to an exact location, or (ii) are within 25 km of an exact location (i.e., AidData's precision codes 1-2). Note that, while data are available as of 1995, in our main regressions we restrict the sample to the 2000-2014 period to ensure comparability.

[10] It is important to note however that China only grants a small share of its aid as budget aid and does not simply finance any type of project proposed by recipient countries. Obviously, recipient governments could otherwise simply ask China to support its military budget rather than asking it to finance a health-related project and use the additional budgetary leeway in support of the recipient's preferred spending.

ally and regionally representative.[11] Data are available for census enumeration areas, which can be villages in rural regions or blocks of a city in urban spaces. On average, these enumeration areas cover 0.38 (0.34) km2 for urban (rural) clusters and survey 25-30 households. The center of each enumeration area ("DHS cluster") is geo-referenced but slightly displaced to protect the anonymity of the observed units (see ICF, 2013 for details).

a) Infant Mortality **b)** Probability Aid

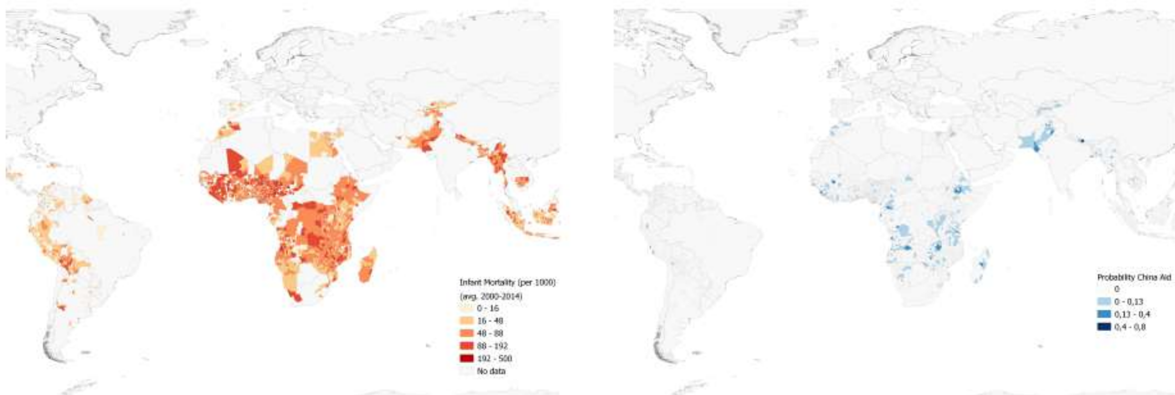


Figure 1.2 Infant mortality and Chinese aid

Notes: Both maps display figures at the ADM2 level. The figure on infant mortality rates on the left averages values from 2000 to 2014 to coincide with our time sample. The figure on the right shows the probability to receive Chinese projects as described in Section 1.2.

One key advantage of the DHS is the inclusion of information about children born prior to a survey. This allows us to extract information about the health status of children also in years without a DHS so that we can exploit the full range of years for which we have information on aid flows. The resulting dataset includes yearly information on infant mortality for 103,008 children per year, on average, over the 2002-2014 period, defined as the number of children that died before they were 12 months old in 1,000 children born alive. The total number of children included in our sample is around 1.3 million, and the average infant mortality rate is 55. We make use of 54,946 different DHS clusters, with an average of 3.7 children covered per cluster and year. 390,869 (344,934) births were registered in areas with active (inactive) Chinese projects—for a preliminary visualization of the key aid and health data used see Figure 1.2. There are 2,161 Chinese aid project locations in our sample; 1,507 of these locations are within a radius of 55 km of at least one DHS cluster, and 217 of these are health-related projects.

[11]Our sample includes 90 Standard surveys, and two Interim surveys (Rwanda in 2008 and Egypt 2003), out of the available 296 Standard and 6 Interim surveys. We do not use the full sample either for the lack of sufficiently precise geo-referenced information on aid projects or the lack of geo-referenced information in DHS.

For further tests, we also extract information on whether or not a birth was attended by skilled health personnel for 46,913 children per year, on average. Skilled health personnel is defined as a doctor, nurse, or midwife. We contrast the use of modern health services by examining reliance on informal and local health services. To this end, we use data on the number of births attended by traditional birth attendants. To examine whether changes in the frequency that skilled staff assists births are driven by developments in the private or public sector, we look at the place of delivery. In particular, we examine the number of deliveries that took place at private and public health facilities respectively, per 1,000 deliveries. Similarly, we examine whether there is evidence pointing at health aid having an impact on the distance to hospitals by assessing the share of women that did not have their deliveries in hospitals because they considered they were too far. To broaden our analyses of the health sector and capture potential interdependencies between different areas within the health sector, we examine whether the number of pregnant women (out of 1,000) who took antimalarial pills is affected by aid (the fight against Malaria is a major part of Chinese health-related aid interventions). We also evaluate if the supply of hospitals is altered relying on a geocoded dataset covering almost 100,000 health facilities in Sub-Sahara Africa, provided by Maina et al. (2019).[12] To test whether aid affects the composition of staff, we leverage surveys of staff in some of these facilities (from the Service Provision Assessments which are part of the DHS program). We make use of nine surveys covering seven countries, 8,514 health facilities and 260,616 workers.[13] Specifically, we assess whether more or fewer people (out of those interviewed) were hired in a given year and whether they are more or less educated than other interviewed workers that were already employed in the previous year.

Absent any fungibility of aid, one would expect the effect of aid to be most visible when we focus on small areas in the vicinity of aid disbursements. Aid is often highly concentrated on certain regions inside a country (Dreher and Lohmann, 2015). Even if the aid is effective there, improved development outcomes might not visibly materialize at the country-level. We expect the opposite to be true if aid is fully fungible. The scenario under which fungibility would make the local effect of aid appear most harmful is one in which the aid is fungible between geographic areas, or over time within the same sector. For instance, assume that a recipient government intends to use its own funds to reduce infant mortality if no external donor is available.[14] Assuming that the government would instead finance that project at a later point in time, or elsewhere, under a scenario where China finances an anti-malaria project, the effect of Chinese aid on infant mortality would appear negative, given that any region is more likely to

[12]The dataset is based on national lists which are compiled by the respective Ministry of Health and are supposed to be a complete and authoritative listing of health facilities. They are most comprehensive for facilities managed by the government, non-governmental organizations, or faith-based organizations. To our knowledge it is the most comprehensive dataset covering geocoded health facilities in Sub-Sahara Africa available.

[13]The seven countries are Bangladesh, Haiti, Kenya, Malawi, Namibia, Senegal, and Tanzania.

[14]Figure 1.A.1 in the Appendix illustrates this graphically.

reduce infant mortality at a time they do not receive aid from China compared to when they receive it.

We test the local effect of aid by focusing on small geographic areas within a radius of 0.5 around the center of each DHS cluster.

We estimate our basic regressions as follows:

$$Health_{i,t} = \beta_1 active_{i,t} + \beta_2 inactive_{i,t} + \beta_3 X_{l,t} + \beta_4 \eta_{j,t} + \beta_5 \gamma_k + \epsilon_{i,t} \qquad (1.1)$$

where $Health_{i,t}$ is the infant mortality rate in recipient cluster i in year t. Our sample covers all 54,946 sub-national clusters within a radius of 55 km for which we have geocoded data on Chinese projects in the 2002-2014 period.[15] Variation in the availability of aid within DHS clusters over time is insufficient to allow the inclusion of cluster-fixed effects. We however include fixed effects for country-years $\eta_{j,t}$ and ADM2 regions (such as districts and municipalities) γ_k. We cluster standard errors at the ADM2-year level ($\epsilon_{i,t}$ in equation (1) represents the error term). All regressions use cluster-weights as recommended by DHS.

Rather than comparing clusters that received aid to those that did not, we follow the approach in Isaksson and Kotsadam (2018) and compare health outcomes in clusters with projects that have started disbursing aid ($active_{i,t}$) before the birth year of the child to outcomes in clusters that also received projects during the sample period, but that have not started to disburse before the child was born ($inactive_{i,t}$).[16] In our sample, 25 percent of the observations refer to enumeration areas that are located within 55 km of an active Chinese project, while 21 percent refer to projects where the implementation period has not yet started. Rather than comparing cluster regions that have been selected as project-sites to others that have not, we thus exclusively compare health outcomes in regions that have been selected as project sites, but at different points in time. We expect this to reduce the importance of omitted variables bias to a considerable extent.

In order to estimate the effect of aid, we test whether β_1 is significantly different from β_2 in equation (1) above, in addition to testing whether any of the two is significantly different from zero. In order to code a cluster as active or inactive, we rely on the first aid project in each cluster in our sample. This ignores the potential effect of earlier projects (on which we have no data). Regions with active aid projects earlier in the sample period might be more likely to have received projects in the recent past

[15] Focusing on the sub-national level addresses a further important problem of the aid effectiveness literature. According to Ioannidis et al. (2017), only about one percent of the 1,779 estimates in the aid and growth literature surveyed have adequate statistical power (see also Doucouliagos, 2019). Given the small number of observations available at the country-year level, there is nothing that researchers can do to increase power. Focusing on the sub-national level—and the substantially larger number of observations available there—thus is an important step forward in this literature.

[16] Isaksson and Kotsadam (2018) make use of this approach to show that Chinese aid in Africa fuels corruption. Kotsadam and Tolonen (2016) use the same approach to investigate the effect of natural resources on gender inequality in Africa. We also tried to make use of data for children born to the same mothers but had insufficient data on projects nearby to allow us to control for mother fixed effects.

compared to regions that receive a project at a later point in time. The effect of active projects that we measure is thus not necessarily the effect of the specific projects in our sample, but might to some extent reflect the effects of earlier projects as well.[17]

To further reduce the importance of omitted variables bias, we control for a number of variables at the cluster level, included in the vector $X_{l,t}$. We include variables that we expect to be correlated with health outcomes and the probability to receive an aid project in a particular year, mostly taken from the DHS geospatial covariates dataset (Mayala et al., 2018): an index variable ranging from zero to one (with one referring to extremely urban, and zero to extremely rural), indicating the proportion of built-up infrastructure nearby; a categorical variable of the predominant mode of settlement in the region ("SMOD population"), which distinguishes between rural, urban, or urban center; an indicator of a region's average slope (measured in degrees, where one degree roughly equals 111 km); rainfall (in meters per location and year); travel time to the next larger settlement of 50,000 people or more (measured in days); a vegetation index measuring the density of green leaves (between zero and one with larger values indicating denser vegetation); and the logarithm of the number of people living within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster location.[18] We control for distance to (i) the nearest water body, (ii) the nearest area protected by the United Nations if any, and (iii) the border of the nearest neighboring country (all measured in kilometers).[19] All time-varying variables are lagged by one year.[20]

[17] Just like Isaksson and Kotsadam (2018) we test robustness along several dimensions. First, we estimate project-fixed effects regressions for the reduced sample that we have information for health outcomes before and after a project started to be implemented. Second, we control for the duration of active projects and the time until an inactive project becomes active (expecting the former to have a positive effect on outcomes, and the latter to be insignificant). We also test whether the results remain robust when we code projects as inactive once they are completed, and when we focus on projects that have been active in the five years prior to an interview.

[18] The data were generated by the Center for International Earth Science Information Network of Columbia University in 2016. They have a 1 km by 1 km spatial resolution for the years 2000, 2005, 2010, and 2015; we linearly interpolate the years in between.

[19] A number of these variables—the urban areas indicator, travel time, and all distance data—do not vary over time. Others are not available for every year. Data for population count, mean temperature, and the vegetation index, therefore, refer to the year 2000; data for slope are from 1996. Precipitation, aridity, and mean temperature refer to the year of the respective DHS. Note that we do not include data derived from the household level, such as education or wealth, as these are likely to be affected by aid as well (Martorano et al., 2020). Appendix 1.B reports the exact definitions of all variables and their sources. Appendix 1.C shows descriptive statistics.

[20] In tests for robustness, we also control for luminosity as an indicator of development and a binary indicator that is one when the head of the government's birth region is within 55 km of a DHS cluster. Controlling for development is crucial and problematic at the same time. According to Bruederle and Hodler (2018), nightlight and health indicators are correlated at the local-level. Omitting nightlight, our coefficient for aid in the OLS regressions might just capture an effect of aid on development more broadly. On the other hand, luminosity might be a transmission channel by which aid affects health. Hodler and Raschky (2014) show that leaders favor their birth regions, leading to better development. Dreher et al. (2019a) show that one channel for such favoritism can be Chinese foreign aid. We, therefore, do not include these variables in the main regressions. When we include their lagged values our results are however robust.

Our second set of sub-national regressions focuses on the number of projects a cluster region receives rather than the binary indicator.[21] The second stage of our 2SLS regressions is:

$$Health_{i,t} = \beta_1 Aid_{i,t-2} + \beta_2 p_i + \beta_3 Pop_{l,t-1} + \beta_4 \eta_{j,t} + \beta_5 \gamma_k + \epsilon_{i,t} \tag{1.2}$$

The aid effectiveness literature typically uses disbursements of aid rather than commitments, given that aid can only register impacts if it has been disbursed. Such data are however unavailable for China, so we rely on projects committed.[22] In our main specifications, we lag aid by two years, following previous work (e.g., Dreher et al., 2021b). One might expect aid to take longer to potentially affect outcomes, given that commitments need some time to disburse. However, while this might hold true for Western donors, Chinese aid typically disburses quickly, so that we do not add further lags in our main specifications (e.g., Dreher et al., 2019a, 2021a). Given that the appropriateness of these lags is largely an empirical question, we however test different timings in further specifications.[23] In addition to focusing on health aid, we also investigate the totality of aid projects. Such aid might be less likely to affect health outcomes. On the other hand, to the extent that aid is fungible initial labels might not matter much. What is more, aid given to areas other than health might easily affect health outcomes as well.[24]

Of course, aid is likely to be endogenous to health outcomes. One likely source of endogeneity is reverse causation in which recipient economic features influence the allocation of aid. On the one hand, China might provide more aid to poorer regions. On the other, donors might prefer to channel more aid to wealthy regions if these recipients provide more attractive commercial opportunities (Frey and Schneider, 1986; Dreher et al., 2019a) or might want to avoid difficult geographic regions.[25] It should also be noted that a large number of variables that are excluded from our models are potentially correlated with aid as well as with our health indicators, introducing omitted variables bias. To address the potential endogeneity of aid, we employ an instrumental-variables strategy.

[21]Note however that in most cases sub-national regions receive just one health-related project at any point in time. Using a binary variable that indicates whether or not a new project has been committed in a year instead of project numbers does not change our results.

[22]One might think of using commitment amounts rather than project numbers. However, 43 percent of the health projects in our sample have no data on the amount of aid. Therefore, we prefer to focus on project numbers but report results using amounts in tests for robustness below.

[23]In line with Dreher and Lohmann (2015) and Galiani et al. (2016), we below also average aid projects over three years to test robustness.

[24]This also comes with the advantage of a much-increased number of aid locations that we can exploit in the analyses.

[25]Empirical research on Chinese aid allocation demonstrates a strong negative correlation between Chinese ODA and the per-capita income of recipient countries (Dreher and Fuchs, 2015; Dreher et al., 2019a). However, Chinese OOF (in Africa) tends to favor creditworthy countries (with higher loan repayment capacity) and countries that import more from China (Dreher et al., 2019a). Cervellati et al. (2020) show that Chinese (infrastructure) projects are less likely to be implemented in areas with higher risks of malaria.

Our proxy for the availability of Chinese aid in a year follows Dreher et al. (2021a), Bluhm et al. (2020) and Dreher et al. (2021b). Dreher et al. (2021a) introduces (logged) annual Chinese steel production as part of an instrument for Chinese aid. They argue that Chinese officials are rewarded for producing as much steel as possible, resulting in a production that is in excess of domestic needs. Parts of the excess supply are then shipped abroad as exports, foreign direct investment, or aid. The larger the volume of steel available in a year, the cheaper are additional Chinese aid projects using this material, consequently increasing the supply of aid. Countries with established aid relationships are then expected to receive a larger chunk of this aid, all else equal. Since Dreher et al. (2021a) have first introduced this instrument in 2016, a number of follow-up studies have used it, including Brazys and Vadlamannati (2021), Gehring et al. (2019), Humphrey and Michaelowa (2019), Zeitz (2021), and Iacoella et al. (2021). The instrument was further refined in Bluhm et al. (2020). Rather than just using China's production of steel, they suggest focusing on several additional factors that are important inputs for Chinese aid projects: timber, glass, iron, aluminum, and cement. Given that the production of these materials trends over time, Bluhm et al. (2020) de-trend them rather than only relying on country-year fixed effects to absorb the trend. They then extract the first principal factor of the five de-trended (logged) input factors and interact it with the number of years each region has received positive amounts of Chinese aid over the sample period. The resulting product is our first instrument for Chinese aid.

Our second instrument is introduced in Dreher et al. (2021b). As they point out, a key reason for China to grant non-concessional, dollar-denominated loans is the possibility to earn interest rates above what would be possible domestically. At the same time, concessional projects should become more easily available as well. This is because the income effect of cheaper aid provision should make the availability of both types of aid more likely (though a substitution effect would work in favor of non-concessional projects).[26] In years in which China's reserves of foreign currency are high, Dreher et al. (2021b) therefore expect (and show) the supply of aid to increase. Their instrument is then the interaction of changes in China's net currency reserves and the location-specific probability to receive Chinese aid. In our study, the instruments are the interactions of the two indicators for Chinese aid which vary over time but not between sub-national regions or countries ($Input_{t-1}$, $Reserves_{t-1}$), and the probability of receiving aid from China, p_i, which varies across recipient locations but not over time. We calculate the probability of receiving aid as the share of years in the 2000-2014 period a region has received positive amounts of aid.[27] More precisely, we define the probability of receiving aid from China as $p_i = \frac{1}{15}\sum_{(y=1)}^{15} p_{(i,t)}$, where $p_{i,t}$ is a binary variable that equals one when recipient location i received a (health) project from China in year t. The first-stage regression is:

[26] To the extent that part of the materials used in a project are purchased in international markets, the availability of reserves makes such inputs appear cheaper as well.

[27] This directly follows the analyses in Nunn and Qian (2014), Bluhm et al. (2020), Dreher and Langlotz (2020), and Dreher et al. (2021a). Also see Werker et al. (2009).

$$Aid_{i,t-2} = \beta_1 Input_{t-3} * p_i + \beta_2 Reserves_{t-3} * p_i + \beta_3 p_i + \beta_4 Pop_{l,t-1} + \beta_5 \eta_{jt} + \beta_6 \gamma_k + \mu_{i,t}. \quad (1.3)$$

In our IV regressions we control for (log) population, $Pop_{l,t-1}$, in a cluster l but omit the other control variables. While their inclusion would increase the efficiency of the estimator, they are also potentially correlated with aid and health and therefore introduce endogeneity to the estimates. Given that our identification strategy does not require their inclusion, we estimate our main regressions without them (but test robustness to their inclusion below). Also, note that we control for the cluster-specific probability p_i to receive health projects in the first and second stages of the regressions.[28] As before, we cluster standard errors at the ADM2-year level and include country-year- and ADM2-fixed effects. Our country-level regressions are analogous to the local ones; with the probability to receive aid and the other variables measured at the country-level, and fixed effects for years and countries instead. Given that the probability of receiving aid does not vary for a country over time, it is captured by the fixed effects. We cluster standard errors at the country-level there.

One might think that the Local Average Treatment Effect (LATE) we estimate with physical inputs into aid projects would not well predict projects in the health sector. However, the majority of health projects in our sample involve the construction and rehabilitation of hospitals and other health facilities—as one can preliminary see on the field "Construction Rehabilitation" of Table 1.D.4 in Appendix 1.D. These projects rely heavily on Chinese raw materials (and workers).[29] What is more, the abundance of physical aid inputs arguably also increases the supply of aid projects that are unrelated to such inputs. This is the case if the income effect of cheaper aid provision exceeds the substitution effect away from the (relatively more expensive) projects that do not require physical inputs in years in which such inputs are more easily available (Dreher et al., 2021b). Given that China's aid responds to recipient's requests for aid projects, and recipients have no reason to alter the composition of their requests in response to Chinese production of raw materials, we consider it likely that all types of aid projects are more easily available in years the provision of aid becomes cheaper. We also have no reason to assume that projects financed due to the easy availability of reserves differ fundamentally from projects financed independently of reserve availability. While we use both instruments jointly in our main regressions, we test robustness to including them individually.[30]

One might also be concerned that our instruments violate the exclusion restriction because the probability of receiving aid may directly affect population health. However, our second-stage regressions control for the probability of receiving aid itself. China's

[28] This variable is however omitted from the OLS regressions that we also show below.

[29] For details, see Dreher et al. (2021b).

[30] We also tested an alternative instrument that includes China's aid budget as part of the interacted IV (Temple and Van de Sijpe, 2017; Dreher et al., 2021b). However, first-stage F-statistics are very low in the country-level analyses, so we do not report these results.

aid inputs are captured through the inclusion of country-year-fixed effects (or country- and year-fixed effects in the country-level setting). Controlling for country-year-fixed effects, these variables cannot be correlated with the error term and are thus (conditionally) exogenous to aid. Given that we control for the probability of receiving aid, its interaction with an exogenous variable results in an exogenous instrument under the assumption of parallel trends before the intervention (Bun and Harrison, 2018; Nizalova and Murtazashvili, 2016).[31] The intuition of this approach is that of a difference-in-differences regression, where we investigate a differential effect of the ease of access to aid projects in regions with a high compared to a low probability of receiving aid. The identifying assumption is that health outcomes in regions with differing probabilities of receiving aid will not be affected differently by changes in China's production of aid inputs and the availability of reserves, other than via the impact of aid, controlling for the probability to receive aid and the country-year-fixed effects. In other words, as in any difference-in-differences setting, we rely on an (conditionally) exogenous treatment and the absence of different pre-trends across groups. In order for different pre-trends to bias our estimates, trends across regions with a high probability compared to a low probability of receiving aid would have to vary in tandem with year-to-year changes in China's production of raw materials and reserve availability.

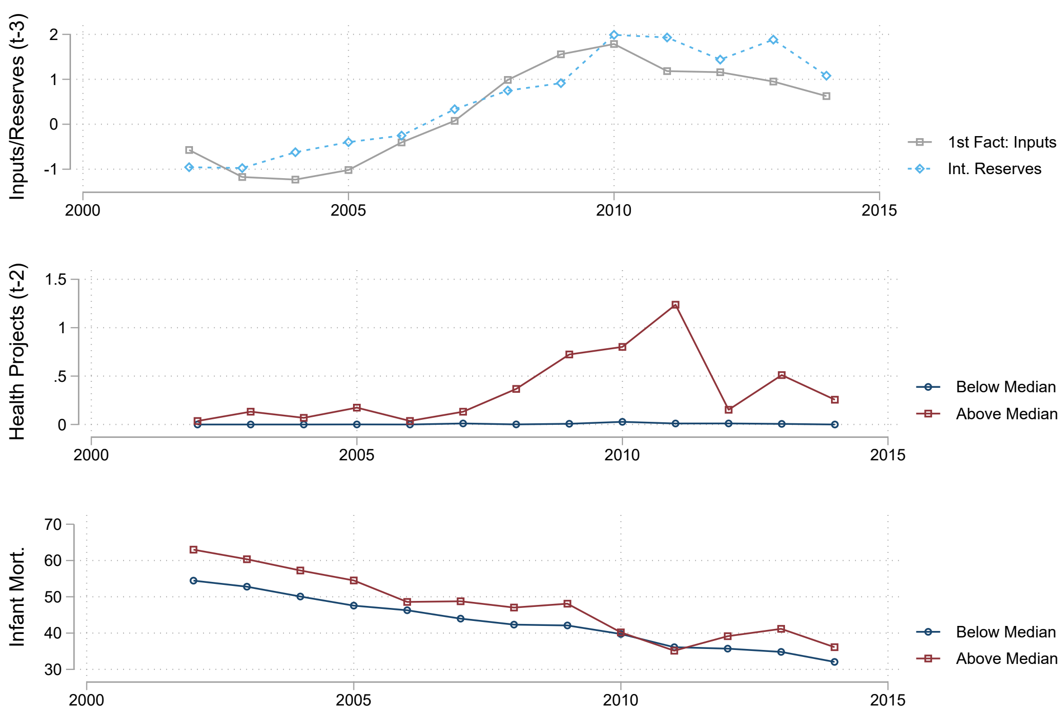
Following Christian and Barrett (2017), we plot the variation in Chinese aid inputs and currency reserves in concert with the variation in the average number of new aid projects world-wide and the infant mortality rate for two different groups that are defined according to the median of the probability to receive aid (given that any aid was received). Figure 1.3 plots this graph. The results give little reason to believe that the parallel trends assumption is violated in our case. More precisely, the probability-specific trends in infant mortality seem rather parallel across the regular recipients (those with a probability of receiving aid that is above the median) and the irregular recipients (those with a probability of receiving aid that is below the median). There is also no obvious non-linear trend in regular recipients compared to irregular recipients that are similar in aid and mortality.

The excludability of our interacted instrument would be violated if changes in China's physical inputs into aid projects or reserves availability would affect sub-national health outcomes differentially in regions with a high probability of receiving aid compared to regions with a low probability of receiving aid for reasons unrelated to aid. Reserves and input production are arguably correlated with a large number of other variables, and some of them might differentially affect health in regions that are frequent or infrequent recipients of aid. Most plausibly, the availability of reserves could be correlated with the worldwide business cycle. Potentially, frequent recipients of Chinese aid are also more severely affected by recessions. This could imply that any

[31] As Bluhm et al. (2021) point out, identification in our setting can be based on exogenous variation in the time-series shocks, as shown by Borusyak et al. (2021), to the extent that the covariance between the time-varying components of the instruments and a weighted average of the within location variation over time in unobserved factors that also affect the dependent variables approaches zero in large samples. We return to this in section 1.5.

differential effects of aid on health that we observe result from the world business cycle rather than aid. Chinese aid inputs could also be correlated with China's exports and foreign direct investment, and the probability of receiving aid might be correlated with the probability to trade or receive investments. To address these concerns, we control for yearly worldwide GDP growth as well as Chinese FDI or exports to a country—all interacted with the probability to receive non-concessional funding—in robustness tests below.

Figure 1.3 Testing Spurious Trends: China



Notes: The first panel shows the first principal factor of China's (logged and detrended) production of aluminum (in 10,000 tons), cement (in 10,000 tons), glass (in 10,000 weight cases), iron (in 10,000 tons), steel (in 10,000 tons), and timber (in 10,000 cubic meters) over time. It also shows the (detrended) change in net foreign exchange reserves (in trillions of constant 2010 US dollars). The second panel shows the average number of Chinese health projects within the group that is below the median of the probability of receiving projects and the group that is above the median (conditional on receiving any project). The lower panel shows the infant mortality rate within these two groups.

1.3 Results

Table 1.1 shows the main results of how Chinese (health) aid affects infant mortality. According to the difference-in-differences results of column 1, infant mortality is lower in regions with active Chinese health projects, on average, though with an insignificant coefficient. However, infant mortality is also lower in regions with inactive projects, pointing to selection bias with respect to regions that are not selected to receive projects at all. Regions with an active health aid project experience lower infant mortality compared to regions without projects—the same, however, holds for regions with inactive projects. As can be seen at the bottom of the table, while the coefficient is larger for inactive projects compared to active ones, the difference between the two is not statistically significant at conventional levels.

In column 2, we turn to the (lagged) number of Chinese health projects committed rather than the binary indicators. The OLS results in Panel A show that the (conditional) correlation between health aid and infant mortality is positive but imprecisely estimated. Panel B turns to our instrumental variables results. As can be seen, the coefficient of aid stays positive and is estimated more precisely. The coefficient implies that an additional health project increases the infant mortality rate in regions receiving it by more than eight children in 1,000 children born alive, at the five-percent level of significance. The first-stage F-statistic (shown at the bottom of the table) indicates the power of our instruments.[32]

Columns 3 and 4 turn to the country-level for comparison, including all projects that can be attributed to a sub-national location to facilitate comparability. Focusing on all countries with available data, the results of column 3 show that Chinese health aid reduces infant mortality at the country-level, at the one-percent level, and with a coefficient that exceeds that obtained at local scales by about 50 percent in magnitude. To rule out that these contrasting results arise from differences in the countries and projects included in the local and country-level samples, we replicate the analysis focusing on those countries and projects that are included in the sub-national analysis of column 2. The result remains similar though the coefficient is estimated less precisely (p-value=0.103, see column 4). Specifically, we find that an additional aid project from China reduces infant mortality by about 10 children for every 1,000 live births.[33]

In columns 5 to 8, we replicate the analyses focusing on all aid from China rather than just health aid. To the extent that better infrastructure makes hospitals more accessible or better education improves the quality of health staff, aid broadly is the better measure to capture the effects of aid on health. When aid is fungible not just within but also between sectors, focusing on all aid is preferable to investigating the effects of specific types of aid, as the labels given to aid would then be irrelevant. If fungibility is limited, to the contrary, focusing on all aid rather than health aid could be expected to decrease the magnitude and precision of our estimates. The results are overall similar, the exception being an insignificant (but positive) coefficient in the subnational 2SLS regression of column 6. All three coefficients are smaller in magnitude,

[32] These results stand to some extent in contrast to those of Martorano et al. (2020), who find that Chinese aid improves household health, wealth and education levels. They focus on only 13 countries in Sub-Sahara Africa and do not employ an instrumental variable though, limiting comparability.

[33] Note that while the first-stage F-statistic is substantially lower compared to the sub-national analysis, the test has sufficient power in the smaller, more comparable, sample.

in line with an environment of limited fungibility across sectors.

Table 1.1 Chinese Aid and Infant Mortality, 2002-2014

	Health Aid				*All Aid*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: OLS				
Chinese projects (t-2)		0.625	-0.432*	-0.019		0.122	-0.373**	-0.222
		(1.047)	(0.261)	(0.236)		(0.401)	(0.154)	(0.157)
China active	-1.365				1.808			
	(2.297)				(1.649)			
China inactive	-2.980				0.759			
	(2.422)				(1.514)			
				Panel B: Second Stage				
Chinese projects (t-2)		8.090**	-16.074***	-10.888		1.893	-5.789***	-7.660***
		(3.811)	(5.480)	(6.564)		(1.317)	(1.431)	(2.793)
				Panel C: First Stage				
Input (t-3) x Prob.		0.086	0.233	0.470		1.006***	-0.031	-0.158
		(0.182)	(0.330)	(0.497)		(0.154)	(0.220)	(0.345)
Reserves (t-3) x Prob.		7.342***	1.779	0.233		2.453**	5.372***	5.742**
		(1.242)	(2.450)	(4.022)		(1.079)	(1.661)	(2.784)
Number of countries	52	52	152	53	52	52	152	53
Number of observations	398,228	367,410	1,866	579	398,228	367,410	1,866	579
Sample	local	local	all countries	DHS countries	local	local	all countries	DHS countries
China: Difference active-inactive	1.615				1.049			
China: active = inactive (p-value)	0.372				0.356			
Adjusted R-squared	0.04	0.04	0.94	0.93	0.04	0.04	0.94	0.82
Kleibergen-Paap F		100.7	3.49	11.89		180.1	17.31	5.81

Notes: All regressions in Panels B and C control for (log) population. Columns 1, 2 and 5, 6 include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 3, 4 and 7, 8 include country and year fixed effects. Standard errors are clustered at the country level. DHS countries are those that are also included in the local analyses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

The stark differences in results we obtain at the local- compared to the country-level can potentially be explained with aid being fungible within the health sector. The previous literature is mixed in this regard. For example, Van de Sijpe (2013) finds that health aid is fungible to a limited degree, while Dykstra et al. (2019) find fungibility to be substantial. In the context of our paper, fungibility has a number of observable implications. Some refer to health outcomes, others to inputs. In the following tables, we test several such implications, focusing on health aid to reduce clutter.[34] However, we would like to stress from the outset that the results we obtain are overall similar for all aid from China.[35]

Table 1.2 tests potential transmission channels, focusing on our local-level 2SLS estimates. We investigate the effect of aid on the number of births that are attended by (i) skilled or (ii) traditional health staff. To the extent that aid-financed projects focus on areas less directly related to infant mortality and recipient governments or other donors provide fewer services in return to Chinese largesse, fewer births should be attended by skilled health staff. We would then expect births to be attended by traditional birth attendants instead (which are less likely to be poached or replaced by Chinese aid projects). We further investigate how deliveries develop due to aid at public hospitals compared to private hospitals. Again we would expect fungibility to reduce the number of deliveries in public hospitals, but not necessarily in private ones. This is because private hospitals, on average, pay higher wages compared to public ones (McCoy et al., 2008). What is more, they are arguably less likely to withdraw their services from a certain area as a consequence of foreign aid, given that the aid is granted on the governments' request and thus in those areas the governments would likely have prioritized their own spending too. We further explore whether fewer births might have been delivered in hospitals because respondents to the DHS surveys in our sample consider them to be too far, in order to—indirectly—assess if the capacity to provide health services has been overall reduced by the decline in the number of health facilities.[36] On average, one would expect that places with fewer health facilities experience a decrease of their health indicators as less trained staff and fewer proper tools would be available to attend to overall disease. In concert, these tests of availability and uptake thus provide evidence on whether the availability of hospitals is affected by aid, or rather that aid affects the uptake of services for a given supply of hospitals, which would suggest that donor-financed hospitals substitute government-financed ones, potentially specializing on the provision of different services. We also test this suggestion more closely by investigating whether aid improves outcomes related to malaria, which is a focus area of Chinese health-related aid, as suggested by Table 1.D.4 in Appendix 1.D. The latter table shows that malaria-related interventions are the bulk of Chinese health projects among projects with a specified illness-related purpose. Namely, Malaria-related projects concentrate 14.29% of the total health aid committed worldwide—that

[34] In our sample, projects explicitly described as health aid represent a bit more than 10% of all aid projects.

[35] Detailed results are available on request.

[36] The exact question we rely on is "Q: Why didn't you deliver in a health facility? R: TOO FAR."

share being, at least, more than five times greater than the share assigned to any other specific illness.[37] Finally, we focus on the number of new hires at existing hospitals, as well as the staff's level of education. To the extent that poaching is relevant for our results, staff turnover should increase, while the quality of staff at existing hospitals should decline, with the most qualified staff being poached first.

Column 1 of Table 1.2 shows that an additional Chinese health project reduces the number of births attended by skilled health staff by more than 60 in 1,000 live births delivered, at the one-percent level of significance. In column 2 we focus on the number of births attended by traditional health workers. As can be seen, the coefficient is positive and substantial (indicating that an additional project increases the number of births attended by almost 107 in 1,000 live births delivered). Columns 3 and 4 test how China's health aid affects the availability of public and private health clinics, using a sub-set of the data that allows testing whether private and public health facilities settle close to Chinese aid projects. Columns 5 and 6 focus on the number of deliveries at public and private clinics; column 7 tests whether having a Chinese project nearby affects survey respondents' perception of how distant they are to a health facility (i.e., whether they respond they did not deliver at a hospital because they consider it too far). According to the results, the availability of clinics is not affected by aid. On the contrary, Chinese aid reduces the number of deliveries at public health clinics, at the one-percent level of significance. While it also reduces the number of deliveries at private health clinics, the coefficient is less than one-fourth of those for public clinics (and the difference is statistically significant, with a p-value of 0.002). Taking these results at face value, they are in line with the suggestion that the services provided in aid-financed clinics differ from government-financed ones: Total availability of hospitals does neither increase nor decrease as a consequence of aid, while uptake for delivery declines and hospitals are perceived to be more distant. We test the potential shift in focus within the health sector as a consequence of aid more specifically in column 8, where we find that aid increases the probability that women took anti-malaria pills during pregnancy, at the one-percent level of significance. Though the increased use of anti-malaria pills should reduce rather than increase infant mortality, this positive effect seems to be dominated by shifts in other services provided by hospitals.[38]

[37] Also see Anshan (2011).

[38] As can be seen in Appendix 1.D, the fight against Malaria stands first among China's health projects targeted at specific diseases, with the second-largest share—the fight against HIV/AIDS, Ebola and Tuberculosis combined—receiving less than three percent of the projects. Unsurprisingly, we consequently do not find a significant effect of China's projects on the number of people ever tested for HIV/AIDS (specific results available on request).

Table 1.2 Transmission Channels at the Local Level

	(1) **Assisted births skilled**	(2) **Assisted births traditional**	(3) **Public facility availability**	(4) **Private facility availability**	(5) **Public hospital uptake**	(6) **Private hospital uptake**	(7) **Hospital too far**	(8) **Antimalarial medication**	(9) **Staff hires**	(10) **Staff education**
Panel A: OLS										
Chinese projects (t-2)	2.092	2.281	17.665	0.624	-1.354	-0.860	-0.008	3.503***	0.032	-0.134***
	(3.135)	(2.552)	(12.817)	(0.565)	(3.073)	(2.410)	(0.012)	(0.917)	(0.024)	(0.037)
Panel B: Second Stage										
Chinese projects (t-2)	-62.285***	106.559***	40.413	0.847	-70.695***	-17.209**	0.064*	9.266***	0.373***	-1.088***
	(10.472)	(12.968)	(52.549)	(1.204)	(13.181)	(8.507)	(0.038)	(2.964)	(0.092)	(0.209)
Panel C: First Stage										
Factor (t-3) x Prob.	0.116	0.085	4.850***	4.850***	0.088	0.088	2.196***	0.040	-0.057	0.045
	(0.290)	(0.281)	(1.276)	(1.276)	(0.279)	(0.279)	(0.425)	(0.190)	(0.118)	(0.142)
Reserves (t-3) x Prob.	8.563***	8.598***	13.071**	13.071**	8.622***	8.622***	-18.643***	7.303***	3.804***	3.034***
	(1.873)	(1.822)	(5.788)	(5.788)	(1.810)	(1.810)	(3.065)	(1.306)	(0.858)	(1.031)
Number of countries	51	51	27	27	51	51	25	33	7	7
Number of observations	191,948	201,327	9,907	9,907	208,079	208,079	34,634	236,131	100,822	70,379
Adjusted R-squared	0.54	0.44	0.95	0.88	0.43	0.36	0.38	0.38	0.15	0.99
Kleibergen-Paap F	79.85	79.91	7.54	7.54	81.88	81.88	19.39	89.66	84.26	49.77

Notes: All columns include (log) population, the probability to receive aid, and country-year fixed effects. Columns 1 to 10 include ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 11 and 12 include facility fixed effects. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

We conclude this table investigating staff turnover and educational quality. Columns 9 and 10 focus on staff in health facilities close to Chinese health-aid projects. According to the results, health facilities hired significantly more workers after health aid projects were committed nearby. This could either indicate that the aid was used to expand existing facilities that, in turn, require more workers or that existing workers are replaced, potentially as a consequence of poaching. According to column 10, the average years of education of interviewed staff already employed in the facility declines by almost 1.1 years due to staff with fewer years of education being hired as a consequence of aid committed two years earlier. In concert, these results thus suggest that new staff is hired as a result of aid, likely replacing staff poached by aid-financed projects.[39]

In summary—while not providing bullet-proof evidence—the results of Table 1.2 are in line with the fungibility hypothesis. In Table 1.3 we investigate channels that we expect to facilitate the fungibility of aid. First, we expect fungibility to be more substantial if government expenditures on health are larger when aid is given. Larger expenditures give governments more leeway to reallocate resources, either from the health budget to other areas of government spending or to alternative health projects in different areas of the country. In column 1 we, therefore, interact the number of Chinese health projects with the recipient government's health spending (relative to all government expenditures) a year before the aid project was received. While the level of health expenditures itself is captured by country-year fixed effects, we can still test whether and to what extent larger health budgets make foreign aid less effective in increasing the number of skilled health staff.[40] As can be seen, the coefficient of the interaction is negative and significant at the five-percent level. In tandem with the positive coefficient of aid projects, its magnitude implies that the number of deliveries attended by skilled health staff increases with aid to a larger extent when health expenditure shares are smaller.

Along similar lines, we would expect Chinese aid to be more fungible when recipient countries receive more aid from all of their donors combined. Absent Chinese funding, the very same project might have received funding from another donor, with potentially differential effectiveness. We would ideally like to test whether Chinese aid projects crowd out aid from other donors at the local-level. Given that we do not have geocoded data for most other donors, we instead interact the number of Chinese health projects committed to a specific area with the total health-related aid received from other donors in the previous year (relative to GNI). Given that we have geocoded data for the World Bank we also directly test whether Chinese aid crowds out World Bank aid at the

[39]While this result is in line with fungibility, the alternative explanation that the aid is effective in increasing the supply of hospital services so that more staff with fewer years of education are hired cannot be ruled out based on these regressions alone. The previous regressions show however no evidence for this.

[40]We instrument aid and the interaction of aid and health expenditures with our previous instrument, as well as the interaction of this instrument with health expenditures. We follow Nunn and Qian (2014) and include the interaction of health expenditures interacted with the probability of receiving aid as an additional instrument. Results in the regression reported here are robust to excluding the interaction.

Table 1.3 Chinese Health Aid and Other Resources

	(1) Assisted births skilled	(2) Assisted births skilled	(3) World Bank projects	(4) World Bank projects
Chinese projects (t-2)	-25.961	-36.030***	-0.070	
	(19.263)	(13.895)	(0.118)	
Health exp./Gov. exp. x Chinese projects	-4.064**			
	(2.063)			
Other aid/GNI x Chinese projects		-849.013*		
		(471.046)		
Chinese projects (t-1)				-0.352**
				(0.161)
Number of countries	49	51	52	52
Number of observations	163,364	191,948	367,410	406,678
Adjusted R-squared	0.55	0.54	0.62	0.55
Kleibergen-Paap F	21.19	23.51	77.97	77.53

Notes: All columns include (log) population, the probability to receive aid, country-year fixed effects, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

local-level.[41]

The results show that the (negative) effect of Chinese health projects on the number of births assisted by skilled staff gets stronger at times a recipient country received more aid from other donors (column 2). This is in line with the idea that Chinese aid is more likely to have replaced a project from another donor if such aid was given in abundance, and the project that has been replaced by China was targeted at assisted births to a larger extent.[42]

Column 3 shows that there is no significant effect of Chinese health aid on World Bank health aid two years after commitment. However, the coefficient is negative and significant at the five-percent level one year after aid from China is committed (see column 4). It thus seems that the World Bank commits less aid to areas that received Chinese aid. To the extent that World Bank projects are more effective in reducing infant mortality than Chinese aid (a question to which we turn to below), such fungibility can explain our key results in Table 1.1 above.

[41] The analysis thus also contributes to the literature investigating the extent to which donors engage in competition to aid from China or others (see Mascarenhas and Sandler, 2006; Humphrey and Michaelowa, 2019; Asmus et al., 2020; Fuchs et al., 2020, and Zeitz, 2021).

[42] We use the interaction of Chinese aid projects and aid from other donors as a second instrument. We again follow Nunn and Qian (2014) and include the interaction of aid from other donors interacted with the probability of receiving aid as an additional instrument. Results in the regression reported here are not robust to excluding the interaction as an instrument however and should thus be taken with caution.

1.4 Comparison to the World Bank

This section replicates the main analyses focusing on the World Bank. Our data consist of 2,065 geo-coded World Bank health-related projects spreading over 51 recipient countries that have been approved over the 2002-2014 period.

Our instruments are similar to those for China. We proxy available liquidity with two separate indicators for the World Bank's International Development Association (IDA) and the International Bank for Reconstruction and Development (IBRD). We proxy the availability of IDA aid in a specific location with the interaction of an indicator for the resources that the IDA has available for its lending and the regional probability to receive IDA aid. We obtain the IDA's "funding position" from Dreher et al. (2021b), defined by the World Bank as "the extent to which IDA can commit to new financing of loans, grants and guarantees given its financial position at any point in time and whether there are sufficient resources to meet undisbursed commitments of loans and grants" (IDA 2015: 24).[43] This indicator is disclosed in the World Bank's annual financial statements since 2008; we rely on Dreher et al.'s (2021b) calculations for earlier years in our sample. In order to measure the availability of IBRD resources, Dreher et al. (2021b) use its equity-to-loans ratio, which is reported in the IBRD's annual financial statements. The equity-to-loans ratio is a measure of the IBRD's "ability to issue loans without calling its callable capital" (Bulow, 2002, p. 245). Our second instrument for World Bank projects is the interaction of IBRD liquidity with a region's probability to receive aid.

Note that it is not a priori obvious whether increases in liquidity increase World Bank lending at the intensive or extensive margin. While previous work has shown that an increase in resources leads to larger aid volumes for existing recipients of bilateral aid (Werker et al., 2009; Dreher and Langlotz, 2020), Lang (2020) finds that the International Monetary Fund uses the availability of additional resources to extend its lending beyond its traditional recipients. This also holds true for the World Bank in Dreher et al. (2021b).[44]

We replicate our main regressions for the World Bank in Table 1.4, focusing on the number of committed health projects in a particular cluster or country and year. As can be seen, the results stand in some contrast to those obtained for China above. Column 1 shows that World Bank aid reduces rather than increases infant mortality at the local-level, with a first-stage F-statistic above 17 indicating the power of the instruments.[45] When we turn to the country-level in column 2, the power of the instruments is

[43] The approach of Dreher et al. (2021b) follows Lang (2020), who suggests the IMF's liquidity ratio interacted with the probability of a country to be under an IMF program as an instrument for IMF loans.

[44] For approaches that mirror ours in instrumenting World Bank aid, see Gehring et al. (2019) and Jensen et al. (2020). See Figure 1.A.2 in the Appendix for a graphical depiction of (potentially spurious) trends.

[45] Note that this result is less robust than the comparable result for China above. For example, when we lag aid by three rather than two years, the coefficient turns insignificant. The same holds when we focus on the extended sample starting in 1995, for aid lagged by two as well as by three years.

considerably lower. While the coefficient of aid is positive and marginally significant in the full sample of countries with data available, the first-stage F-statistic is below four. When we restrict the sample to those countries included in the sub-national analysis, the coefficient of aid turns insignificant (though with a positive coefficient, see column 3). In column 4 we make use of the additional years that geocoded data are available for the World Bank—extending the sample to the 1997-2014 period—which leads to a considerably lower first-stage F-statistic. Either way, the negative coefficient at the local-level in concert with a positive or insignificant coefficient at the country-level does not provide evidence in line with the fungibility hypothesis.

The further columns of Table 1.4 replicate the regressions of Table 1.2 for World Bank health projects. The results show that there is no significant effect of aid on the number of births attended by skilled staff. In contrast, the number of births attended by traditional health staff decreases rather than increases as a consequence of aid. The availability of health facilities is not significantly affected by aid; however, uptake of public (but not private) hospitals increases with aid. There is no significant effect on women responding that they did not deliver children in a health clinic because it is too far, and World Bank health aid reduces rather than increases the probability that women took anti-malaria pills during pregnancy. There is no significant effect of aid on the number of hires at health clinics, though—in line with the result for China—average years of education of staff that has already been employed before the aid was committed declines. On balance, few of these results are as one would expect in support of the fungibility hypothesis.

Table 1.4 World Bank Aid and Health Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Infant mortality	**Infant mortality**	**Infant mortality**	**Infant mortality**	**Assisted births skilled**	**Assisted births traditional**	**Public facility availability**
				Panel A: Second Stage			
WB projects (t-2)	-7.903*	28.394**	21.027	56.596	7.231	-105.769***	33.313
	(4.150)	(13.521)	(12.907)	(45.447)	(18.552)	(39.142)	(20.421)
				Panel B: First Stage			
IDA (t-3) x Prob.	0.173****	0.032**	0.073***	0.016	0.155***	0.138***	0.557*
	(0.050)	(0.014)	(0.020)	(0.013)	(0.050)	(0.045)	(0.290)
IBRD (t-3) x Prob.	-0.627***	0.077	0.127	0.127*	0.070	0.055	.
	(0.131)	(0.058)	(0.092)	(0.075)	(0.089)	(0.096)	.
Number of countries	52	152	53	54	51	51	27
Number of observations	367,410	1,866	579	846	191,948	201,327	9,907
Sample	local	all countries	DHS countries	DHS countries	local	local	local
Period	2002-2014	2002-2014	2002-2014	1997-2014	2002-2014	2002-2014	2002-2014
Adjusted R-squared	0.03	0.92	0.92	0.60	0.55	0.41	0.94
Kleibergen-Paap F	17.50	3.35	6.67	1.60	5.16	4.95	4.77

continued on next page

Table 1.4 (Continued)

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Private facility availability	**Public hospital uptake**	**Private hospital uptake**	**Hospital too far**	**Antimalarial medication**	**Staff hires**	**Staff education**
			Panel A: Second Stage				
WB projects (t-2)	-0.406	95.302**	2.244	-0.027	-15.721***	0.180	-2.359***
	(1.643)	(39.691)	(20.500)	(0.079)	(5.080)	(0.282)	(0.774)
			Panel B: First Stage				
IDA (t-3) x Prob.	0.557*	0.138***	0.138***	0.108***	0.211***	-0.026***	-0.033***
	(0.290)	(0.045)	(0.045)	(0.033)	(0.057)	(0.007)	(0.008)
IBRD (t-3) x Prob.	.	0.045	0.045	0.753	0.250**	.	.
	.	(0.089)	(0.089)	(0.662)	(0.108)	.	.
Number of countries	27	51	51	25	33	7	7
Number of observations	9,907	208,079	208,079	34,634	236,131	100,822	70,379
Sample	local	local	local	local	local	local	local
Period	2002-2014	2002-2014	2002-2014	2002-2014	2002-2014	2002-2014	2002-2014
Adjusted R-squared	0.87	0.41	0.36	0.38	0.37	0.11	0.94
Kleibergen-Paap F	4.77	4.92	4.92	5.99	9.63	14.20	17.75

Notes: All regressions control for (log) population; the local-level regressions control for the probability to receive aid. Columns 1 and 5-12 include country-year fixed effects and and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 2, 3 and 4 include country and year fixed effects, with standard errors clustered at the country level. Columns 13 and 14 include facility and country-year fixed effects. DHS countries are those that are also included in the local analyses. Columns 7, 8, 13, and 14 include no IBRD projects; the equity-to-loans ratio-IV is consequently omitted as instrument. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

1.5 Alternative Explanation and Robustness Tests

In Table 1.5 we return to Chinese health projects and investigate an alternative explanation for our results. As one possibility, Chinese health projects might affect the composition of mothers in our sample. To the extent that aid increases the probability that families with an ex-ante higher risk of infant mortality have more children, infant mortality would increase, on average. Columns 1-7 of Table 1.5 therefore test whether health projects affect the fertility of mothers differentially according to their education, age, and (minority or majority) ethnicity. We find that Chinese health aid does significantly affect the fertility of educated but not uneducated mothers (defined as their highest educational level being above/below the location-specific median). On the contrary, we find that the age-composition of mothers is not affected by aid: the fertility of young and older women (those below 20 years and above 29 years, respectively), as well as the fertility by women aged between 20–29 years, remain unchanged. We also find that aid increases the fertility of women belonging to an ethnic minority while it decreases those of women belonging to their location's major ethnicity.[46] While aid thus seems to change the composition of mothers, potentially affecting infant mortality, on average, such changes in composition do not affect our results. As columns 8 and 9 show, results are hardly changed when we control for the number of children (multiplied by 1,000) born to mothers belonging to the ethnic minority or to educated mothers, respectively. We can thus rule out that changes in the composition of mothers in terms of education, age, or ethnicity are key drivers of our results.

[46] A location's major ethnicity is defined as the modal ethnicity of a DHS-cluster. On the political economy of health aid allocation on sub-national scales see Widmer and Zurlinden (2019).

Table 1.5 Composition of Mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Fertility uneducated	**Fertility educated**	**Fertility young**	**Fertility middle**	**Fertility older**	**Fertility ethnic minority**	**Fertility ethnic majority**	**Infant mortality**	**Infant mortality**
Chinese projects (t-2)	0.016	-0.095*	-22.731	25.212	-47.472	279.609***	-402.302***	8.619**	9.678*
	(0.018)	(0.057)	(18.976)	(41.029)	(29.365)	(46.153)	(80.364)	(4.115)	(5.157)
Children educated mothers								1.614***	
								(0.122)	
Children ethnic minority mothers									0.001***
									(0.000)
Number of countries	52	52	52	52	52	29	29	52	29
Number of observations	409,031	409,031	409,031	409,031	409,031	233,463	233,463	409,031	233,463
Adjusted R-squared	0.164	0.433	0.217	0.267	0.263	0.329	0.427	0.036	0.042
Kleibergen-Paap F	113.5	113.5	113.5	113.5	113.5	70.42	70.42	113.5	70.46

Notes: All regressions control for (log) population and include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. DHS countries are those that are also included in the local analyses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

We explore more alternative explanations. In columns 1 to 3 of Table 1.D.3—in Appendix 1.D—we assess how Chinese aid might impact mothers' trust (in health facilities) and wealth (proxied by night lights). Knutsen and Kotsadam (2020) argue that the allocation of aid might increase people's levels of trust. To the extent that the arrival of aid projects may systematically affect trust of women in health facilities and therefore the quality of their children's health, infant mortality can increase on average. Similarly, Colleran and Snopkowski (2018) show that for many countries in our sample, the increase of wealth can have a positive impact on the number of births among less educated women. Thus, if Chinese aid does positively impact levels of subnational wealth, one can expect an overall increase of infant mortality, given that women with a potential higher risk of losing their children would have more children if the effect of education dominates that of wealth. In the other half of the table (columns 4 to 6), we look into key institutional heterogeneities that might explain the worsening of children's health service and, therefore, the increase of infant mortality. Isaksson and Kotsadam (2018) show that Chinese aid fuels corruption in the vicinity of the places where it goes. In turn, such corruption might deteriorate the overall quality of health services as, especially, public facilities might systematically receive less governmental support as part of that support might serve as bribes. With the lessened quality of health service, one should expect an increase of infant mortality, as well. We delve, however, into more direct indicators of institutional quality. To the extent that better public administration in general, or better management of public funds in particular, may improve the distribution of aid subnationally (e.g., via better information and decision-making), we assess the effects of public administration quality.[47] The results in Table 1.D.3 show that neither institutional factors mediate the effect of aid on infant mortality, nor that trust is associated with Chinese aid. While the results do show a positive impact of Chinese projects on wealth, once directly controlling for night lights (wealth variable), the main result of Table 1.1 (column 2) remains unchanged.

We next test the timing after which aid commitments affect outcomes in some detail. We focus on the key regressions reported in columns 2–4 of Table 1.1, testing how aid affects infant mortality sub-nationally, at the country-level in the full sample, and at the country-level in the sample restricted to those countries that are also included in the sub-national analysis ("DHS countries"). To reduce clutter, we only report the coefficients and standard errors of the variable of interest, in concert with the number of countries and observations as well as first-stage F-statistics.

While the previous analysis considered the effect of aid two years after commitment, shorter or longer delays with which aid affects health outcomes are well possible. Unfortunately, while we are confident that the exclusion restriction for our instrument holds

[47] The results show that these variables do not explain heterogeneous effects of aid on infant mortality, suggesting a priori, that the impact of Chinese aid is negative regardless of the quality of the public administration. This, however, can also be explained by the relatively low administration quality in the countries under study overall, as the majority (75%) of the country sample report an index below 3 (mid- to low-quality public administration). Note as well, that the sample is significantly reduced due to many missing values in the quality-related data.

with respect to variables unrelated to aid, the same does not hold for aid given from the same donor in different years. To some extent, aid is concentrated on the same regions for prolonged periods of time. Given that our instruments are also correlated over time (for example, the correlation of our instruments with their values one year earlier exceeds 0.8 for both of them), the effect of aid in any year that we detect with our instrumental variables strategy can partly also reflect the effect of aid in earlier or future years. To rule this out, we ran regressions that included up to four lags and—as placebo variables—two leads of the aid variable, instrumented with the respective lags of our instruments. Unfortunately, the power of the instruments is very low in these regressions, so we do not report them here.

Table 1.6 instead shows results of individual regressions, where we include one lag of aid at a time and instrument aid with our instrumental variables (lagged by one additional year). As can be seen, our previous results hold in the year of commitment already. The magnitude of the coefficients declines after the second year—corresponding to the lag structure in the main analyses of this paper. At sub-national scales, the effect of Chinese health aid turns insignificant commencing with the third lag. At the country-level, aid remains significant until the first lag in the smaller sample and is significant for all lags in the larger sample. Given that aid projects and the instruments are correlated over time, and the lack of power in the first stage does not allow us to include more than one aid variable at the same time, it is however not possible to differentiate the effects of different lags in a bullet-proof way. For what it is worth, the final rows of Table 1.6 report results for the (instrumented) second lag, where we include different leads and lags of aid without instrumenting for them. Our results are again highly similar.

We conclude this section by testing the robustness of our key results along various dimensions. First, we measure Chinese health aid as either (logged) commitment amounts or a binary project indicator rather than project numbers.[48] Second, we report results using our two instruments separately rather than jointly. Third, we average our data over three-year periods. Fourth, we focus on sub-national clusters within a 111 km radius rather than 55 km. Fifth, we control for all variables that we did also include in the basic regressions in column 1 of Table 1.1 above, but omitted from the instrumental variables regressions. Sixth, we examine—in three individual regressions for each test of robustness—whether our results hold when we control for one of three variables, each interacted with the probability to receive non-concessional aid (OOF): world GDP growth, Chinese exports to a specific recipient country, and Chinese Foreign Direct Investment (FDI) to each recipient country. To the extent that our instruments are correlated with the world business cycle, Chinese exports or Chinese FDI, and at the same time regions that receive more aid are more likely to be affected by shocks related to the business cycle, trade, or FDI, our aid variable might reflect in part the effect of these variables rather than aid itself. Controlling for these variables tests this possibility. We also aggregate all data at the ADM2-level, testing whether aid given to

[48] Note that we have added a value of one before taking logs in order to avoid losing values with zero aid.

ADM2 regions—which are geographic units of sizes in between the local and country areas we have so far focused on—affects infant mortality there. And finally, we test the effect of ODA and OOF (health-related) projects separately rather than including them in one variable.

Table 1.7 shows the results, again focusing on our three key specifications. As can be seen, our results are overall robust to these tests. Though some coefficients turn insignificant in some of the regressions (as one would expect when running a large number of them), all combinations of regressions are in line with fungibility. That is, in cases the local effect of aid is significant in increasing infant mortality, effects at the country-level are either negative and significant or insignificant. At the same time, when we do not find a consistent effect of aid at local scales, aid is effective in reducing mortality at the country-level. Most importantly, we find that the effect of aid on mortality persists when we control for other variables potentially correlated with parts of our instruments—the world business cycle, or Chinese exports and FDI. While this does not rule out that other omitted variables correlated with the input factors into aid projects or changes in China's foreign currency reserves differentially affect health outcomes in regions with different probabilities to receive aid, we do not expect such variables to be consequential for our results given that controlling for the most obvious candidates has little impact.

Interestingly, the effect of aid at the ADM2 level is insignificant, thus being in between the results we obtain at local- and country-levels. Finally, it seems that our results are driven by ODA—i.e., aid in the strict sense—rather than OOF. Note however that approximately 90% of projects in the health sector are coded as ODA so that the estimates for OOF rely on a small number of observations.[49]

[49] When we replicate these results for all OOF projects rather than just health-related ones the coefficient is not statistically significant at the local level, while at the country-level results are significant at least at the five-percent level, with negative coefficients.

Table 1.6 Chinese Aid and Infant Mortality, Different Lags

	(1) Local	(2) All countries	(3) DHS countries
Chinese projects (t)	10.880**	-15.287***	-12.566*
	(4.689)	(5.227)	(6.444)
	77.88/53/458,739	13.70/154/2,026	10.15/53/632
Chinese projects (t-1)	9.713**	-14.665***	-11.957*
	(3.988)	(4.691)	(6.559)
	89.28/52/410,457	6.04/154/2,026	15.51/53/632
Chinese projects (t-2)	8.090**	-16.074***	-10.888
	(3.811)	(5.480)	(6.564)
	100.7/52/367,410	4.27/152/1,866	13.63/53/579
Chinese projects (t-3)	2.479	-14.528***	-9.146
	(4.436)	(5.190)	(6.078)
	79.52/52/322,410	3.76/151/1,714	12.49/52/525
Chinese projects (t-4)	2.470	-12.250***	-7.922
	(5.016)	(4.120)	(5.242)
	50.44/51/278,555	4.88/151/1,562	9.21/51/472
Chinese projects (t-5)	0.538	-9.397***	-4.833
	(6.442)	(3.338)	(4.282)
	31.86/50/237,112	5.75/148/1,409	11.41/50/420
Chinese projects (t-6)	12.177	-9.358***	-2.511
	(10.873)	(3.294)	(3.349)
	26.58/50/197,725	4.42/144/1,257	8.77/50/370
	Controlling for projects in t+1, t, t-1, t-3		
Chinese projects (t-2)	10.311**	-9.643***	-5.477
	(4.021)	(3.682)	(3.366)
	69.65/51/227,744	2.52/151/1,565	8.97/51/472
	Controlling for projects in t, t-1		
Chinese projects (t-2)	9.854***	-13.785***	-8.796
	(3.543)	(4.874)	(5.434)
	92.69/52/336,028	3.44/152/1,866	12.88/53/579

Notes: All regressions control for (log) population. All specifications in column 1 include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. All specifications in columns 2 and 3 include country and year fixed effects, with standard errors clustered at the country level. The last row of each specification reports the following: Kleibergen-Paap F-statistic/number of countries/number of observations. DHS countries are those that are also included in the local analyses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Finally, we turn to the evolving literature on shift-share instruments to which our empirical strategy relates (based on Bluhm et al., 2020). Shift-share instruments are usually constructed as sums of shocks to a variety of industries with varying local exposures. Relying either on the share or the shift, there are two ways to achieve identification in such settings under an alternative set of assumptions. Local industry shares can be interpreted as instruments, provided they are exogenous (Goldsmith-Pinkham et al., 2020). Or, as Borusyak et al. (2021) demonstrate, identification can also be purely based on exogenous variation in the time-series shocks, even when variation in local exposures is endogenous. Contrary to this literature, our setting does not involve many shocks in different industries but the exposure to a single, potentially endogenous, shock. When we convert our data to time-series in line with Borusyak et al. (2021) and run our main specification with either the reserves or input material instruments, all results are similar.[50] Our results are thus not driven by random shocks that are correlated across DHS clusters and that similarly affect areas with comparable probabilities of receiving aid. Nevertheless, rather than trying to convince the reader that at least one of the parts forming our interacted instruments is—unconditionally—exogenous, we rely on the alternative assumptions outlined in section 1.2. We probe these assumptions in the remainder of this section.

In addition to visual inspection of trends in Figure 1.3 we follow Christian and Barrett (2017) in randomizing observations for aid projects and our instrumental variables across (a) space and time, (b) space, (c) time within the same space, and (d) space but independently for each year. As can be seen from the results of 1,000 regressions shown in Figure 1.4, coefficient estimates center around zero. According to an exact Fisher test, the coefficient from our main estimate above (indicated by the dashed vertical line) is significantly different from the randomized coefficients in all four sets of regressions (p-value=0.083 or below). Figure 1.A.3 in the Appendix shows results of analogous regressions at the country-level, with similar results.[51] It is thus unlikely that omitted variables, correlated with our key variables in a similar way across space, drive the result.

[50] At the local-level, the first stages are strong. At the level of countries, the first-stage F-statistics are however lower, ranging between 4.8 and 6.9. We also tried to replicate our results with a time-series and both of our instruments. However, these regressions did not converge.

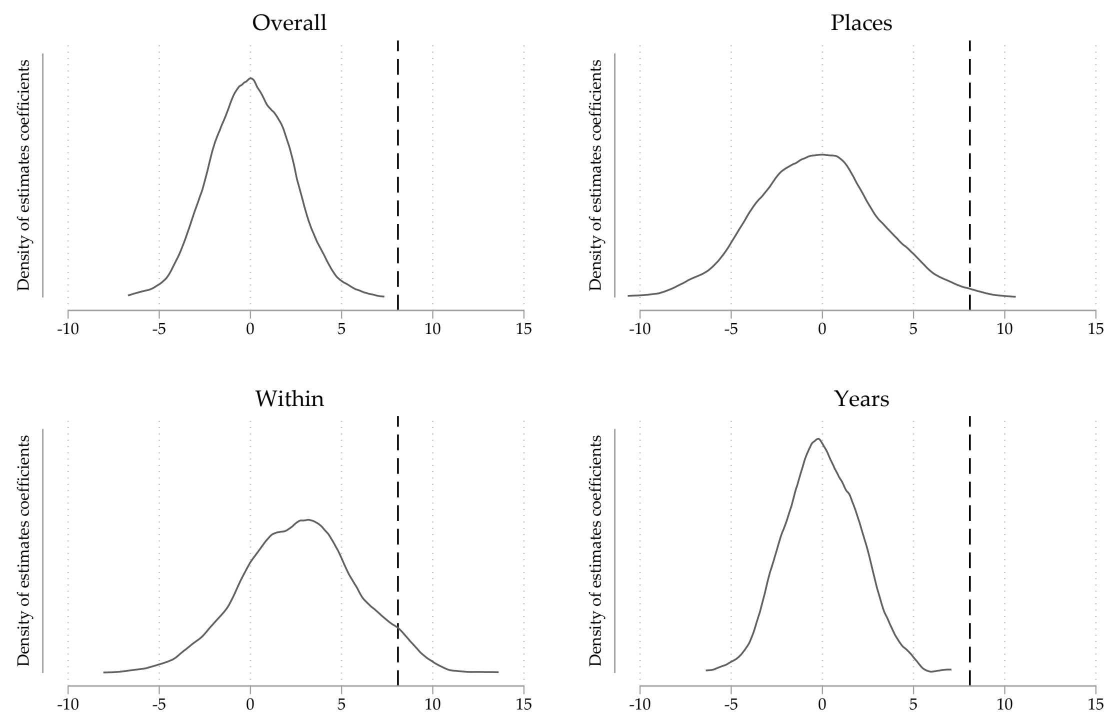
[51] The Fisher test again indicates that the coefficients from our main estimates above are significantly different from the randomized coefficients (p-value=0.004 or below).

Table 1.7 Tests for Robustness

	(1) Local	(2) All countries	(3) DHS countries
(log) Commitment amounts	1.159	-0.828***	-0.431
	(0.724)	(0.316)	(0.374)
	80.76/52/367,410	5.35/152/1,866	10.42/53/579
Health Aid (binary)	13.777**	-19.981***	-14.334*
	(6.906)	(5.866)	(7.976)
	137.00/52/367,410	6.78/152/1,866	12.29/53/579
Input interaction IV	5.027	-15.078***	-10.778*
	(4.321)	(4.667)	(6.412)
	172.10/52/367,410	8.62/152/1,866	20.25/53/579
Reserves interaction IV	8.319**	-16.932***	-12.493
	(3.799)	(6.410)	(9.293)
	201.40/52/367,410	7.23/152/1,866	6.52/53/579
Averages, three years	7.810	-12.149***	-8.416*
	(6.292)	(3.881)	(4.942)
	107.60/50/191,804	4.49/148/1,408	11.09/50/420
111 km	7.894**		
	(3.156)		
	185.2/52/367,410		
Full set of controls	7.335*		
	(3.920)		
	97.23/52/358,183		
World GDP growth (t-3)	8.17**	-16.096***	-10.987
	(3.807)	(5.500)	(6.683)
	100.57/52/367,410	4.26/152/1,866	13.39/53/579
Chinese exports (t-3)	7.532*	-6.641***	-7.938**
	(3.856)	(2.304)	(3.768)
	93.58/52/367,410	3.88/152/1,866	7.70/53/579
Chinese FDI (t-3)	7.166*	-9.637***	-8.812**
	(3.885)	(2.971)	(4.085)
	91.38/52/367,410	6.11/152/1,866	4.93/53/579
ADM2	21.203		
	(16.123)		
	14.25/52/61,737		
ODA projects	7.149*	-15.923***	-10.145
	(4.144)	(5.786)	(6.736)
	83.94/52/367,410	3.30/152/1,866	8.52/53/579
OOF projects	-28.436	1.937	3.333
	(25.899)	(1.431)	(2.312)
	8.73/52/367,410	6.26/152/1,866	5.87/53/579

Notes: All regressions control for (log) population. All specifications in column 1 include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. All specifications in columns 2 and 3 include country and year fixed effects, with standard errors clustered at the country level. The last row of each specification reports the following: Kleibergen-Paap F-statistic/number of countries/number of observations. DHS countries are those that are also included in the local analyses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Figure 1.4 Testing Spurious Trends: China, Local



Notes: The figure shows point estimates of 1,000 regressions at the local-level, while randomly shifting the aid variable and the instruments across a) space and time, b) space, c) time within the same space, and d) space but independently for each year. The dashed vertical line marks the point estimate from Table 1.1, column 2.

Finally, Table 1.8 reports the results of falsification tests. First, we replace China's input materials and reserves with their analogous values in the United States when forming our instruments. If global trends in production inputs and reserves drive our instrument, rather than China's, our results might be spurious. Panel A shows that this results in insignificant coefficients in the second stages and very low power in the first stages. Second, we test whether the change in infant mortality between 1990 and 2000 correlates with (year-to-year) changes in the predicted number of Chinese aid projects between 2000 and 2014 (using the first stages of our regressions). The resulting coefficients (shown in Panel B) are completely insignificant, further indicating the absence of pre-trends in our main dependent variable that correlate with Chinese aid.[52]

Panel C of Table 1.8 allows for correlation of the error term at different levels. To this end, we re-run our analyses clustering standard errors at three different levels—

[52]This analysis was inspired by similar tests in Mayda et al. (2020).

Table 1.8 Tests for Robustness II

	(1) **Local**	(2) **All countries**	(3) **DHS countries**
		Panel A: Placebo	
US input & US reserve IV	-7.09	11.35	-5.14
	(16.384)	(82.82)	(13.26)
	3.74/52/367,410	0.01/152/1,866	0.02/53/579
		Panel B: Pre-Trends	
IM 2000 - IM 1990	-0.04	-0.08	0.03
	(0.15)	(0.05)	(0.06)
	52/246,986	152/1,866	53/579
		Panel C: Clustering	
Probability	8.09***	-16.074***	-10.888*
	(2.31)	(6.188)	(5.770)
	50.07/52/367,410	4.32/152/1,866	20.36/53/579
Year	8.090*	-16.074***	-10.888*
	(4.24)	(4.992)	(5.196)
	13.54/52/367,410	4.79/152/1,866	2.99/53/579
Country	8.090***		
	(2.16)		
	16.34/52/367,410		

Notes: All regressions control for (log) population. All specifications in column 1 include country-year fixed effects and the probability to receive aid (Panels A and C: ADM2 fixed effects in addition). Column 1 clusters standard errors at the ADM2-year level in Panels A and B. All specifications in columns 2 and 3 include year fixed effects (Panels A and C: country-fixed effects in addition). Columns 2 and 3 cluster standard errors at the country level in Panels A and B. Panel C clusters standard errors at the probability-to-receive-aid, year and country level respectively. The last row of each specification in Panels A and C reports the following: Kleibergen-Paap F-statistic/number of countries/number of observations (Panel B: number of countries/number of observations). "DHS countries" are those that are also included in the local analyses. Standard errors are reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

the country-level, the year-level, and the probability-to-receive-aid-level.[53] As can be seen, all coefficients stay significant at conventional levels. Overall, the results in this

[53]To the extent that unobserved shocks in Chinese construction materials or international reserves as parts of our instrumental variables affect areas with a high probability to receive aid more than those with a low probability and therefore result in a correlation of the error term between regions with similar shares, clustering at the probability-to-receive-aid-level would address such correlation (Adao et al., 2019).

section give us confidence that the assumptions underlying our identification strategy—a conditionally exogenous treatment in concert with parallel pre-trends—hold in our setting.

1.6 Conclusions

Recent work in the aid effectiveness literature shifts the unit of analysis from the country to sub-national areas such as provinces or districts or even small grids within these areas. Although this has the advantage of greater statistical power and could facilitate the detection of local effects that are insufficiently strong to be recorded when countries are the unit of analysis, it can also make the aid seem less effective than it is. To the extent that aid is fungible within sectors, and donors finance projects that are less effective compared to government-funded projects they replace, aid can appear harmful at the local-level.

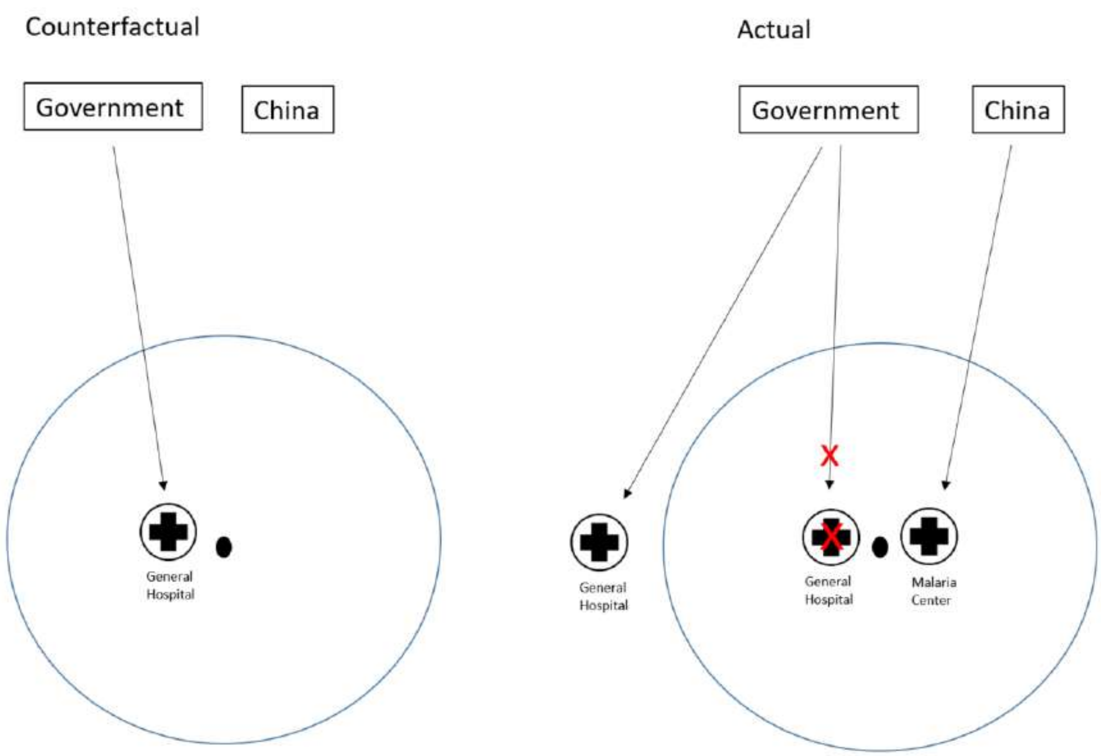
In this paper, we have compared the effects of Chinese health aid at the local-level with those at the country-level. According to our results, China's projects increase infant mortality locally but reduce mortality at the level of countries. We explain this stark contrast with aid being fungible. In particular, we find that aid reduces the number of births attended by skilled staff, while it increases the number of births attended by traditional staff. Though the number of hospitals available is not affected by aid, aid reduces the number of deliveries in public hospitals by more than it does reduce them in private ones. At the same time, mothers are more likely to perceive hospitals as too distant to deliver there as a consequence of aid. We find staff turnover at hospitals to increase and average years of education of pre-existing staff to decline with aid. These findings are consistent with a setting where scarce domestic human capital is poached from existing providers of these services, which are more effective than those replacing them (Deserranno et al., 2020). We also find evidence in line with shifting priorities resulting from aid, documented by increases in the probability that women took anti-malaria pills during pregnancy as a consequence of aid (with the fight against malaria being a major goal of Chinese health operations).

These results bear important implications. First, we find that Chinese health aid, and its aid more broadly, reduce infant mortality overall. In line with several earlier papers (e.g., Dreher et al., 2021b) this adds further evidence against the claim that Chinese aid is unequivocally harmful to its recipients ("Rogue Aid" for a discussion see, e.g., Isaksson and Kotsadam, 2018). Second, our results show that China's aid does not necessarily bring its positive effects to those localities it was delivered to. Quite the contrary, Chinese aid seems to increase infant mortality in the vicinity of aid projects. To the extent that China aims to add to local development in the areas it nominally supports it needs to rethink its non-interference policy, changing its aid allocation process in a way that is more similar to those of the World Bank (for which we did not find evidence in line with the fungibility hypothesis here). And finally, our results should remind researchers that the benefits of sub-national analysis have to be weighed carefully against their drawbacks. Ideally, questions of aid effectiveness should thus be investigated at different levels of analysis, allowing for the possibility that fungibility at sub-national scales makes the aid appear more negative than it actually is.

Appendix

1.A Additional Figures

Figure 1.A.1 The Local Effect of Aid on Infant Mortality with Fungibility

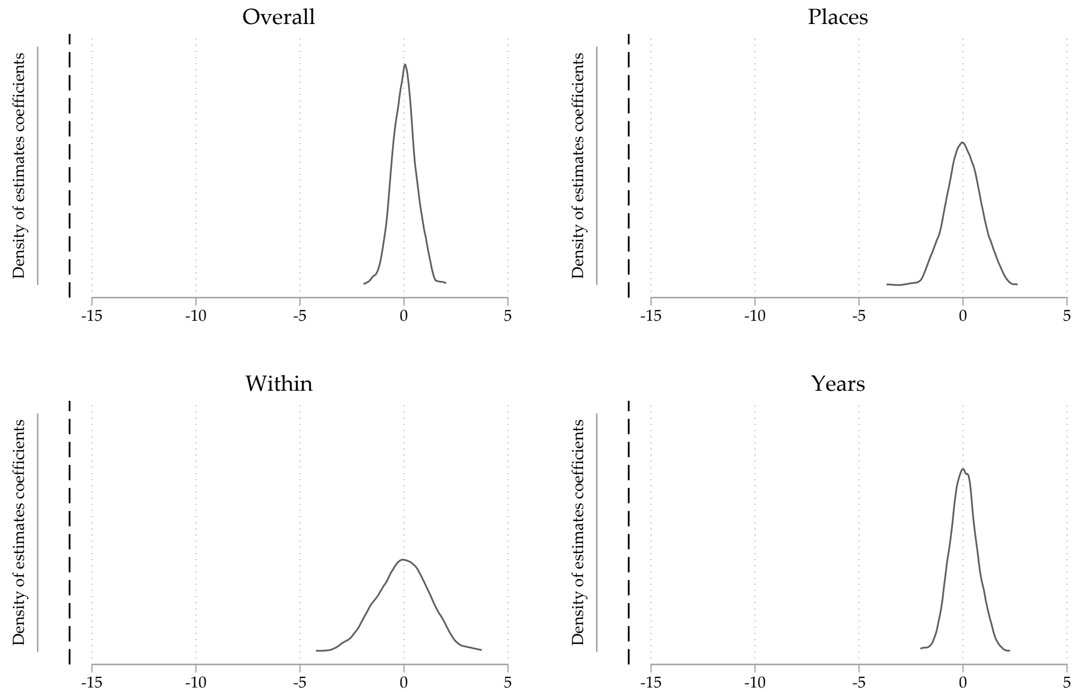


Notes: The figure illustrates fungibility. The figure on the left shows what would have happened without the Chinese health project. The figure on the right shows what happens with China implementing a health project. As can be seen in the figure, the government shifts its resources to another location in order to not spend resources on similar projects in the same location in response to the Chinese health project. Thus, the exemplary location receives a Chinese-funded project instead of a government-financed one. However, since China focuses more on Malaria treatment, the focus of the health project shifted from general to Malaria which can render the effect on infant mortality negative compared to the counterfactual.

Figure 1.A.2 Testing Spurious Trends: World Bank

Notes: The first panel shows the IBRD's equity-to-loans ratio and the IDA's funding position over time. The second panel shows the average number of World Bank health projects within the group that is below the median of the probability of receiving projects and the group that is above the median (conditional on receiving any project). The lower panel shows the infant mortality rate within these two groups.

Figure 1.A.3 Testing Spurious Trends: China, All Countries



Notes: The figure shows point estimates of 1,000 regressions at the country-level, while randomly shifting the aid variable and the instruments across a) space and time, b) space, c) time within the same space, and d) space but independently for each year. The dashed vertical line marks the point estimate from Table 1.1, column 3.

1.B Sources and Definitions

Table 1.B.1 Local Level

Variable	Definition	Data Source
Infant Mortality	Number of children that did not survive the first year out of 1,000 live births in a DHS cluster and year.	DHS (ICF, 2004-2017)
Chinese health projects	Total number of Chinese health-related projects committed within 55km of a DHS cluster.	Bluhm et al. (2020)
Chinese projects, all	Total number of Chinese aid projects committed within 55km of a DHS cluster.	Bluhm et al. (2020)
Chinese health commitments	Total amount of Chinese health-related projects committed within 55km of a DHS cluster.	Bluhm et al. (2020)
Chinese health projects, dummy	Dummy = 1 if at least one Chinese health-related project is committed within 55km of a DHS cluster.	Bluhm et al. (2020)
Chinese health projects (ODA)	Total number of Chinese health-related Official Development Assistance projects committed within 55km of a DHS cluster.	Bluhm et al. (2020)
Chinese health projects (OOF)	Total number of Chinese health-related Other Official Finance projects committed within 55km of a DHS cluster.	Bluhm et al. (2020)
World Bank health projects	Total number of World Bank health-related projects committed within 55km of a DHS cluster.	AidData (2017)
Reserves	China's yearly net currency reserves.	World Bank (2020)
Inputs	China's detrended first factor of yearly production of steel, timber, glass, aluminum, and cement.	National Bureau of Statistics of China (NBSC 2019), United States Geological Survey (USGS 2019)
US Reserves	United States' yearly net currency reserves.	World Bank (2020)
US Inputs	United States' detrended first factor of yearly production of steel, timber, glass, aluminum, and cement.	United States Geological Survey (USGS 2019)
Probability	Share of years (2000-2014) that at least one Chinese health related project is committed within 55km of a DHS cluster.	Own construction based on Bluhm et al. (2020)
Probability (ODA)	Share of years (2000-2014) that at least one Chinese health related ODA-project is committed within 55km of a DHS cluster.	Own construction based on Bluhm et al. (2020)
Probability (OOF)	Share of years (2000-2014) that at least one Chinese health related OOF-project is committed within 55km of a DHS cluster.	Own construction based on Bluhm et al. (2020)
Probability (amounts)	Share of years (2000-2014) that at least one Chinese health related project with known amounts is committed within 55km of a DHS cluster.	Own construction based on Bluhm et al. (2020)
Population (log)	(log) number of people within the 2 km (urban) or 10 km (rural) buffer surrounding a DHS survey cluster location.	Center for International Earth Science Information Network at Columbia University (2016)
Distance to National Borders	Straight-line distance to the nearest international border.	Department of State's Office of the Geographer (2014)
Distance to Protected Areas	Straight-line distance to the nearest protected area.	UNEP-WCMC and IUCN (2017)
Distance to Water	Straight-line distance to the nearest major water body.	Wessel and Smith (1996, 2017)
Travel Time	The average travel time of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points) to reach a settlement of 50,000 or more people in the year 2000.	Nelson (2008)
Slope	The average slope of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points).	Earth Resources Observation and Science Center (1996)

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Table 1.B.1 (continued)

Variable	Definition	Data Source
Nightlights	The yearly average nighttime luminosity of the area within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster location.	National Centers for Environmental Information (2015)
Rainfall	The yearly average rainfall of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points).	Climate Hazards Group (2017)
SMOD Population	Divides classes in 1: rural cells, 2: urban clusters, and 3: urban centres, and take the mode of the Settlement Model (SMOD) of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points).	Pesaresi and Freire (2016)
Vegetation Index	Yearly vegetation index value between 0 (least vegetation) and 10,000 (Most vegetation). Spatial resolution at 0.05 degrees at the equator.	Didan (2016)
Built Population	Built-up index between 0.00 (extremely rural) and 1.00 (extremely urban). Constructed as the average built-up index of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points).	Pesaresi et al. (2015)
Leader Birth Region	Dummy = 1 if the birth region of the country's head of government is within 55km of a DHS cluster.	Hodler and Raschky (2014)
World GDP Growth	Global yearly GDP growth.	World Bank (2020)
Chinese FDI	Outflow of Chinese foreign direct investments to a recipient country.	UNCTAD (2017)
Chinese Exports	Chinese exports to a recipient country.	World Bank (2020)
Assisted births skilled	The number of deliveries with a skilled health worker present out of 1,000 live births in a DHS cluster and year.	DHS (ICF, 2004-2017)
Assisted births traditional	The number of deliveries with a traditional health worker present out of 1,000 live births in a DHS cluster and year.	DHS (ICF, 2004-2017)
Staff hires	The number of new staff employed at a health facility in a year.	Service Provision Assessments, DHS (ICF, 2004-2017)
Staff education	The average years of education of interviewed staff already employed in a facility in a year.	Service Provision Assessments, DHS (ICF, 2004-2017)
Public hospital uptake	The number of deliveries at public clinics out of 1,000 live births in a DHS cluster and year.	DHS (ICF, 2004-2017)
Private hospital uptake	The number of deliveries at private clinics out of 1,000 live births in a DHS cluster and year.	DHS (ICF, 2004-2017)
Antimalaria medication	The probability of women taking anti-malaria pills during pregnancy in a DHS cluster and year.	DHS (ICF, 2004-2017)
Hospital too far	Share of mothers in a DHS cluster and year who did not deliver at a hospital because it was too far.	DHS (ICF, 2004-2017)
Fertility ethnic minority	Total number of births of mothers of an ethnic minority in a DHS cluster and year.	DHS (ICF, 2004-2017)
Fertility ethnic majority	Total number of births of mothers of an ethnic majority in a DHS cluster and year.	DHS (ICF, 2004-2017)
Fertility young	Total number of births of below-20-years-old mothers in a DHS cluster and year.	DHS (ICF, 2004-2017)
Fertility middle	Total number of births of mothers aged between 20 and 29 years old in a DHS cluster and year.	DHS (ICF, 2004-2017)
Fertility old	Total number of births of above-29-years-old mothers in a DHS cluster and year.	DHS (ICF, 2004-2017)
Fertility uneducated	Total number of births of mothers with below median education in a DHS cluster and year.	DHS (ICF, 2004-2017)
Fertility educated	Total number of births of mothers with above median education in a DHS cluster and year.	DHS (ICF, 2004-2017)
Private hospital availability	Number of private hospitals near a Chinese aid project.	Maina et al. (2019)
Public hospital availability	Number of public hospitals near a Chinese aid project.	Maina et al. (2019)
Health exp./Gov. Exp.	Amount of government health expenditure as a share of total government expenditure.	World Bank (2020)
Other aid/GNI	Total amount of other donors' health aid as a share of recipient Gross National Income.	OECD (CRSI 2017) and World Bank (2020)

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Table 1.B.2 Country Level

Variable	Definition	Data Source
Infant Mortality	Number of children that did not survive the first year out of 1,000 live births per country and year.	DHS (ICF, 2004-2017)
Chinese health projects	Total number of Chinese health-related projects committed within a country/year.	Bluhm et al. (2020)
Chinese projects, all	Total number of Chinese projects committed within a country/year.	Bluhm et al. (2020)
Chinese health commitments	Total amount of Chinese health-related project commitments within a country/year.	Bluhm et al. (2020)
Chinese health projects, dummy	Dummy = 1 if at least one Chinese health-related project is committed within country/year.	Bluhm et al. (2020)
Chinese health projects (ODA)	Total number of Chinese health-related Official Development Assistance projects committed within a country/year.	Bluhm et al. (2020)
Chinese health projects (OOF)	Total number of Chinese health-related Other Official Finance projects committed within a country/year.	Bluhm et al. (2020)
World Bank health projects	Total number of World Bank health-related projects committed within a country/year.	AidData (2017)
Reserves	China's yearly net currency reserves.	World Bank (2020)
Inputs	China's detrended first factor of yearly production of steel, timber, glass, aluminum, and cement.	National Bureau of Statistics of China (NBSC 2019), United States Geological Survey (USGS 2019)
Probability	Share of years (2000-2014) that at least one Chinese health related-project is committed in a country.	Bluhm et al. (2020)
Probability (all aid)	Share of years (2000-2014) that at least one Chinese project is committed in a country.	Bluhm et al. (2020)
Probability (ODA)	Share of years (2000-2014) that at least one Chinese health-related ODA-project is committed in a country.	Bluhm et al. (2020)
Probability (OOF)	Share of years (2000-2014) that at least one Chinese health-related OOF-project is committed in a country.	Bluhm et al. (2020)
Probability (amounts)	Share of years (2000-2014) that at least one Chinese health-related project with known amounts is committed in a country.	Bluhm et al. (2020)
Population (log)	The logarithm of population size.	World Bank (2020)
World GDP Growth	Global yearly GDP growth.	World Bank (2020)
Chinese FDI	Outflow of Chinese foreign direct investments in a recipient country.	World Investment Report (2017)
Chinese Exports	Chinese exports to a recipient country.	World Bank (2020)

1.C Descriptive Statistics

Table 1.C.1 Descriptive Statistics

	N	Mean	SD	Min	Max
			Panel A: Local		
Infant Mortality	367,410	47.610	134.592	0.000	1000.000
Chinese health projects	367,410	0.033	0.276	0.000	8.000
Chinese projects, all	367,410	0.215	0.940	0.000	18.000
Chinese health commitments	367,410	137,135	3,089,208	0.000	1.35e+08
Chinese health projects, dummy	367,410	0.021	0.143	0.000	1.000
Chinese health projects (ODA)	363,932	0.026	0.224	0.000	6.000
Chinese health projects (OOF)	363,932	0.000	0.022	0.000	1.000
World Bank health projects	363,932	0.150	1.133	0.000	54.000
Reserves	367,410	0.071	0.142	-0.086	0.326
Inputs	367,410	-0.088	0.976	-1.206	1.583
Probability	367,410	0.025	0.070	0.000	0.600
Probability (ODA)	367,410	0.011	0.044	0.000	0.533
Probability (OOF)	367,410	0.0003	0.004	0.000	0.133
Probability (amounts)	367,410	0.010	0.046	0.000	0.533
Population (log)	367,410	9.795	1.972	-1.533	15.865
Distance to National Borders	367,410	71918.63	78675.56	0.102	594,383.40
Distance to Protected Areas	367,410	63,159.43	57,780.06	0.000	496,119.70
Distance to Water	367,410	130,330.50	140,077.80	0.000	1,054,545
Travel Time	367,399	193.900	244.445	0.900	4,928.180
Slope	366,935	1.650	2.184	0.000	22.804
Nightlights	363,800	2.600	5.965	0.000	56.208
Rainfall	363,996	1,228.448	848.981	0.000	8,735.8
SMOD Population	367,400	0.944	1.175	0.000	3.000
Vegetation Index	363,655	3,052.771	1,065.874	-60.80	6,614.473
Built Population	367,400	0.118	0.233	0.000	1.000
Leader Birth Region	367,410	0.171	0.376	0.000	1.000
World GDP Growth	367,410	3.168	1.369	-1.679	4.403
Chinese FDI	367,410	14,750.61	20,224.88	915.777	74,654.04
Chinese Exports	367,410	6.56e+11	4.68e+11	2.04e+11	2.01e+12
Assisted births skilled	191,948	678.239	380.015	0.000	1,000.000
Assisted births traditional	201,327	265.806	359.328	0.000	1,000.000
Staff hires	100,822	0.246	0.610	0.000	15.000
Staff education	70,379	10.330	6.883	0.000	34.000
Public hospital uptake	208,079	504.529	414.250	0.000	1,000.000
Private hospital uptake	208,079	121.944	270.910	0.000	1,000.000
Antimalaria medication	236,131	55.686	100.411	0.000	1,000.000
Hospital too far	34,634	0.292	0.397	0.000	1.000
Fertility ethnic minority	233,463	842.862	1,390.421	0.000	22,000.000
Fertility ethnic majority	233,463	3,308.854	2,780.755	0.000	40,000.000
Fertility young	409,031	599.9961	919.004	0.000	14000
Fertility middle	409,031	1,907.310	1,697.119	0.000	28,000.000
Fertility old	409,031	1,169.596	1,359.983	0.000	20,000.000
Fertility uneducated	409,031	0.401	0.803	0.000	13.000
Fertility educated	409,031	3.193	2.659	0.000	42.000
Private hospital availability	9,907	0.897	6.388	0.000	116.000
Public hospital availability	9,907	108.934	152.744	0.000	1,206.000
Health exp./Gov. Exp.	163,364	7.610	3.817	1.221	18.321
Other aid/GNI	191,948	0.010	0.012	0.000	0.053

Table 1.C.1 (Continued)

	N	Mean	SD	Min	Max
	Panel B: All Countries				
Infant Mortality	1,866	38.546	27.279	3.500	137.700
Chinese health projects	1,866	0.102	0.396	0.000	7.000
Chinese projects, all	1,866	0.621	1.248	0.000	18.000
Chinese health commitments	1,866	778,637.417	9,787,306.287	0.000	3.450e+08
Chinese health projects, dummy	1,866	0.072	0.259	0.000	1.000
Chinese health projects (ODA)	1,866	0.090	0.344	0.000	4.000
Chinese health projects (OOF)	1,866	0.003	0.080	0.000	3.000
World Bank health projects	1,866	0.050	0.232	0.000	3.000
Reserves	1,866	0.120	0.148	-0.086	0.326
Inputs	1,866	0.176	0.963	-1.206	1.583
Probability	1,866	0.074	0.118	0.000	0.600
Probability (all aid)	1,866	0.301	0.271	0.000	1.000
Probability (ODA)	1,866	0.068	0.114	0.000	0.600
Probability (OOF)	1,866	0.002	0.014	0.000	0.133
Probability (amounts)	1,866	0.049	0.092	0.000	0.533
Population (log)	1,865	15.594	2.177	9.173	21.034
World GDP Growth	1,866	3.036	1.617	-1.679	4.403
Chinese FDI	1,866	24,479.619	26,478.287	915.777	74,654.039
Chinese Exports	1,866	8.654e+11	5.643e+11	2.044e+11	2.006e+12
	Panel C: DHS Countries				
Infant Mortality	579	57.409	26.490	7.600	137.700
Chinese health projects	579	0.207	0.578	0.000	7.000
Chinese projects, all	579	1.121	1.670	0.000	18.000
Chinese health commitments	579	1,795,874.865	16,541,998.338	0.000	3.450e+08
Chinese health projects, dummy	579	0.140	0.347	0.000	1.000
Chinese health projects (ODA)	579	0.180	0.473	0.000	4.000
Chinese health projects (OOF)	579	0.010	0.144	0.000	3.000
World Bank health projects	579	0.074	0.275	0.000	2.000
Reserves	579	0.104	0.147	-0.086	0.326
Inputs	579	0.103	0.981	-1.206	1.583
Probability	579	0.140	0.146	0.000	0.600
Probability (all aid)	525	0.455	0.283	0.000	1.000
Probability (ODA)	579	0.130	0.143	0.000	0.600
Probability (OOF)	579	0.006	0.023	0.000	0.133
Probability (amounts)	579	0.083	0.119	0.000	0.533
Population (log)	579	16.373	1.305	13.252	19.212
World GDP Growth	579	3.061	1.586	-1.679	4.403
Chinese FDI	579	20,523.490	24,190.145	915.777	74,654.039
Chinese Exports	579	7.839e+11	5.240e+11	2.044e+11	2.006e+12

Notes: "DHS countries" refers to those countries that are also included in our regressions at the local level.

1.D Additional Tables

Table 1.D.1 Transmission Channels at the Local Level: All Aid

	(1) **Assisted births skilled**	(2) **Assisted births traditional**	(3) **Public facility availability**	(4) **Private facility availability**	(5) **Public hospital uptake**	(6) **Private hospital uptake**	(7) **Hospital too far**	(8) **Antimalarial medication**	(9) **Staff hires**	(10) **Staff education**
					Panel A: OLS					
Chinese projects (t-2)	-0.298	3.150***	7.625*	0.176	-1.275	0.140	0.002	0.694***	0.011**	-0.016**
	(1.018)	(0.969)	(4.216)	(0.183)	(1.299)	(0.986)	(0.005)	(0.254)	(0.005)	(0.007)
					Panel B: Second Stage					
Chinese projects (t-2)	-29.814***	53.331***	-1.551	-0.099	-36.546***	-3.306	0.014	4.557***	0.034***	-0.076***
	(4.297)	(5.095)	(21.613)	(0.477)	(5.219)	(3.248)	(0.024)	(0.841)	(0.010)	(0.019)
					Panel C: First Stage					
Factor (t-3) x Prob.	1.136***	1.197***	3.747	3.747	1.169***	1.169***	1.355***	1.310***	2.897***	3.213***
	(0.189)	(0.192)	(2.288)	(2.288)	(0.189)	(0.189)	(0.174)	(0.174)	(0.165)	(0.193)
Reserves (t-3) x Prob.	1.630	1.335	24.515***	24.515***	1.380	1.380	-6.918***	1.323	-6.731***	-7.940***
	(1.370)	(1.379)	(8.751)	(8.751)	(1.361)	(1.361)	(1.170)	(1.168)	(0.969)	(1.144)
Number of countries	51	51	27	27	51	51	25	33	8283	7791
Number of observations	191,948	201,327	9,907	9,907	208,079	208,079	34,634	236,131	100,822	70,379
Adjusted R-squared	0.542	0.438	0.954	0.874	0.428	0.358	0.379	0.375	0.156	0.986
Kleibergen-Paap F	120.8	128.0	5.327	5.327	129.8	129.8	30.69	190.1	570.6	436.2

Notes: All columns include (log) population, the probability to receive aid, and country-year fixed effects. Columns 1 to 10 include ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 11 and 12 include facility fixed effects. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 1.D.2 World Bank Aid and Health Outcomes: All Aid

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Infant mortality	**Infant mortality**	**Infant mortality**	**Infant mortality**	**Assisted births skilled**	**Assisted births traditional**	**Public facility availability**
				Panel A: Second Stage			
WB projects (t-2)	3.816**	7.746*	35.835	-39.021	-33.971***	74.977***	-0.257
	(1.574)	(4.209)	(32.009)	(35.308)	(8.749)	(13.212)	(6.067)
				Panel B: First Stage			
IDA (t-3) x Prob.	-0.138***	0.031**	0.024	-0.008	-0.146***	-0.154***	-0.414***
	(0.022)	(0.014)	(0.022)	(0.011)	(0.024)	(0.023)	(0.118)
IBRD (t-3) x Prob.	0.328**	0.084*	0.029	0.028	0.044	0.041	1.931
	(0.140)	(0.044)	(0.044)	(0.033)	(0.294)	(0.310)	(1.265)
(1.265)							
Number of countries	52	152	53	54	51	51	27
Number of observations	367,410	1,866	579	846	191,948	201,327	9,907
Sample	local	all countries	DHS countries	DHS countries	local	local	local
Period	2002-2014	2002-2014	2002-2014	1997-2014	2002-2014	2002-2014	2002-2014
Adjusted R-squared	0.028	0.928	-0.333	-0.481	0.502	0.175	0.957
Kleibergen-Paap F	23.97	3.400	0.685	0.773	18.65	23.21	8.210

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Table 1.D.2 (Continued): All Aid

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Private facility availability	**Public hospital uptake**	**Private hospital uptake**	**Hospital too far**	**Antimalarial medication**	**Staff hires**	**Staff education**
	Panel A: Second Stage						
WB projects (t-2)	0.237	-43.684***	-1.605	0.019	11.436***	0.265***	-0.616***
	(0.164)	(10.404)	(4.542)	(0.022)	(2.274)	(0.080)	(0.213)
	Panel B: First Stage						
IDA (t-3) x Prob.	-0.414***	-0.153***	-0.153***	-0.130***	-0.126***	-0.042***	-0.041***
	(0.118)	(0.023)	(0.023)	(0.023)	(0.017)	(0.010)	(0.011)
IBRD (t-3) x Prob.	0.143	1.931	0.143	0.108	-0.101	0.143	0.226
	(1.265)	(0.280)	(0.280)	(0.175)	(0.246)	(0.159)	(0.212)
Number of countries	27	51	51	25	33	7	7
Number of observations	9,907	208,079	208,079	34,634	236,131	100,822	70,379
Sample	local	local	local	local	local	local	local
Period	2002-2014	2002-2014	2002-2014	2002-2014	2002-2014	2002-2014	2002-2014
Adjusted R-squared	0.873	0.366	0.358	0.376	0.328	-3.572	0.887
Kleibergen-Paap F	8.210	23.33	23.33	16.89	27.97	9.942	7.562

Notes: All regressions control for (log) population; the local-level regressions control for the probability to receive aid. Columns 1 and 5-12 include country-year fixed effects and and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 2, 3 and 4 include country and year fixed effects, with standard errors clustered at the country level. Columns 13 and 14 include facility and country-year fixed effects. DHS countries are those that are also included in the local analyses. Columns 7, 8, 13, and 14 include no IBRD projects; the equity-to-loans ratio-IV is consequently omitted as instrument. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 1.D.3 Alternative explanations

	(1)	(2)	(3)	(4)	(5)	(6)
	Trust	**Wealth**	**Infant Mortality**	**Corruption**	**Budget Management**	**Public Administration**
Chinese projects (t-2)	0.002	1.316***	8.655**	157.672	56.539	231.926
	(0.011)	(0.113)	(3.900)	(485.496)	(167.530)	(741.476)
Chinese projects (t-2) x Corruption				-48.732		
				(150.860)		
Chinese projects (t-2) x Quality					-15.394	-66.111
					(47.546)	(213.370)
Number of countries	25	52	52	38	39	39
Observations	34,634	357,063	357,063	29,179	29,639	29,639
Adjusted R-squared	0.126	0.985	0.035	0.014	0.018	0.011
Kleibergen-Paap F	19.39	97.21	97.47	0.577	10.26	0.607

Notes: Column 1 describes the impact of Chinese projects on the share of women in the DHS data who reported not having given birth in a health facility because they did not *trust* it. Column 2 shows the impact of Chinese aid on the level of night light emissions in the region. Column 3 replicates the main specification on infant mortality—as in Table 1.1, column 2—but explicitly controls for night light emissions. Columns 4 to 6 look into the effects on infant mortality, and use three Country Policy and Institutional Assessment (CPIA) indicators constructed by the World Bank (2020): 1. transparency, accountability and corruption, 2. quality of public budget, and 3. quality of public administration. These indicators range between 1 (low) and 6 (high). All regressions control for (log) population and include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. "DHS countries" refers to those countries that are also included in our regressions at the local level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 1.D.4 Share of Aid within Health Aid

Sector	**Percent**
Purpose specified	
Malaria	14.29
HIV, Ebola, Tuberculosis	2.72
Reproductive	2.72
Eyes	1.36
Traditional Medicine	1.36
Cancer	0.68
Purpose not specified	
Construction & Rehabilitation	26.53
Equipment	16.33
Medical Teams	25.85
Other	8.16

Notes: Shows the percentage of the number of Chinese health projects committed for a purpose or sub-sector in the total number of health projects.

Chapter 2

Free Trade Agreements and Development

Single Authored

Abstract

This paper analyzes the effects of Free Trade Agreements (FTAs) on various measures of local development in 207 countries over the 1990-2015 period. Using a Difference-in-Differences approach, I exploit spatial and time variation by comparing regions with (exogenously determined) exploitable and non-exploitable land before and after FTAs are "activated." I show that FTAs have a limited yet positive impact on a region's human development (as measured by the Human Development Index). The results also indicate that this limited impact can be explained by the positive effects of Free Trade Agreements on economic activity (night lights and GDP), together with the lack of significant influence on patterns of inequality (distribution of night lights among population). Finally, I also show that FTAs' impacts on human development are stronger for urbanized regions. Conversely, there is neither clear evidence of a weaker positive effect if trade partners belong to the Global North nor if the agreements include arrangements beyond the elimination of tariffs and quotas.

2.1 Introduction

Trade is one of the most pervasive processes of economic globalization, yet, so is the discontent with agreements liberalizing it. Since the industrial revolution, costs associated with trade have considerably decreased, inviting even geographically isolated countries to participate. As a result, trade interactions among countries have flourished, along with the incentives to exchange even further. The signing of the General Agreement on Tariffs and Trade (GATT) in 1947 was arguably the role model of block and bilateral trade agreements that later followed in Western countries. Since then, trade agreements have taken many forms, with Free Trade Agreements (FTAs) and preferential trade agreements (PTAs) being the most encompassing examples. Both types have been rather ubiquitous throughout the second part of the 20th century and the beginning of the 21st. The importance of such agreements is not only defined by their ubiquity, but, more importantly, by their capacity to set the rules of the trade game. This trade game, as most games, results in winners and losers and thus should not strike as especially different. However, what if the winners represent only a stark minority worldwide? Or even worse, what if the winners are systematically favored by the rules of the game?

Classical economic literature predicts that countries will benefit from increased exposure to trade; however, evidence of anti-FTA attitudes is neither scarce nor regionally concentrated. For instance, consider the case of Latin America vis-à-vis more developed regions. On the one hand, the adoption of Free Trade Agreements has remained a recurrent debate throughout Latin America for more than three decades (Falconí and Acosta, 2005; Paz y Miño Cepeda, 2007; Bohigues and Rivas, 2019).[1] Few would consider Latin America as a region winning the trade game in the end, as its countries—e.g., Mexico or Colombia—have often been explored as case studies for the negative consequences of FTAs (Otero, 2011; Salamanca et al., 2009). On the other hand, while the latter might lead to believe that trade agreements are to be contested only in developing regions—as predicted by Heckscher-Ohlin's theory[2]—rallies against them can be traced back to at least the seventies in developed countries as well (Held and McGrew, 2007; Reitan, 2012).[3] Indeed, the widespread mobilizations in Europe against the Transatlantic Trade and Investment Partnership (TTIP) in 2015, and the street demonstrations that have taken place since 2004 in South Korea and China against a trilateral FTA with Japan, are just some examples of an existing anti-FTA attitude in the developed world (Teney et al., 2014). Losing in the FTA game then may very well not be exclusive to developing regions but rather a globally expected outcome for specific groups within

[1] The recurrent meetings of the World Social Forum in the region are one of the most tangible demonstrations of such debates. The World Social Forum is an almost annual event that, since 2001, has gathered social movements that protest various epiphenomena of globalization, especially regarding economic liberalization and their effects on inequality (Seoane and Taddei, 2002).

[2] For an empirical discussion, see O'Rourke (2003).

[3] These demonstrations have also appeared in various, less straightforward, cultural forms (see Blyth, 2002; Camic and Gross, 2004; Tonkiss, 2006; Béland, 2009).

nations (Kriesi et al., 2012; Flesher Fominaya, 2014).

This potentially negative, global outcome, however, has not hindered the proliferation of FTAs worldwide. As a matter of fact, the relative number of more complex forms of FTAs has dramatically increased since 1990 (Dür et al., 2014), and most economists would still argue that free trade is a superior form of economic policy vis-à-vis protectionism.[4] The question of *why* Free Trade Agreements seem to provoke a backlash from heterogeneous labor groups worldwide yet continue to be a common and sometimes praised instrument of globalization remain both unanswered and relevant. In this paper I explore the impact of FTAs on both human and economic development at the subnational level in order to determine whether real world manifestations against FTAs can be associated with particular effects of such free trade policies on overall welfare.[5]

The empirical approach utilized makes use of global high spatial-resolution land cover data (ESA, 2017) which describe the predominant type of land—e.g., cropland, urban land, bare land, etc.—on the surface of subnational areas between 1992 and 2015, and a time-series (1990-2015) national-level proxy of FTAs' depth for a maximum of 207 countries (Dür et al., 2014). By exploiting subnational and variation over time via a difference-in-differences design, it is possible to overcome the well-known endogeneity of trade policies. With respect to development, I estimate the local impact of Free Trade Agreements by comparing their effects on subnational regions with naturally-determined exploitable and non-exploitable land cover. The relevant dummies then are specified as $Post_{j,t-\tau}$ (FTAs' dummy) and $Treat_i$ (land cover dummy). Countries that have an FTAs' depth indicator ≥ 1 are coded as $Post_{j,t-\tau} = 1$, and are countries that, some years before the analysis of indicators of development, have signed trade agreements with a substantive provision on tariffs and quotas of goods. Regions with exploitable land ($Treat_i = 1$) are areas covered mostly by cropland, urban areas, natural vegetation, or consolidated bare land, whereas non-exploitable regions ($Treat_i = 0$) are areas covered predominantly by non-consolidated bare land (e.g., deserts), water bodies, or permanent snow and ice.[6] My identifying assumption is that other than via the impact of Free Trade Agreements, and conditional on the use of relevant covariates such as country-year and regional fixed effects, development trends in subnational regions with and without naturally(exogenously)-determined land cover should not be different. Indeed, I test the common pre-trends assumption and find no evidence of a threat on this regard.[7] In other words, I rely on an identification strategy that uses a conditionally exogenous interaction (treatment). Thus, the interaction of interest between $Post_{j,t-\tau}$ and $Treat_i$ is not correlated with the error term and, therefore, is indeed conditionally

[4]See Fourcade (2009), Rodrik (2018).

[5]Giuliano and Spilimbergo (2014) show that economic shocks can alter policy preferences and, therefore, in adverse economic environments one can expect people to blame recently introduced economic policy such as FTAs.

[6]In section 2.2 I go into more detail about the construction of the referred $Post_{j,t-\tau}$ and $Treat_i$ dummies.

[7]If pre-trends represented a threat for identification, development across regions with and without exploitable land cover would have to show different trends before the FTAs' activation period, yet no evidence of such trends was found.

exogenous to the development outcomes analyzed.

To account for a more holistic understanding of development, I run tests on local proxies of human development, economic activity, and inequality: Human Development Index, night lights, GDP, and inequality—based on the distribution of night lights among the population.[8] The main results show that, in regions with exploitable land (treated regions), vis-à-vis non-exploitable regions (control regions), the local effect of FTAs on human development is *positive* (sig. at the 5% level) and leads to an average change of 0.00046 points (0.0029 standard deviations) on the Human Development Index.[9] I argue that this small, yet positive impact on human development is best explained by an increase of economic activity that does not alter inequality levels; in other words, the benefits brought by a general increase of trade and economic activity do not effectively translate into welfare as levels of inequality are not significantly impacted. This mechanism was tested in various ways. To begin with, I show that FTAs have a *positive* impact on economic activity, measured by an increase of night light emissions and GDP, and *no* impact on the night lights' GINI index. The estimated increase of night lights and GDP is of 9.7% and 6.8% (both at the 1% level of significance), respectively.

I also constructed country groups based on different measures of inequality, and show that, while the effects of FTAs on human development are rather *negative* for *more unequal* countries, their *positive* effects on economic activity remain mostly undifferentiated from the ones seen in more equal nations. In other words, while the increase in economic activity is statistically similar across country groups, the negative impact on human development is stronger for more unequal countries. Even when, in some cases, the positive effects on GDP and night light are statistically different between more or less equal countries, the effects are always larger in more unequal nations. Depending on the country grouping used, the decreases of the human development index in more unequal countries range between 0.04 and 0.14 percentage points, while the increases in night lights and GDP range between 6.4% and 18.3%.

FTAs, nevertheless, are agreements involving provisions that differ from dyad to dyad, from agreement to agreement, and from sector to sector. Therefore, I look into complementary answers in the form of impact heterogeneity. First, I separate FTAs signed with countries of the Global North from those signed with the Global South. In principle, given that my main indicator of FTAs conveys standardized information on the FTAs' depth, their effects should be indistinguishable as the FTAs' depth indicator does not discriminate between partner types to determine the FTAs' depth figure—it only focuses on the legal provisions included in such an agreement. However, authors like Diwan and Rodrik (1991), Marchetti and Mavroidis (2011), and Sell (2011) have

[8]The Human Development Index is an indicator that assesses key dimensions of human development, such as health, education and income, with figures between 0 and 1, where 1 refers to the highest degree of development, and 0 to the lowest.

[9]For context, the average inter-annual change of HDI at the country level between 1990 and 2015 was 0.0051 UNDP (2017). The average 5 year change was 0.026. The effect found then would represent approximately 9.2% of the historical yearly change average, and 1.8% of the 5-year historic change.

argued that developed countries' gains have come at the expense of developing countries for some types of trade agreements—e.g., the protection and enforcement of intellectual property rights found in the Trade-Related Aspects of Intellectual Property Rights (TRIPS) mostly benefited pharmaceutical companies located in the Global North, and made medicine nearly unaffordable for people from countries of the Global South.[10] While the estimated effect of FTAs signed with countries of the north is of a smaller size, it is not statistically different from the impact generated by FTAs signed with countries of the south. Second, drawing from Rodrik (2018), I look into the role of added complexity (or depth) to FTAs. This added complexity comes in the form of added legal provisions that go beyond the usual elimination of tariffs and quotas. Other works (e.g., Sakyi et al., 2017) have shown that an increase of legal complexity can increase transactional costs and therefore hinder potential benefits of any other policies. Although the effect of more complex FTAs is negative, I do not encounter robust evidence signalling that such effect is significantly different from that of simpler or less deep FTAs. Third, I look into heterogeneities across economic sectors. Different levels of skills associated with each sector and region can inform the effect of trade (Van den Berg, 2012; Hausmann and Hidalgo, 2010; Hidalgo, 2015; Balland et al., 2020). My estimates reveal that urban-associated productive regions perform better than any other exploitable region. These results are in line with regional studies that show structural differences in diverse human development measures in favor of urbanized areas.

Previous works assessing the effects of trade policy on development explore mechanisms such as power relations, cultural values, human capital accumulation, or the efficiency of institutions (see among others, Ferguson, 2006; Acemoglu and Robinson, 2012; Gokmen, 2017; Jensen et al., 2017). While these conduct tests beyond simple correlations, trade policies are fundamentally politically-informed arrangements whose causes and effects are difficult to identify. My model exploits an exogenous interaction that overcomes the analytical problem of endogeneity in such works.

Attempts around the identification of trade effects at the national level already exist, however (among many others, Frankel and Romer, 1999; Were, 2015). On the one hand, Frankel and Romer's country-level work tried to circumvent endogeneity by creating a geographic instrument to assess the effect of trade on income. On the other hand, Were's piece concentrates on the differential impacts of trade on growth in what he categorizes as developed, developing, and least developed countries. While both works find positive effects of trade on indicators of economic growth, their results show either low significance levels of the main effects, or country-specific heterogeneities. Moreover, other specific national-level impact assessments of FTAs' role on economic development are either anecdotal (Francois et al., 2005; Athukorala and Kohpaiboon, 2011; Busse and Groening, 2011) or very specific to particular elements of trade, such as trade flows

[10] This argument also connects with the rationale of the uneven geographic distribution of wealth covered in the core-periphery literature (a.o. Hirst, 1997; Wallerstein, 1976, 2005), where a geographic division of globalization winners (countries of the Global North) and losers (countries of the Global South) is also drawn.

or technology adoption (Bustos, 2011; Beyene, 2014; Parra et al., 2016). Closest to my work's mechanism discussion is the contribution by Artuc et al. (2019) and Cingano (2014). They argue that while income growth seems to be consistent for countries that have liberalized trade, increases in inequality are also. Inequality, as explained before, is the main mechanism explored in my work to reconcile the considerable impact of FTAs on a region's economic activity and its limited positive role on the same region's human development. Altogether, the results of the studies mentioned suggest that within-nation mechanisms are yet to be understood. The gap is even more evident for local-level studies and, to the best of the author's knowledge, this is the first global work assessing the impact of FTAs on subnational development.

Furthermore, FTAs are arrangements regarding trade, not human development per se. Good-quality, local-level trade data with panel dimension is unavailable for the majority of the world. Thus, one cannot directly assess the impact of FTAs on trade. There are, however, local economic indicators that might proxy trade and economic activity in general well enough. Two of those indicators are GDP and night lights. The use of such indicators, together with a measure of inequality (night lights GINI), represents another contribution of this work to the literature given that the joint assessment of levels of economic activity and inequality can provide an overview of the economic development of the studied region. As economic progress is one of the key bridges between low- and high-levels of human development, to understand FTAs' impacts on human development one needs to have a clear grasp of their effects on more direct forms of such development, i.e., on economic development. Indeed, my work assesses not only the impact of FTAs on indicators of human development but also on indicators of economic improvement (GDP, night lights, and night lights GINI).

To address robustness, I conducted several more tests. For instance, inspired by Christian and Barrett (2017), I show that both exploitable and non-exploitable regions share parallel trends in their human development indicators before the FTA's activation period. Similarly, given that the effective implementation of FTAs may differ greatly from country to country (Stevens et al., 2015), I also look into the role of FTAs-effect time structure. As the main analysis explores the mid-term effects of FTAs and therefore uses a lag of five years, I test other time specifications and control for different activation periods to assess whether they have a significant role on the impact of FTAs. The main estimated effect is consistent for most time-structures tested—even when all activation periods are used in tandem—suggesting that the small yet positive impact of FTAs is robust in the mid-term. Furthermore, I also explore time-placebo tests, i.e., leads of the preferred activation period. As expected, the placebo variables' coefficients are not statistically significant and barely affect the efficiency of my main estimator. Similarly, one of the concerns regarding my preferred specification arises from the absence of controls, besides the set of fixed effects, and the possibility that the absence of further covariates produces an omitted variables bias. It is partly shown that this concern is inconsequential in the main result tables, where I demonstrate that the inclusion of my preferred covariates does not affect the efficiency of the main point estimates. An additional test includes other geographic, political economy, and population controls and shows they are also of little relevance. Another common concern relates to the composition of the reference group and, specifically, to the lack of non-exploitable regions in some countries. As explained before, non-exploitable regions are areas that are mostly covered by non-consolidated bare land, permanent ice, or water. While few in number, there are countries that do not have such regions and therefore only have areas with consolidated bare land, urban areas, cropland, natural vegetation, or a mixture of these on their surfaces. For that reason, I run my main model including only a subsample of countries that have at least one non-exploitable region within their national borders. The results indicate that neither the inclusion of more covariates nor the exclusion of countries without non-exploitable regions bias the main results and, therefore, that the main results explored are robust to a potential omitted variables bias.

This work sheds light on the effects of FTAs on different indicators associated with development. It reconciles the impact of FTAs on human development by assessing the interaction between changes in economic activity and inequality patterns generated by the same FTAs. Moreover, it uses information on most countries of the globe and, thus, is more generalizable than previous studies using a limited number of countries (and usually using different empirical strategies, which limits comparability). An additional advantage of global yet highly disaggregated data is the possibility to explore local heterogeneities and unveil subnational causal mechanisms. Local identification is key because studies with lower spatial resolution can hide dynamics or impact heterogeneities mostly visible at the subnational level, e.g., power capture and its subsequent inefficient redistribution. Moreover, national-level studies lack by construction the statistical power of local-level studies, which pragmatically facilitates the analysis for researchers. For policy makers, this work offers key lessons about the conformation and negotiation of FTAs by shedding light on the aspects of an agreement that they should closely inspect, e.g., the sectoral composition of their economies. However, more importantly, it provides lessons about the goal indicators to be stressed in trade agreements since accounting for impacts on inequality is shown to be key to translating increased levels of economic activity into increased levels of human development.

This work is divided into five sections. In section 2.2, I discuss my identification strategy and I detail my data. In section 2.3, I explore the main results and, subsequently, some of the potential alternative answers to the main causal mechanism explored (section 2.4). In the final section, I summarize my work and state the human and economic development, as well as the sectoral, legal, and geographic implications of the results for future research and trade policy-making.

2.2 Identification Strategy

Country-level studies mostly refer to the elimination or reduction of tariffs and quotas as one of the main transmission mechanisms between a trade-related shock and economic development, as FTAs should positively impact development through less restricted trade.[11] The meta-analysis by Stevens et al. (2015), for example, suggests that less than 5% of the literature shows a negative effect of FTAs on volumes of trade. However, while tariff alleviation can bring higher margins of utility to both demand and supply via costs reductions of raw, intermediate, and final products (Amiti and Konings, 2007), this mitigation could also compromise countries' long-term welfare via loss of competitiveness in international markets and recurrent trade deficits (Astorga, 2010; Furceri et al., 2018). There are also other strands of the trade literature that consider that some FTAs have paved the way for the protection of particular commercial interests. In other words, through increased networking—with influential political spheres—transnational companies strengthen their market influence both across and within nations. This literature argues that such dynamics then contribute to an already unequal redistribution of wealth between countries of the Global North and Global South (Diwan and Rodrik, 1991; Caliendo et al., 2015).

Most of these works nevertheless use different models, country samples, units of observation, limiting comparability, and in turn, making any generalizable, conclusive statement on FTAs' impact on trade, let alone development, at least questionable. Moreover, the studies use national-level data that conceal subnational transmission mechanisms and effect heterogeneities, which local data can address more plausibly. Still, local analyses are not always straightforward as subnational data are often not comparable across countries due to their uneven quality. Fortunately, with the increasing accessibility to remote-sensing data, such studies have become more attainable. Perhaps the most relevant example using local data—and closely associated to this study—is the work by Henderson et al. (2018). Their work argues that economic development (proxied by night light satellite imagery) derives from the interplay of determinants such as trade intensity, geographical traits (e.g., distance to partner, altitude, temperature, relative distance to coast, etc.), and a path-determined human capital that divides the globe between early- and late-developed countries.[12] These types of studies, however, which analyze the local level impact of FTAs in heterogeneous countries, are still scarce. Given the limited empirical evidence using local level data, and the lack of replicability of national studies on the matter, it is safe to argue that the economic impact of FTAs on development is not yet fully understood. To the best of the author's knowledge, no work has yet assessed FTAs' impact on development using local-level data.

[11] Classical trade theory argues that even when trade liberalization produces losing parties, compensatory measures could potentially support such parties (Hicks, 1939; Kaldor, 1939). The possibility to enact such counter-measures then should converge towards Pareto optimality. This dynamic, however, might make sense if FTAs were shaped in a highly simplified setting, abstracted from political and cultural, thus neglecting the potential systematic role of power relations, institutional or regulatory flaws, cultural counter-values, etc. on redistribution patterns. Therefore, such pareto interpretation fades away in the current (complex, diverse, mobile) context of globalization.

[12] In early-developed countries, as Henderson et al. (2018) argue, high human capital and high trade (transportation) costs informed an even (geographically) settlement of productive activities. In late-developed countries, low human capital and the same trade costs (path-) determined the high geographic concentration of production.

This study focuses on the impact of FTAs on local development. To identify such impact, I implement a particular form of a difference-in-differences (DID) model that uses different activation periods, i.e., an event study. The intuition behind my strategy is the same as that in a difference-in-differences design, where one investigates the effect of a shock by comparing a treated and a control group over time. I exploit the interaction of local land cover traits to distinguish between treatment and control groups (i.e., cross-sectional variation) and the FTA status of a country to divide between pre- and post-periods of the treatment (i.e., time variation). In other words, I look into the effect of Free Trade Agreements in subnational regions with contrasting land exploitability. Naturally, endogeneity concerns arise mainly regarding the adoption of FTAs given that it normally depends on the partner countries' pre-agreement strengths and weaknesses, and therefore, they are hardly exogenous to a region's development. To address part of the potential endogeneity (omitted variables bias), I control for their direct effects on economic development using country-year fixed effects. The set of country-year fixed effects captures the direct impact of trade agreements given that FTAs are determined at the country-year level. In my main model I also use regional (individual grid) fixed effects, as I am also interested in directly controlling for subnational determinants of the development (land cover being one of those determinants). Thus, FTAs' and land cover's direct effects cannot be correlated with the error term and, therefore, the interaction between my dummies of land cover and FTAs conforms as a plausible, conditionally exogenous impact for my measures of development. The exogeneity of this interaction however, is strengthened further as the predominant land cover on the regions under study is naturally-determined, and therefore, is a priori exogenous; had it not, diverging pre-trends of development across regions with and without exploitable land would have to exist before FTAs' "activation." Following Christian and Barrett (2017), I plot the variation (Figure 2.1) in regions with different types of land cover together with the variation in human development for the period before the activation of Free Trade Agreements. As can be seen, the graph provides little reason to believe that the parallel-trends-before-treatment assumption is violated while trends in human development seem rather parallel across those regions with predominant non-exploitable land and those with mostly exploitable land from $t - 10$ until $t = 0$. Moreover, in Table 2.8 I explore the association between figures of development and future FTAs' impacts in both non- and exploitable regions and find no correlation between them. My identifying assumption then is that controlling for country-year and region fixed effects, and other potentially relevant covariates, development outcomes in subnational regions with naturally-determined exploitable and non-exploitable land, will not be affected differently in the post-period, other than via the impact of the trade agreements.

The implementation of my DID was carried out as is commonly done in these setups, and therefore initially constructs two main dummies: $Post$ and $Treat$. The usual $Post$ dummy then takes the value of 1 when t corresponds to the post-treatment period, and 0 when it corresponds to a pre-treatment period—note that, given that my DID uses different activation periods, the units of analysis might have different pre- and post-treatment periods. In parallel, the dummy $Treat$ takes the value of 1 for a unit

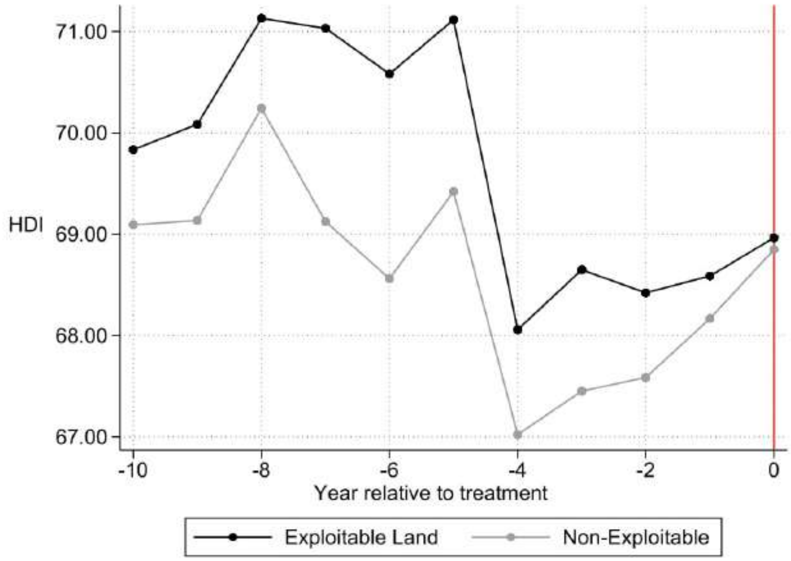


Figure 2.1 Testing for Spurious Trends

Notes: The figure shows the average HDI levels for the studied regions with exploitable land (black line), and without exploitable land (gray line), 10 years before FTAs' activation period (red vertical line). HDI is scaled ($\times 100$).

in the treated group (exploitable-land region), and 0 for a unit in the control group (non-exploitable-land region). The interaction of these two dummies then constitute the interaction of interest, $Post \times Treat$. Thus, my initial specification has the following general form:

$$Development_{i,t} = \beta_1 Post_{j,t-\tau} \times Treat_i + \beta_2 Z_{i,t-1} + \beta_3 \eta_{j,t} + \beta_4 \gamma_i + \epsilon_{i,t} \tag{2.1}$$

$Development_{i,t}$ is the average level of development in region i in year t. The components of the $Post_{j,t-\tau} \times Treat_i$ interaction follow the observational rules described in Table 2.1. Z is a vector of several individual (region) political economy and geographic controls, which I describe below. η_{jt} and γ_i represent the country-year and region fixed effects, respectively. Note that both the dummies $Treat_i$ and $Post_{j,t-\tau}$ are not separately included in equation 2.1 as these are directly captured by the region γ_i and country-year fixed effects $\eta_{j,t}$, respectively. In other words, I later show that the predominant land cover of region i is time-invariant, and thus, that this characteristic is effectively captured by the region fixed effects included in equation 2.1. Similarly, the FTAs indicator of any region i is defined at the national level, and while it does vary over time, the country-year fixed effects capture the impacts of these yearly changes. Recognizing the likely spatial- and time-correlation across my error terms, the standard errors ϵ are clustered at the regional and country-year level.

Table 2.1 delves into the main dummies of interest. Namely, the first row of the

Table 2.1 Observational Rules

Country j,t Post = 1		Region i Treat = 1	All	Agriculture	Manu&Serv	Other	Reference Group
FTA depth ≥ 1 in t-τ	&	Exploitable land ($10 \leq$ Mode LC ≤ 190 and Mode LC = 201) in i	1	0	0	0	0
FTA depth ≥ 1 in t-τ	&	Cropland ($13 \leq$ Mode LC ≤ 30 and Mode LC = 10) in i	0	1	0	0	0
FTA depth ≥ 1 in t-τ	&	Urban land (Mode LC = 190) in i	0	0	1	0	0
FTA depth ≥ 1 in t-τ	&	Other expl. land ($30 <$ Mode LC < 190 and Mode LC = 11-12)	0	0	0	1	0
FTA depth ≥ 1 in t-τ	&	Mostly non-exploitable land ($190 <$ Mode LC < 220 exc. 201) in i	0	0	0	0	1

Note: As seen, the range of land for our most general treated group, namely *Treat*=1, would be for Mode land cover figures between 10-190 or equal to 201, and the reference group would range between 190 and 220—with the exception to a Mode LC value of 201. The Mode LC values are at the regional level. FTA depth is the average depth level of all FTAs signed per country-year, and its depth is determined as detailed in Table 2.5. The lag on FTA depth is $t - \tau$, where my preferred specifications use a τ of 5.

table details the observational rules of the local regions under study for equation 2.1. I code the treatment period or $Post = 1$ if the region-year observation belongs to the period post-FTA's treatment. Consider region i; if the average FTA in the region includes provisions on—at least—tariffs and quotas (i.e., FTA depth[13] ≥ 1) in t-τ, then for all years after t, $Post_{j,t-\tau}$ will be set to 1. It follows that for all periods before t, the $Post_{j,t-\tau}$ dummy is set to 0.[14] I test several time structures, yet my preferred specification uses a $\tau = 5$ given that I am mostly interested in the impact of FTAs in the mid- to long-term.

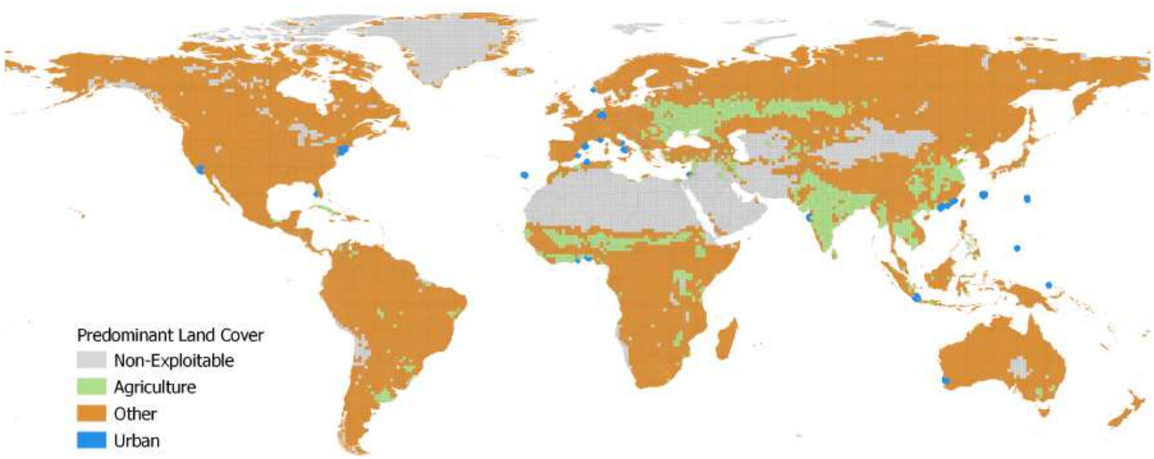


Figure 2.2 Subnational Land Cover categories in the World

Furthermore, I define as exploitable regions ($Treat_i = 1$) those areas where there is,

[13]In Table 2.5, I explain the construction of this variable in detail.

[14]In Table 2.A.5 of the Appendix 2.A, I run tests using a "generalized" difference-in-differences approach. The approach allows for the $Post_{j,t-\tau}$ dummy to "activate" on the exact period when FTAs treat the regions under study (i.e., $Post_{j,t-\tau} = 1$), and "deactivate" on the periods when there is no FTAs treatment (i.e., $Post_{j,t-\tau} = 0$). The results are qualitatively comparable to those of the main tables.

predominantly, cropland, urban land, other forms of natural vegetation, or consolidated bare land—those which have a mode land cover between 10 and 190, or equal to 201. The control group of non-exploitable regions, or $Treat_i = 0$, are defined as those regions with mostly unconsolidated bare land, water, or permanent snow and ice covering them, i.e., a mode land cover greater than 190 and smaller than 220, with exception to those with mode LC equal to 201—for a nuanced overview of all exploitable and non-exploitable land cover distinctions used in this chapter see Figure 2.2. As $Post_{j,t-\tau}$ varies over time at the national level, and $Treat_i$ incorporates information that changes at the regional level, their interaction—as in any standard DID—effectively exploits time and space variation.

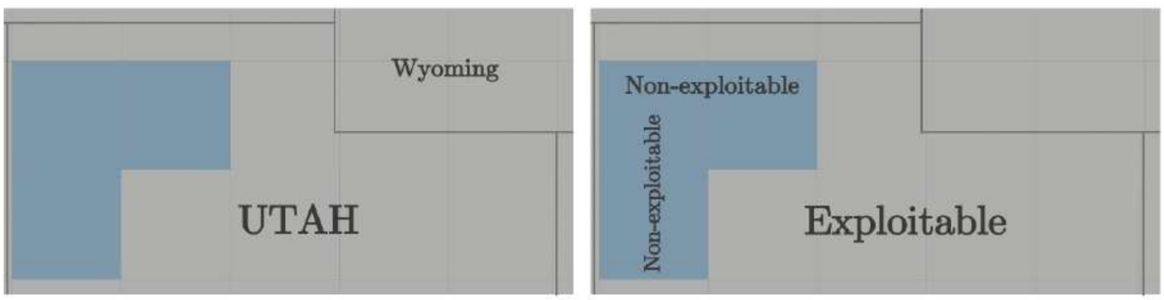


Figure 2.3 Subnational Land Cover categories of Northern Utah

In Figure 2.3, I illustrate the main subnational, land cover division operationalized in this work by using the example of the state of Utah in the United States. In my sample, 3 out of 25 subnational regions in the north-east of Utah are defined as "non-exploitable." Those regions are mostly covered by water or bare unconsolidated land and, therefore, according to my characterization, these are areas where it is considerably more difficult for productive activities or factories to settle or thrive.[15] It follows that the rest (22 out of 25) of the subnational regions in Utah are "exploitable." Contrary to the "non-exploitable" regions, these areas are mostly covered by shrubland and trees, which I argue makes them more suitable for new ventures to come in and grow.

In this way, I pose that the non-exploitable regions constitute a relevant control group for the effect of FTAs at local levels given that they simulate the non-treated status of countries that have not experienced an economic shock, including an FTA shock. For instance, countries might only be indirectly—if at all—affected by trade shocks in their neighboring countries via spillovers (see Khan, 2020). At the subnational level, one may indeed see such mechanics both at the borders of the neighboring "non-exploitable" regions and in the exploitable regions surrounding them (as in Figure 2.4). While exploitable regions seem to grow uniformly across space and exponentially over time, the non-exploitable areas seem to marginally grow at the borders when neigh-

[15]Given that fishing industries can indeed settle in areas mostly covered by water, in Table 2.A.2 of Appendix 2.A I run robustness tests where I code regions mostly covered by water bodies as "exploitable." The results are qualitatively identical.

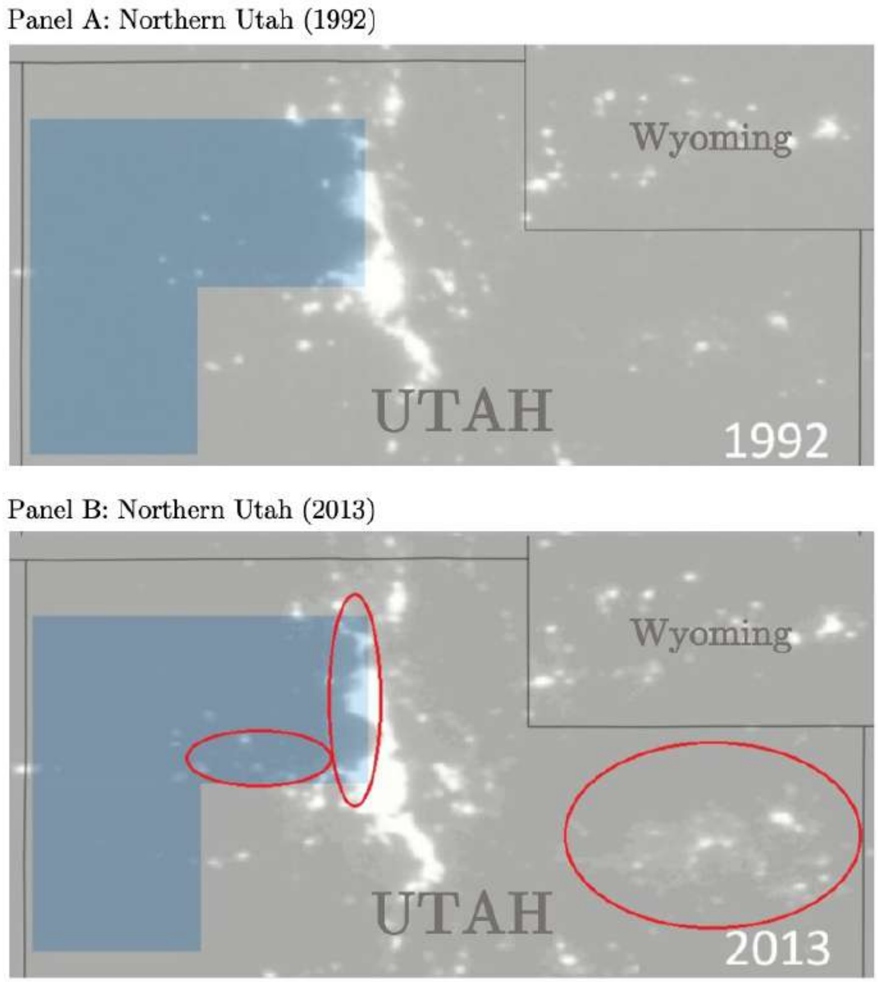


Figure 2.4 Growth and spillovers of night light on exploitable and non-exploitable regions.

Notes: Areas marked by red show a significant increase of night light output between 1992 and 2013.

boring exploitable regions. Indeed, between 1992 and 2013, the red-marked exploitable regions experienced a 146.25% increase of its mean night light output, whereas the non-exploitable region experienced just a 9.06% increase. This is the kind of dynamic that I expect post economic shock from exploitable and non-exploitable regions, ceteris paribus.

2.2.1 Data

Studies assessing development at the subnational level are increasingly common. The works by Sutton and Costanza (2002) or Sutton et al. (2007) canonized the use of remote sensing data by using night light emissions to proxy levels of economic activity at the local level. Night-time light emissions (night lights) are one of the most standardized proxies for economic activity. Apart from its panel and global nature, which adds to comparability, it reduces the recurrent measurement error in the production of local data, which is common in developing regions of the world. Henderson et al. (2012) and Jean et al. (2016) furthered the use of remote sensing data in development studies by proposing the prediction of rates of growth and poverty via the use of geographically-detailed data, e.g., altitude, temperature, geo-location. Upon these works, the literature has expanded with new ways of assessing satellite imagery's quality (Chen and Nordhaus, 2011; Chen, 2016; Mellander et al., 2015) and associating it to development. Night lights, for instance, have not only been shown to be correlated to economic activity but also to figures of wealth, health, or education (Noor et al., 2008; Weidmann and Schutte, 2017; Bruederle and Hodler, 2018).

I use a baseline sample of countries that have engaged in FTAs in the last three decades and construct subnational geographic divisions within those countries to study the local impact of FTAs. In total, the study encompasses 19,033 unique region cells—each roughly 111 by 111 km in size—that cover 2078 provinces/states and 207 countries of the world, during the 1990-2015 period. This paper assesses the impact of FTAs on local human development by using the subnational Human Development Index (HDI) (Kummu et al., 2018). The HDI portrays the degree of overall accomplishment in fundamental development dimensions considered by the human development definition of the United Nations: health, education, and economic development (UNDP, 2017). These dimensions are measured by jointly assessing the life expectancy at birth, the expected years of schooling, and the gross national income per capita of the regions under study. The subnational index includes global data between 1990 and 2015, which have a (roughly) 10 km-at-the-equator spatial resolution, and that were generated using different country- and local-level datasets. For non-European countries and based on a nearly complete, global subnational HDI report of 2009, Kummu et al. (2018) mostly used country-specific censuses and UNDP reports to extrapolate the equivalent HDI figures for subnational regions in years where information was not available. For European countries, Kummu et al. (2018) used the subnational HDI data of the Eurostat directly and extrapolated data points using national-level data on population and HDI when the data were not available. Similarly, in order to better understand the causal mechanism of FTAs on HDI, this paper studies the effects of FTAs on local economic development and uses various indicators: night lights, GDP, and subnational inequality.

The night light data come from satellite imagery generated by the Earth Observation Group, part of the National Oceanic and Atmospheric Administration of the United States NOAA 2015. The dataset covers the 1992-2013 period and has a spatial resolution of 1 by 1 km. For GDP, I also use the datasets constructed by Kummu et al.

(2018).[16] Their work contains values on subnational GDP from 1990 until 2015 at a roughly 10 by 10 km resolution, expressed in constant 2011 US dollars. The data on GDP are key to understanding the effects of FTAs on economic activity too, while, apart from adding robustness to potential results on night lights, it is a good proxy for local trade.[17] The subnational inequality indicator was constructed following Elvidge et al. (2012). This inequality measure uses the Lorenz curve principle to plot the cumulative distribution of night lights against the cumulative distribution of population density. For each year, I first sort grids of 55 km^2 (h) from the lowest to the brightest average night light intensity jointly with the respective population count within my units of analysis—grids of 111 km^2 (i). Lorenz curves are then generated and used to compute inequality (night lights GINI) for all 111 km^2 subnational regions. In other words, the coefficient is computed as the area between the Lorenz curve and the diagonal (0.5), divided by the area above the diagonal (0.5), as in[18]:

$$Inequality_{i,t} = \frac{0.5 - \int_0^1 (CumulativePopulation_{h,i,t}, CumulativeLight_{h,i,t})}{0.5} \tag{2.2}$$

This results in an index that resembles the income Gini index, ranging from 0 to 1, where 0 represents the highest level of inequality and 1 the lowest.[19] The local population data that I use come from the History Database of the Global Environment (HYDE) produced by Goldewijk et al. (2011), and has a spatial resolution of roughly 10 km^2.

The main goal of this paper is the measurement of the effect of national- or supra national-level arrangements at the local level. This is the case of FTAs as their figures are aggregated at the country level—with the exception of supranational regions like the EU. To overcome this shortcoming I combine the local data on human and economic development with an interaction of indicators of (national) FTAs and (local) land cover,

[16]Both night lights and GDP figures are skewed to the right; therefore, in order to smooth them out, I use the log of their values in all preferred specifications. I also use the inverse hyperbolic sine function for my preferred specifications, as seen in Table 2.A.1 in Appendix 2.A. For completeness, tests on their raw values were conducted resulting in qualitatively comparable outcomes. The details of such computations are available upon request.

[17]FTAs have previously been shown to be robustly and positively associated with trade in several studies (Stevens et al., 2015). Similarly, trade and GDP are positively correlated at national levels, as can be seen in Table 2.A.15 in Appendix 2.A.

[18]A graphical representation of such a computation by Elvidge et al. (2012) can be seen in Figure 2.D.1 in the Appendix.

[19]One can worry that such a local inequality measure is driven by the variation of night lights or population data. This concern was indeed shared by the author and was the main reason why smaller regions (than 55 km^2) within the areas studied (111 km^2) were not constructed. Doing so would have increased the probability of distortions driven, especially, by the population data. However, to test whether the inequality measure is driven by the light or population data, in Table 2.A.16 I run a correlation test between the indicators of night lights and population with the one on inequality. As seen, such correlation is pretty low, which suggests that neither night lights or population is driving such inequality measure.

and argue that the interaction delivers a good proxy for the subnational shock of such national FTAs variables. Following this logic I create dummies for each subnational region to capture information on FTAs' presence and land exploitability as a productive region—as explained in detail above, and particularly in Table 2.1 and Figure 2.3. The data on FTAs come from the work of Dür et al. (2014), who construct country-level indicators for the depth or conditions added to 1,002 FTAs since 1948. The FTAs' depth is an additive indicator of the type of provisions that a particular FTA includes that ranges between zero and seven, i.e., higher values of FTAs' depth include all conditions/provisions corresponding to lower FTAs' depth values—as detailed in Table 2.5.[20] For instance, the values of $depth \geq 1$ refer to FTAs with almost no tariffs and quotas for most goods, whereas the values $depth \geq 2$ refer to agreements that, apart from eliminating barriers on tariffs and quotas, include the elimination of most impediments on the exchange of services. My sample consists of 749 different FTAs negotiated between 1990 and 2015, and I construct the main indicator of FTAs' depth as an annual average of FTAs signed by a country in a year.

The remote-sensing land cover data describe the surface of the land, i.e., whether it has cropland, shrubland, water bodies, bare spaces, etc. The data do not describe the suitability of the land but rather the actual characteristics of the land covering the region's surface. These land cover (LC) records come from the work of the European Space Agency (ESA) and the Climate Change Institute, which released the LC project in 2017 (ESA, 2017). The LC data are global, include yearly information from 1992 to 2015, and use the Land Cover Classification System (LCCS)[21] designed by the Food and Agriculture Organisation (FAO) to categorize different types of land cover. The spatial resolution of the data is mostly at 300 meters—with some areas up to 30 meters. In order to define the predominant category of LC in region i of 111 km^2, I use the mode of LC categories within region i. Thus, as shown in Figure 2.2, if the mode LC value of region i is for instance urban land, I define urban land as the predominant LC in region i, or similarly, if the mode LC value of region i is agricultural land, I then define agricultural land as the predominant LC in region i. One might be concerned that the LC mode is not the right proxy to establish the predominant type of land in any region i. However, note that the spatial resolution of the land cover data—30 to 300 meters—is high enough to argue that, in a region of 111 km^2, the LC mode approximates the region's most common land cover category. Note that, depending on the resolution of land cover data in region i—as said, 30 or 300 meters—each area of 111 km^2 would consist of 137,174 or 1,371,739 land cover data points or pixels.[22]

I turn now to the description of the control variables that I use in the main tables to further reduce potentially omitted variable concerns. Being under a particular FTA is arguably correlated with factors that affect development differently in regions with

[20] I consider "accessions" as different FTAs while they add a new country to the deal and as my analysis is at the subnational region-year level.

[21] The LCCS data legend is included in the Appendix, Table 2.B.3.

[22] For robustness, I also used the mean LC value per region to define economic sectors. Results do not change qualitatively and are available upon request.

high or low exploitability. For instance, the degree of exploitability could be correlated with geographic patterns (e.g., temperature, distance to cities), which could imply that any differential effects of land cover on development resulted from those patterns rather than from the contrasting endowments of exploitable land. The data on temperature is computed by using the PRIO-GRID vector grid Tollefsen et al. (2012). It is yearly calculated as the mean degrees Celsius within the region i. Apart from controlling for temperature, I include a distance-covariate utilized in the geography-trade literature analysis: distance to capital city (Allen and Arkolakis, 2014; Martin and Pham, 2020; Rauch, 2016). The distance variable comes also from the PRIO-GRID dataset, and is computed as the average distance (in km) from region i to the capital city. I also use two political-economy controls, namely the birth region information of leaders of the executive (Hodler and Raschky, 2014) and aid disbursements by the World Bank (AidData, 2017). The birth region variable is meant to capture the role of political favoritism on a region's development. Hodler and Raschky (2014) show that leaders seem to favor their birth regions as suggested by higher night light emissions and aid amounts in areas close to their birthplaces. I construct a dummy indicating whether the leader of a country j is in office by year $t-1$ and was born in region i. Following Hodler and Raschky's rationale, I expect significant results on development a year after the leader took office. I thus include the lagged dummy variable in the covariates vector. The data were directly provided by Hodler and Raschky. Similarly, Dreher et al. (2019a) show that one of the channels of favoritism is Chinese aid. Moreover, Cruzatti C et al. (2020) show a relevant impact of Chinese aid on health indicators. In this study I use the geo-referenced data constructed by AidData (2017) on World Bank (WB) aid from 1995-2015, and calculate the region's yearly mean WB aid disbursements in constant 2014 USD. I only use projects that have coordinates with exact location information, within 25 km, or refer to the center of the country's second order administrative division—depending on the country, either province or state.[23]

2.2.2 Remaining Identification Concerns

My estimation strategy combines the use of national- and local-level data. Due to the precision of the local data, one might circumvent problems of omitted variables bias of national data or lack of statistical power. For instance, exploiting data at finer levels of spatial resolution allows for the inclusion of finer levels of fixed effects, thus controlling for potentially unobserved determinants of the effect more precisely.

On the one hand, in the context of national-level models of trade, one of the axioms is that international exchange is directly proportional to the size of the country and inversely proportional to the distance of the counterpart (the so called "gravity model", e.g., Rauch, 2016). These models argue that the geographic distance between industries is a relevant explanatory variable of varying levels of trade. One can claim that this study does not include such a control on distances between compatible sectors (i.e., with

[23]Tables 2.B.1 and 2.B.2 in Appendix 2.B show the study variables' sources and definitions, and their descriptive statistics, respectively.

the same predominant land cover) and therefore might ignore a problematic correlation of my interaction of interest with the error term that contains the unexplained determinants of development. The distance between regions with the same predominant sector (land cover), however, varies across regions but not over time and, thus, their potential correlation is already captured by the regional fixed effects in the model. On a similar note, one may also be concerned about the human- or technology-associated malleability of land cover at the regional level, which would threaten an—arguable—as-if randomness and time invariability of the predominant types of land covering my studied regions and, ultimately, the identification of the local effects of FTAs. Indeed, due to human intervention, predominant land cover in a region could change and its direct effects then would not be captured by the chosen level of fixed effects. However, while theoretically possible, the probability to do so, such that the predominant LC of areas of 111 km by 111 km changes in a few years, is small. For instance, the share of regions in my sample that experience at least one change from a land category to another in any of the years under study is only 4.10% (780 out of 19,033). Among this 4.10%, 99.23% of them (774 out of 780) return shortly thereafter to their most frequent category and, therefore, were easily categorized as either exploitable or non-exploitable regions. These facts strengthen the assumption of time-invariant land cover for any region i, which in turn render the set of fixed effects of my specifications sufficient. Nevertheless, robustness tests with specifications dropping units that experience land-category changes were conducted in Table 2.A.3 of Appendix 2.A.[24]

On the other hand, there may remain concerns regarding the construction of my $Post_{j,t-\tau}$ dummy. As detailed above, $Post_{j,t-\tau}$ is mainly determined by the FTAs' status. This status is in turn determined by the yearly average "depth" of all trade agreements in a country. Thus, there may be trade-intensity issues that I cannot account for directly. For instance, by taking the average of all FTAs, I assume all partner countries have the same relevance. While numerically speaking this might hold true—i.e., one sovereign country does not count less than any other before the international community because of its size or power—the reality is that some partners are more impactful than others.[25] Therefore, one should control for the size or economic power of the country's partner in order not to bias the FTAs' indicator. Note, however, that such control already exists in my equation 2.1. The characteristics of trade partners of any country for any year t are already captured by the inclusion of country-year FE, as they control in its most general form for all year-to-year determinants of economic development in the countries under analysis.[26] In other words, country-year FE capture national-year-specific characteristics that could have an impact on the development of

[24]No qualitative change in the main results were found.

[25]Imagine the case where a small country like Ecuador signs an FTA with Paraguay, with a depth equal to 1 in year t. Now lets imagine that in $t+1$ Ecuador signs another FTA, with the same depth of 1, but with the USA. For computation purposes, Ecuador will have the same FTA figure in year t and $t+1$ even when the partner's power, economically and politically, is incomparable.

[26]Even supranational characteristics—such as belonging to blocs like the European Union, for example—since the country-year fixed effects are already absorbing more precise territorial variation.

all regions in a particular country, with one of these characteristics being the size or power of its trade partners. Notwithstanding all the latter, I run several robustness tests detailed in Appendix 2.A where I explore different constructions of the FTA variable instead of the dichotomous $Post_{j,t-\tau}$ presented in equation 2.1. Namely, in Table 2.A.6 I use the number of FTAs signed in any given year ($FTANumber_{it-5}$), in Table 2.A.7 I use the mean depth of FTAs signed in any given year ($FTADepth_{it-5}$), and finally, in Table 2.A.8 I differentiate between big and small countries and interact such categorization with my main $Post_{j,t-\tau}$ variable. Results do not challenge my main findings and are detailed in the Appendix section.

2.3 Results

Table 2.2 shows the results for the impact of FTAs on the Human Development Index of the subnational regions studied. All columns reflect the effect of FTAs on a scaled HDI (HDI×100). Columns 1 to 6 report the estimates for equation 2.1 and include fixed effects progressively. In columns 1 to 3 I report the results without the geographic and political-economy controls described in Section 2.2. In columns 4 to 6 I include the controls detailed in the previous section. My preferred specifications correspond to those in columns 3 and 6, which include the full set of fixed effects at the country-year and regional (individual) level. In all the result tables, the discrepancy between the number of observations in the specifications that include all control variables and those that do not stem from the missing values in the dataset of the temperature variable.

In order to get a first insight into the potential impact of FTAs on human development, columns 1 and 4 describe the impact of FTAs on the Human Development Index without fixed effects. Columns 1 and 4 report a statistically significant figure for my coefficient of interest, β_1. The results, however, show contradictory patterns; while column 1 shows an increase of 2.249 percentage points (p.p.) on the HDI at the 5% level, column 4 shows a detrimental effect of 4.487 p.p. Although the inclusion of covariates in column 4 means the sample is not directly comparable to the one used column 1, if the results were to be robust to different types of bias, one would expect qualitative similar patterns to be observable in the variables of interest. Given that FTAs are negotiated at the country level—and sometimes even at higher levels, such as the European Union—it makes sense that non-captured country heterogeneity is able to distort results in such a way. In columns 2 and 5 I turn to a specification that includes the use of country-year fixed effects. Columns 2 and 5 report a positive effect of FTAs on HDI. While column 2 shows an increase of 0.223 percentage points (p.p.) on the Human Development Index, column 5 shows a positive effect of 0.945 p.p. The results for column 2, however, are not statistically significant, whereas the results for column 5 are so at the 1% level. Given that there are several non-observed persistent determinants of Human Development at the local level that are also correlated with my variable of interest, such as geographical, cultural, or historical features, it is necessary that one controls for such potential sources of omitted variables bias.

Table 2.2 FTAs and Human Development

	(1) **HDI**	(2) **HDI**	(3) **HDI**	(4) **HDI**	(5) **HDI**	(6) **HDI**
$Treat_i \times Post_{jt-5}$	2.249**	0.223	0.046**	-4.487**	0.945***	0.098**
	(1.134)	(0.243)	(0.023)	(1.739)	(0.348)	(0.039)
$Treat_i$	2.049**	0.435***		7.338***	-0.066	
	(0.895)	(0.167)		(1.553)	(0.277)	
$Post_{jt-5}$	-0.356			2.350*		
	(1.261)			(1.245)		
Observations	450,237	449,786	449,786	238,673	238,453	238,453
Adjusted R-squared	0.009	0.949	0.997	0.260	0.946	0.997
Controls	NO	NO	NO	YES	YES	YES
Country-Year FE	NO	YES	YES	NO	YES	YES
Region FE	NO	NO	YES	NO	NO	YES
Countries	207	190	190	193	175	175
Regions	18,392	18,375	18,375	16,737	16,719	16,719

Notes: All HDI values are scaled (HDI×100). When specified, columns include country-year and regional fixed effects. When specified, the set of controls includes temperature, World Bank aid, Leaders' birth regions and mean distance to capital city (when region fixed effects are not used). Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

In columns 3 and 6 I turn to the most stringent variations of my model. The results on these specifications show a rather small yet statistically significant positive effect of FTAs on local Human Development. The generated effect ranges between an average increment of 0.046 p.p. (0.29% standard deviations of HDI) in column 3 and 0.098 p.p. (0.56% standard deviations) in column 6. Both coefficients are statistically significant at the 5% level. For instance, consider a subnational region i in period t with an average HDI value correspondent to the sample's HDI mean, 0.71. Based on the estimates shown in column 3, it is expected that five years after the FTA was signed, the HDI value in region i increases to 0.71046. Altogether, the results suggest that the impact of FTAs on human development is small yet positive.

The small size of such a positive effect (less than 0.29% standard deviations) of FTAs on Human Development can be potentially explained by an increase of economic activity that does not redistribute opportunities amongst the population. The benefits brought by an increase of trade and economic activity in general might not be effectively translated into human welfare as levels of inequality are not being impacted by such trade agreements. The literature on that regard is vast, yet the trade-growth literature specifically argues that globalization, while having brought about clear progress in areas such as trade and technology, has neglected other necessary elements for development, such as the reduction of inequality (Artuc et al., 2019). The lack of inequality reduction then may play a fundamental role in the low impact of processes of globalization, such as the implementation of FTAs, on the improvement of more comprehensive indicators of development, which by definition transcend the assessment of mere economic activity.

Table 2.3 FTAs and Economic Development

	(1) **Light**	(2) **Light**	(3) **GDP**	(4) **GDP**	(5) **Inequality**	(6) **Inequality**
$Treat_i \times Post_{jt-5}$	0.097***	0.081***	0.068***	0.041	0.0001	0.011
	(0.021)	(0.027)	(0.018)	(0.032)	(0.006)	(0.009)
Observations	389,968	209,911	448,021	237,763	358,031	197,855
Adjusted R-squared	0.972	0.980	0.985	0.984	0.706	0.693
Controls	NO	YES	NO	YES	NO	YES
Country-Year FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Countries	200	176	193	176	185	173
Regions	18,046	16,392	18,400	16,722	17,169	16,111

Notes: Light and GDP are logged. All columns include country-year and regional fixed effects. When specified, the set of controls includes temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

In Table 2.3, I test potential transmission mechanisms that can explain the limited positive impact of FTAs on human development.[27] On the one hand, I assess FTAs' role in economic activity, measured as night light emissions (columns 1 and 2) and GDP (columns 3 and 4). To the extent that FTAs directly impact levels of trade (Stevens et al., 2015), one should see this impact translated into changes in measures of economic activity that straighforwardly capture increasing/decreasing levels of trade, such as GDP.[28] However, the impact of FTAs on economic activity can also be indirectly seen in the form of infrastructure needed for the establishment of production lines and, therefore, that one should see an increase of night light emissions in places where eco-

[27] The number of total observations varies across dependent variables given that periods and local availability of data differ for each variable. In Table 2.A.9 for the first four columns, I run tests restricting the sample to the time period available for all dependent variables: 1992-2013. In the last four columns of the table, I also restrict the sample to the minimum number of non-missing data for all specifications. Results remain qualitatively unchanged.

[28] As GDP = C + I + G + (X − M), where C is consumption, I is investment, G is government spending, X is exports, and M is imports.

nomic activity has thrived. [29] In principle, the effects on night lights and GDP should be qualitatively similar given the elsewhere shown correlation between the two; however, one can argue that potential discrepancies can be explained by the straightforwardness of the impact of one (night lights) in comparison to the more layered impact of the other (GDP). While night lights directly describe the state of infrastructure and thus indirectly describe the level of aggregated economic activity, GDP is a direct measure of such activity informed by several—sometimes hard to quantify—productive activities that do not necessarily rely on night light infrastructure in order to materialize.

I also evaluate the effect of FTAs on local inequality (columns 5 and 6). Varying levels of inequality have been shown as being key to explaining varying levels of development (Cingano, 2014; Artuc et al., 2019). In these works, it is suggested that an increment of economic activity without an improvement of distributive patterns, on average, should distinctively affect places with high and low figures for inequality. That is, an increment of the economic activity in regions with low inequality should be beneficial on average for the region as such activity premiums would be distributed among more portions of the population, making their current development status better off vis-à-vis their ex-ante (pre-FTAS) status. Conversely, an increment of economic activity in areas with high inequality should have no significant (or even negative) effects for the region's human development on average, as such premiums would concentrate in fewer hands and, hence, widen the access gap to basic goods and services—the access to which constitute a pillar of human development—between upper- and lower-income households.

Similar to Table 2.2, the uneven columns of Table 2.3 report the results with the full set of fixed effects but without other controls. The even columns show the results with the inclusion of the full set of fixed effects and further controls. As can be seen, the results between the two specifications are highly comparable for all outcomes. Columns 1 and 2 report the positive and statistically significant (at 1% level) impact of FTAs on logged night light. The coefficients of interest are nearly equal, i.e., FTAs provoke a 9.7% increase of the geometric mean of night lights for column 1 and 8.1% in column 2. This means that, if the night light output of any region i was 10 in period t, one would expect to see that this region's night light increases to approximately 11 in $t+5$. Similarly, columns 3 and 4 report the positive impact of FTAs on logged GDP. The coefficients of interest are quantitatively comparable, i.e., FTAs leading to a 6.8% increase of the geometric mean of GDP in column 3 and a 4.1% increase in column 4—the latter coefficient, however, is not statistically significant. For instance, for a region i with a GDP output of 1,000,000 USD in year t, the GDP figure will have grown to 1,068,000 five years after FTAs were introduced. The results on GDP and night lights indicate that FTAs do bring an expansion of economic activity in the areas where they are introduced. I conclude this table by assessing the impact of FTAs on inequality in columns 5 and 6. The coefficients of the interaction are quantitatively similar; the effect

[29] Indeed, many authors have already shown such an association between economic activity and night lights (Sutton and Costanza, 2002; Sutton et al., 2007; Weidmann and Schutte, 2017).

of FTAs on inequality is rather small and positive but not statistically significant.[30]

Taken together, the results of Tables 2.2 and 2.3 suggest that the lack of impact by FTAs on inequality diminishes the overall benefits on human development that FTAs otherwise do bring to economic activity. This is in line with the hypothesis that the reduction of inequality is a key factor to the improvement of human development. In parallel, the results of Tables 2.2 and 2.3 also show that the inclusion of geographic and political economy controls do not qualitatively change the coefficients of interest. Thus, to avoid the loss of too many observations due to missing values for the control variables—temperature being the most relevant, with 242,919 missing values—and if not specified otherwise, the remaining specifications of the main text will compute the model of equation 2.1 without such controls. In Table 2.9 I nevertheless run a robustness test to assess whether such controls could represent a threat or not to the identification of my main effect. The results are displayed in Section 2.4.

In Table 2.4 I explore the mechanism by means of which inequality can affect economic and human development. If inequality is indeed the catalyzing factor between economic activity and human development, high inequality patterns should aggravate the results shown in Table 2.2, i.e., I expect more unequal regions to show poorer human development figures while maintaining positive and comparable levels of economic activity. In other words, I expect that poor effects on human development figures, such as those shown in Table 2.2, are more noticeable (even smaller or negative) for more unequal regions, and that the overall performance of economic activity (measured in changes of GDP and night lights) remains positive and comparable between regions of contrasting levels of inequality.

[30]As Salvati et al. (2017) argue, a limitation of the indicator of night light inequality is that it can take similar values for areas that have contrasting degrees of luminosity. This is particularly relevant in areas with a low number of people living in them. That is, a region with its entire population living in almost complete darkness will have the same (perfect) inequality value of 0 as regions where all their population have access to the same level of brightness—as what the indicator measures is the (un-)equal distribution of night lights. For this reason, I run robustness tests separating grids with a number of people above and below the median—many more (population-wise) splits were attempted, yet, results were always qualitatively similar, therefore, are not included in this study (they can however be requested directly from the author). I also winsorize the indicator of night lights inequality to values closer to the minimum and maximum values of income Gini coefficients in the study period: 0.20-0.66. This way, I can restrict computations to historically more representative figures of inequality. Results are shown in Table 2.A.10 and portray how, only when the range of night light inequality is brought to the historic maximum and minimum Gini values (column 4), an increase of inequality becomes significant at the 10% level. These results strengthen the argument that FTAs do not significantly improve inequality, and if anything, worsen it.

Table 2.4 Mechanism: Inequality

	Local Inequality		Historic Inequality		Year-to-year Inequality		LAC vs. The World		Henderson's Early-Late		Henderson's Early-Late: Adjusted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	HDI	**Light**	**HDI**	**Light**	**HDI**	**Light**	**HDI**	**Light**	**HDI**	**Light**	**HDI**	**Light**
$Treat_i \times Post_{jt-5}$	0.117***	0.069**	0.113***	0.091***	0.100***	0.002	0.052**	0.097***	0.076***	0.054*	0.124***	0.033
	(0.035)	(0.030)	(0.030)	(0.033)	(0.028)	(0.035)	(0.024)	(0.022)	(0.023)	(0.030)	(0.023)	(0.032)
$Treat_i \times Post_{jt-5} \times AvgIneq3y_{it}$	-0.092***	-0.023										
	(0.025)	(0.020)										
$Treat_i \times Post_{jt-5} \times Unequal_j$			-0.140***	0.008								
			(0.044)	(0.043)								
$Treat_i \times Post_{jt-5} \times Unequal_{jt}$					-0.051*	0.089**						
					(0.029)	(0.045)						
$Treat_i \times Post_{jt-5} \times LAC_j$							-0.123**	-0.007				
							(0.052)	(0.062)				
$Treat_i \times Post_{jt-5} \times LateDev_j$									-0.043	0.183***	-0.125***	0.179***
									(0.055)	(0.044)	(0.047)	(0.042)
Observations	280,724	277,213	409,862	360,304	194,340	174,713	449,786	389,968	304,221	267,287	304,221	267,287
Adjusted R-squared	0.997	0.976	0.997	0.973	0.997	0.973	0.997	0.972	0.997	0.976	0.997	0.976
Country-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Countries	184	185	156	158	141	140	190	200	116	116	116	116
Regions	17,157	17,167	16,717	16,655	16,329	16,118	18,375	18,046	12,346	12,342	12,346	12,342

Notes: All HDI values are scaled (HDI×100). All columns include country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

I test such a dynamic in Table 2.4, by separating subnational regions and countries of the globe in different ways. First, in columns 1 and 2 I use the local measures of inequality of Tables 2.2 and 2.3 to construct a three-year average of subnational inequality and assess its continuous role on the impact of FTAs at the local level.[31] Second, in columns 3 and 4 I separate countries between those below and above the historic world median of income inequality ($Unequal_j$), measured by the income GINI index. Third, in columns 5 and 6 I divide nations between those below and above a time-variant world median of inequality using the same GINI index. Fourth, in columns 7 and 8 I separate Latin America from the rest of the world as countries of this region have been commonly associated with higher levels of income inequality, as seen in Figure 2.D.2 in the Appendix. Finally, in the last four columns (9-12) I divide the sample following the distinction used by Henderson et al. (2018) between early- and late-developers, as in Figure 2.5.

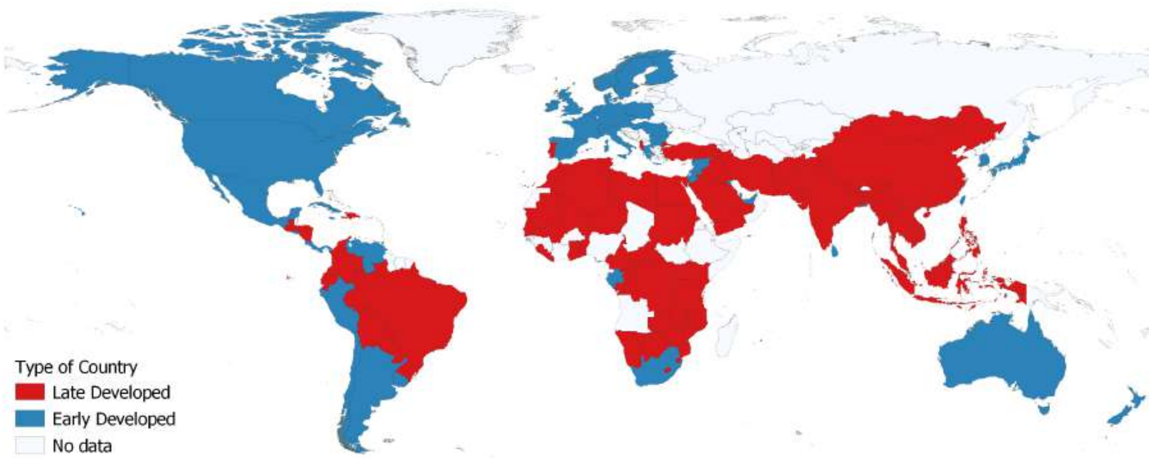


Figure 2.5 Stages of Development by 1950

Notes: Late developed countries do not pass any of the development cutoffs proposed by Henderson et al. (2018). Early developed countries surpass at least one of the three indicators.

Henderson and co-authors implemented this categorization by considering the average performance of countries in 1950 for three elements: education, GDPpc, and urbanization. If a country in their sample passes their established performance threshold in any of the three elements, a country is categorized as early-developed. Conversely, countries that do not pass any of the thresholds are categorized as late-developed countries. Given that access to such development-related elements are key to understanding the main sources influencing the gap between upper and lower socioeconomic classes, Henderson et al.'s distinction constitutes a straightforward proxy for a Global North-South division that focuses on the analysis of the interconnection between inequality, economic activity, and human development.

[31] In Table 2.A.4, I construct different versions of the local inequality measure and use them in tests that use the same specifications of columns 1 and 2 of Table 2.4. Results are qualitatively similar.

Columns 1 and 2 of Table 2.4 display the impact of FTAs in subnational regions with differing levels of inequality. The columns show that, as inequality increases, the overall positive impact of LAC on the HDI is reduced. Namely, while FTAs' effect for totally egalitarian regions is of 0.117 p.p., at the 1% significance level, every percentage point growth of inequality would diminish FTAs' positive impact on the HDI in about 0.0009 p.p., at the 1% level. Similarly, the rest of the table shows that, when statistically significant, the impact of FTAs on the HDI of regions of more equal countries ranges between 0.052 (column 7) and 0.124 (column 11) percentage points. This positive impact however, has a different nature for more unequal countries. Thus, as seen in columns 3, 5, 7, 9 and 11 such impact is greatly undermined. For columns 3, 7 and 11 the total impact of FTAs on human development is even negative among unequal countries.

Results portray a different picture on the impact of FTAs on economic activity (night lights), as more unequal regions often benefit more or similarly to less unequal regions. For instance, column 2 shows that the overall positive effect of FTAs on night lights does not change as inequality grows. The positive impact is of 6.9%, at the 5% level, regardless of levels of inequality. Furthermore, when there is a statistically significant difference between regions, such difference favors those in more unequal countries—the marginal increase ranges between 8.9% (column 6) and 18.3% (column 10).[32] The results, however, do pose an intriguing outcome in column 9. This result on HDI shows that, even when the coefficient trends in the same direction of comparable columns, there is no significant difference between the positive impact of FTAs on HDI in regions of countries of the Global North versus that experienced in regions of the Global South. The difference (between column 9 and previous country-divisions columns) can be explained, however, by the precision of the country distinctions made. For instance, while the divisions between countries in terms of *Historic Inequality* (columns 3 and 4) used an objective and more time-relevant period for my sample (GINI indicators from 1960 to 2019), the division that Henderson et al. make, using indicators from 1950, might no longer represent an accurate measure of inequality.[33] For that reason, in columns 11 and 12 I modify the distinction used by Henderson et al. (2018) between early- and late-developers by including as late-developers those countries that rank above the historic median of inequality.[34] As seen, once the adjustment has been introduced, results are coherent with the other figures of the table.

The interpretation of the results for Table 2.4 appears to be twofold: 1) both the least and most unequal regions experience economic growth due to the introduction of FTAs, yet, and perhaps more importantly, 2) while the least unequal regions experience

[32] I also run tests on GDP instead of Light and HDI in Table 2.A.11 of Appendix 2.A. Results do not qualitatively change the conclusions drawn from Table 2.4.

[33] For instance, El Salvador is considered by Henderson et al. (2018) as *early developed*, yet its figures of inequality have been larger than the yearly median during most of the years considered in this study—1990, 1995-1996, and 1998-2015.

[34] Only 7 out of 116 countries changed denomination: Costa Rica, El Salvador, Gabon, Jamaica, Mauritius, Panama, and Sri Lanka.

a general improvement in their human development after FTA's entry, the regions of the most unequal nations experience deterioration in such human welfare indicators. In sum, inequality is indeed an explanatory factor for the lack of correspondence between the limited benefits brought by FTAs to human development (Table 2.2) and the considerable effects on economic growth (Table 2.3).

2.4 Alternative Answers and Robustness Tests

There are, of course, alternative explanations as to why FTAs impact human development in such a small yet, positive way. In Table 2.6, I delve into some of these potential answers. Diwan and Rodrik (1991) argued that usual legal standards of trade agreements, especially the ones regarding intellectual property rights and patents negotiated in the Uruguay Round, systematically benefit countries of the Global North at the expense of nations of the Global South.[35] Moreover, testing this claim is especially relevant while countries like the United States or supra-regions such as the European Union heavily protect key sectors (e.g., agriculture) in the "free" trade arrangements they enter into (Wise, 2009, 2014; Otero, 2011, Grochowska and Ambroziak, 2018; Grennes, 2018; Kareem et al., 2018). I examine this argument in column 1 and compare FTAs signed with countries of the Global North ($PostNorth_{it-5}$) versus those signed with partners in the Global South ($PostSouth_{it-5}$).[36] If nations of the Global North have systematically captured the benefits of FTAs, differences in the impacts they provoke should be apparent. As can be seen in column 1, the coefficients of interest for $PostSouth_{it-5}$ and $PostNorth_{it-5}$ are positive, yet the effect seems to be weaker for FTAs signed with countries of the Global North. However, the two coefficients are not statistically different from zero, which suggests that the capturing of such benefits is not explained by a differentiation of north and south partners.[37]

Dür et al. (2014) argue that FTAs have become increasingly complex in the last 30 years, including an increasing number of provisions that exceed the usual provisions on tariffs and quotas of past (classic) FTAs, as one can indeed see in Figure 2.6.[38] Some of these additional conditions concentrate on the establishment of shared regulation and law enforcement in sensitive areas such as product standards and intellectual property rights, but it can also contain binding criteria in areas such as services exchange, in-

[35] Regarding such patent and IPP regulations included in FTAs, World Health Organisation Director General Dr. Margaret Chan declared about the Trans-Pacific-Partnership in 2015: "...If these agreements open trade yet close the door to affordable medicines, I have to ask the question: is this really progress at all?..." (Germanos, 2015). Also see Marchetti and Mavroidis (2011) and Sell (2011) for further insight on the consequences of the Uruguay Round for developing countries.

[36] More details on the construction of these variables can be found in Table 2.B.1 in Appendix 2.B.

[37] I also run a test with such division and for the other main results of this study in Table 2.A.13 in the Appendix.

[38] According to Limão (2016), by 2011, 76 percent of existing preferential trade agreements were subject to at least one aspect of investment standardization, 61 percent included intellectual property rights protection, and 46 percent demanded environmental regulations.

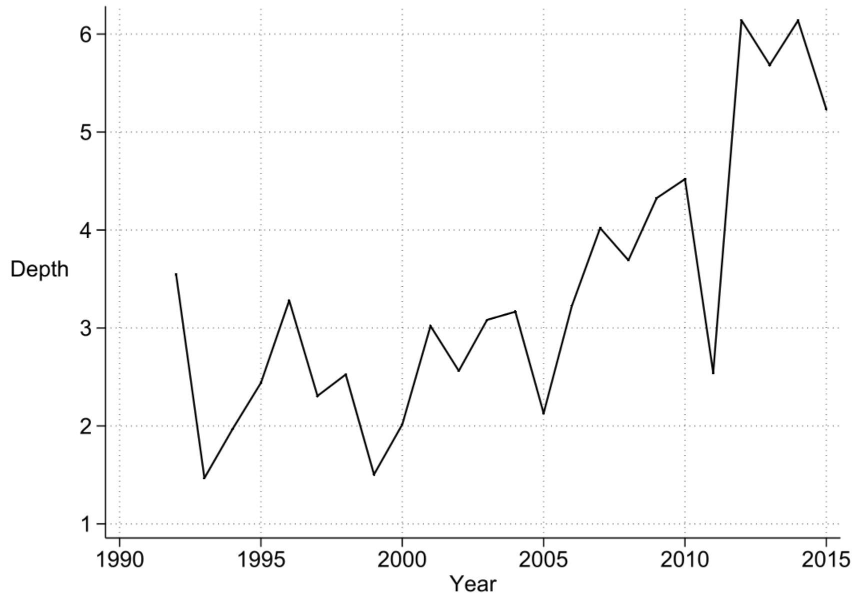


Figure 2.6 FTAs depth evolution

vestments, and rules of competition. Thus, as Rodrik also argued in 2018, a potential explanation for the impact of FTAs on human development might reside in the (depth) type of FTA that a country signs. In other words, the efficiency of an FTA might be defined by the degree of conditionality that is stipulated in such agreements. In column 2, I interact a dummy ($ComplexFTA$) that separates FTAs that, on average, include provisions on tariffs and quotas (FTAs' depth=1) from those which include more than such classic conditions (FTAs' depth≥2), with my main dummies $Treat_i$ and $Post_{t-5}$.

Table 2.5 FTA depth (additive index)

Legal Provision	**Value**
1. More than a partial scope agreement (on goods)	$FTA\ depth = 1$
2. Substantive provision on services and 1.	$FTA\ depth = 2$
3. Substantive provision on investments and 1. to 2.	$FTA\ depth = 3$
4. Substantive provision on standards and 1. to 3.	$FTA\ depth = 4$
5. Substantive provision on public procurement and 1. to 4.	$FTA\ depth = 5$
6. Substantive provision on competition and 1. to 5.	$FTA\ depth = 6$
7. Substantive provision on intellectual property rights and 1. to 6.	$FTA\ depth = 7$
Total range	0-7

Note: Table based on Dür et al. (2014, pp.34)

The results show a positive effect of FTAs that only include provisions on tariffs and quotas, and a rather negative effect for FTAs that go beyond. Similarly, in column 3, I run an extension test in which, instead of using a dummy that separates classic FTAs from more complex ones, uses the total number of *ComplexFTA* signed.[39] Results show that there are no significant difference between the two specifications. The insignificant effects in columns 2 and 3 portray the lack of empirical support for the hypothesis that FTAs' increasing conditionality plays a diminishing role in the FTAs' small yet positive impact on human development.

Table 2.6 FTAs' impact heterogeneity on HDI

	(1) **FTAs North-South**	(2) **FTAs Conditionality**	(3) **Conditionality: Number FTAs**	(4) **Sectoral Heterogeneity**
$Treat_i \times PostSouth_{it-5}$	0.056			
	(0.035)			
$Treat_i \times PostNorth_{it-5}$	0.010			
	(0.022)			
$Treat_i \times Post_{jt-5}$		0.058	-0.0001	0.047**
		(0.064)	(0.0003)	(0.023)
$Treat_i \times Post_{jt-5} \times ComplexFTA_{it-5}$		-0.014	0.0003	
		(0.064)	(0.001)	
$Treat_i \times Post_{jt-5} \times Agriculture_i$				-0.021
				(0.045)
$Treat_i \times Post_{jt-5} \times ManuServ_i$				0.331**
				(0.148)
Observations	449,786	449,786	449,786	449,786
Adjusted R-squared	0.997	0.997	0.997	0.997
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	190	190	190	190
Regions	18,375	18,375	18,375	18,375

Notes: All HDI values are scaled (HDI×100). All columns include country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

I conclude Table 2.6 by exploring another of the most straightforward hypotheses about FTAs' effects on development: sectoral heterogeneities. Otero (2011) and Wise (2009, 2014) argue how the signing of an FTA can compromise the food sovereignty of a country by exposing its agricultural industry to free trade.[40] Similarly, Van den Berg (2012), Hausmann and Hidalgo (2010), and Hidalgo (2015) show how free trade can be particularly beneficial for high-skilled regions that specialize in the provision of services, and that are mostly located in highly-developed cities. Column 4 then

[39] More details on the construction of these variables can be found in Table 2.B.1 in the Appendix 2.B.

[40] Both Wise and Otero use the case of Mexico and the FTA signed with the United States and Canada in 1994—the North American Free Trade Agreement (NAFTA). They show how NAFTA "devastated" the wheat and grain production in Mexico.

poses a sectoral distinction between regions that concentrate on agricultural production $Agriculture_i$ (or that have cropland as the predominant land cover), manufacturing and services $ManuServ_i$ (or that have urban land predominantly), or other regions $Other_i$ that can host productive activities (or that predominantly have natural vegetation or consolidated bare land).[41] In principle this approach would allow for the identification of whether a sector is driving the main results presented in Table 2.2 or not. The results, however, show that, in line with the general results of columns 3 and 6 in Table 2.2, all zone types or sectors experience positive effects on HDI due to the presence of FTAs. Note that the negative coefficient in the row detailing the agricultural sector (column 4) is not statistically different from the base interaction ($Treat_i \times Post_{t-5}$) that captures FTAs' impact in other zones. The positive impact seems to be considerably larger for the manufacturing and services sector as the estimate reports an increase of 0.378 percentage points of human development in regions specialized in such sectors vis-à-vis a 0.047 percentage points increase in the agricultural and other sectors.

[41] As preliminary validation tests, several thresholds of land cover were randomly defined to categorize regions as agricultural, as services and manufacturing, or as other productive activity. Results increasingly change as the cutoffs between land categories become more random.

Table 2.7 FTAs' impact on Human Development: time structure

	T=0	**T=1**	**T=2**	**T=3**	**T=4**	**T=6**	**T=7**	**T=8**	**T=9**	**T=10**	**t-1 - t-10**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	t-T										
$Treat_i \times Post_{jt-5}$	0.053**	0.037	0.070***	0.075***	0.087***	0.104***	0.027	0.065**	0.077**	0.083***	0.119***
	(0.022)	(0.023)	(0.023)	(0.019)	(0.025)	(0.023)	(0.026)	(0.026)	(0.031)	(0.023)	(0.032)
Observations	449,786	449,786	449,786	449,786	449,786	449,786	449,786	449,786	449,786	449,786	449,786
Adjusted R-squared	0.997	0.997	0.997	0.997	0.997	0.997	0.997	0.997	0.997	0.997	0.997
Country-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Countries	190	190	190	190	190	190	190	190	190	190	190
Regions	18,375	18,375	18,375	18,375	18,375	18,375	18,375	18,375	18,375	18,375	18,375

Notes: All HDI values are scaled (HDI×100). All columns include World Bank aid, leaders' birth regions, country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

I next test the time structure of FTAs' impact on human development. While my main analysis uses a $\tau = 5$ for the construction of my $Post_{t-T}$ dummy, a larger or smaller τ is also plausible. FTAs' effective implementation differs greatly from country to country (Stevens et al., 2015) and from FTA to FTA (Diwan and Rodrik, 1991; Rodrik, 2018), and thus, while in some contexts a short/long lag of its impact is conceivable, in others the use of such lag might not be as persuasive. From columns 1 to 11 of Table 2.7, I control for different periods ($Treat_i \times Post_{i,t\text{-}T}$) to assess whether they have a significant role on my impact of interest. As can be seen, the significance of my main variable $Treat_i \times Post_{it\text{-}5}$ is unaltered, even when all activation periods are used in tandem (column 11). The latter results suggest that the small yet positive impact of FTAs is indeed robust in the mid-run ($\tau = 5$).[42]

Table 2.8 Time Robustness Tests

	(1) HDI	(2) HDI	(3) HDI	(4) HDI
$Treat_i \times Post_{jt-5}$	0.053**	0.054**	0.056**	0.058**
	(0.022)	(0.022)	(0.024)	(0.024)
$Treat_i \times Post_{jt}$		-0.011		-0.020
		(0.035)		(0.035)
$Treat_i \times Post_{jt+1}$	0.017	0.019		
	(0.028)	(0.028)		
$Treat_i \times Post_{jt+2}$	-0.040	-0.035		
	(0.046)	(0.047)		
$Treat_i \times Post_{jt+3}$	-0.012	-0.012		
	(0.032)	(0.032)		
$Treat_i \times Post_{jt+5}$			-0.024	-0.015
			(0.029)	(0.031)
Observations	449,786	449,786	449,786	449,786
Adjusted R-squared	0.997	0.997	0.997	0.997
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	190	190	190	190
Regions	18,375	18,375	18,375	18,375

Notes: All HDI values are scaled (HDI×100). All columns include World Bank aid, leaders' birth regions, country-year, and regional fixed effects. Standard errors are clustered at the country-year and regional level and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

In Section 2.2, inspired by Christian and Barrett (2017), I graphically showed in Figure 2.1 that there were no hints pointing towards the presence of pre-trends sys-

[42] To reduce clutter, I did not present the coefficients for the different control periods, yet details are available upon request. Note that, for instance, in the most comprehensive specification detailed in column 11, only the periods with $\tau = 6$ and $\tau = 7$ showed significant coefficients, the former being equal to -0.104% and the latter to 0.088%, which unfortunately does not completely rule out the presence of long-term time heterogeneity effects.

tematically and differentially affecting any of the groups under study. In other words, both exploitable and non-exploitable regions shared parallel trends in their human development indexes before the FTA's activation period. Following Goldsmith-Pinkham et al. (2020), Borusyak et al. (2021) and Borusyak and Hull (2020), I run some tests which include placebo activation periods, i.e., activation periods that correspond to the period pre-FTA, as in $Treat_i \times Post_{i,t+T}$. The placebo variables are not statistically significant and barely affect the efficiency of my main estimator. This suggests that there are indeed no pre-trends threatening the identification of my main effect.

Table 2.9 Other Robustness Tests

			Non-Exploitable region		55 km regions	
	(1)	(2)	(3)	(4)	(5)	(6)
	HDI	**HDI**	**HDI**	**HDI**	**HDI**	**HDI**
$Treat_i \times Post_{jt-5}$	0.098**	0.100**	0.097**	0.100**	0.035*	0.030***
	(0.039)	(0.046)	(0.038)	(0.046)	(0.021)	(0.022)
$Population(log)_{it-1}$		-0.0007		-0.0006		0.004
		(0.006)		(0.006)		(0.004)
Observations	238,453	200,116	220,421	182,365	940,881	721,266
Adjusted R-squared	0.998	0.997	0.997	0.997	0.999	0.999
Controls	YES	YES	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Countries	175	174	100	99	188	185
Regions	16,719	15,216	15,499	14,001	63,504	53,296

Notes: All HDI values are scaled (HDI×100). All columns include temperature, World Bank aid, leaders' birth regions, country-year, and regional fixed effects. Standard errors are clustered at the country-year and regional level and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

I conclude this section by conducting further robustness tests, shown in Table 2.9. For some, the lack of controls, beyond the use of the preferred set of fixed effects, could produce an omitted variables bias. I partly show that this does not seem to be the case in Tables 2.2 and 2.3, yet I run an additional test where, apart from including all geographic and political economy controls described before, a variable on population is also included (columns 2 and 4).[43] In the short-run, different levels of agglomeration in a region can impact its average access to goods and services and consequently inform its human development figures—e.g., high-conflict developing regions. Similarly, one can worry about the composition of the reference group in the sample of the main regression, and specifically on the lack of such reference for countries that only have

[43]Moreover, in Appendix 2.A I include all-controls versions of the main tables (Tables 2.A.12 and 2.A.14) that do not directly use them in the main text. The results in these tests do not qualitatively change the conclusions drawn from the main tables.

exploitable land. For that reason, in columns 3 and 4 I only include countries in the main regression that have at least one non-exploitable region from which comparisons can be drawn directly. As can be seen, there is no major change in relation to the main results of Table 2.2. Finally, in columns 5 and 6 I run tests on different sized subnational regions, as I now construct areas of 55 by 55 km (instead of the 111 km^2 used in my main specifications). Perhaps the preferred grid size used so far is still too large for the study of complex development indicators, as it could obscure the real effects of FTAs. However, the results are qualitatively comparable to the main results as the effects on HDI remain positive but small (≈ 0.03 percentage points). The results for the different robustness tests of Table 2.9 indicate that neither the non-presence of non-exploitable regions in some countries, nor the inclusion of the variable that accounts for short-run agglomeration patterns, nor the size of my units of observation bias the main effects and, therefore, the preferred specifications explored in Tables 2.2 and 2.3 (in column 3) and from Tables 2.4 to 2.8 are robust.

Finally, Figure 2.7 presents an overview of the multiple robustness tests (with-/without controls, fixed effects, heterogeneity tests, trends tests, etc.) conducted in this study. The estimates marked with red correspond to the main results from column 3 of Table 2.2, and columns 1, 3 and 5 of Table 2.3. As can be seen, for the majority of specifications, the effects evidenced earlier hold. That is, the effect of FTAs is positive but small for HDI, positive for night lights and GDP, and rather statistically non-significant for inequality.

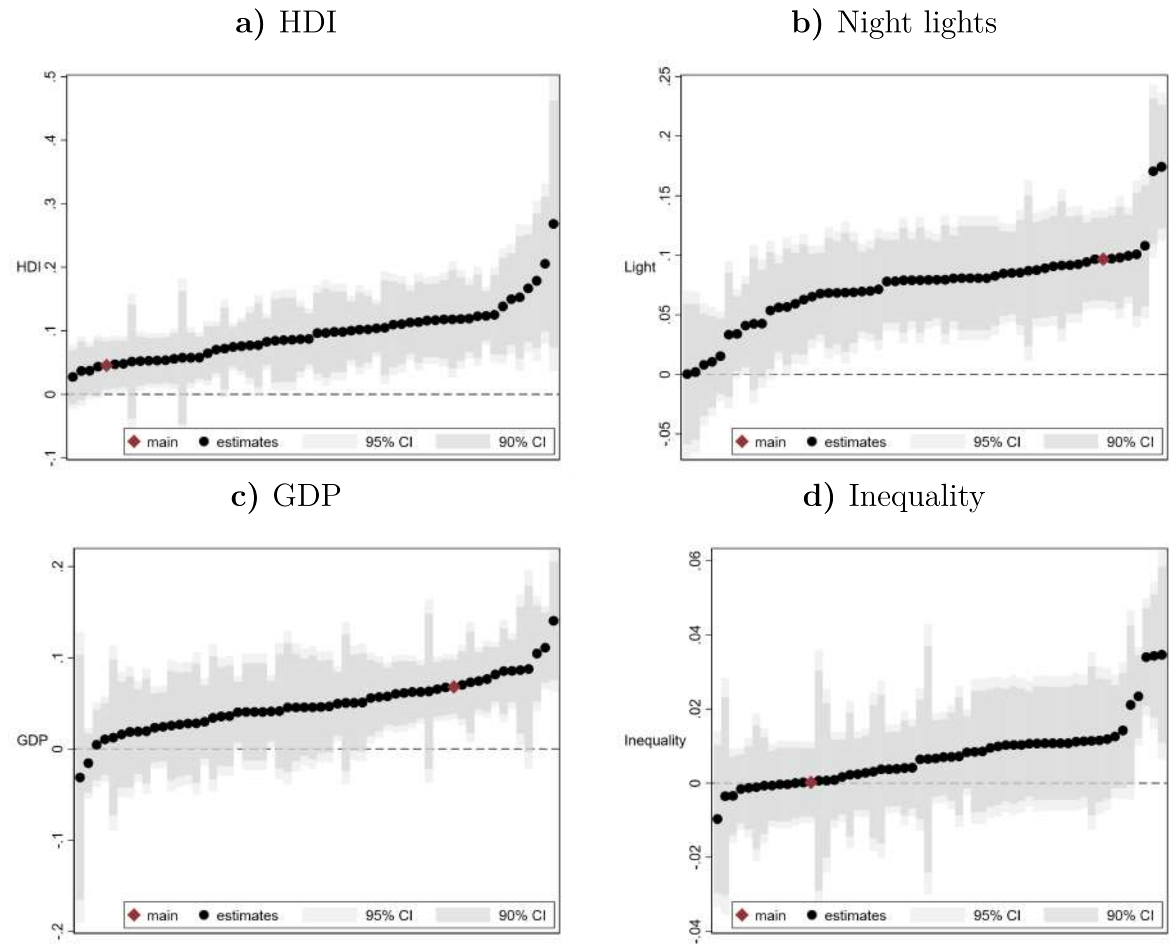


Figure 2.7 Summary of coefficients of interest

Notes: The figure shows the point estimates and their 90% and 95% confidence intervals for the main four variables studied in this chapter: HDI (scaled $\times$ 100)), night lights (log), GDP (log), and inequality. The graphs take into account all the tests explored in this chapter.

2.5 Conclusions

Trade agreements have been one of the most pervasive processes of economic globalization since the GATT meeting in 1947. The importance of trade agreements is not only defined by their ubiquity, but, more importantly, by their capacity to set the rules of the trade game. For instance, the adoption of Free Trade Agreements have remained a recurrent debate in developing and developed regions, as losing in the FTA game has been argued as a globally expected outcome. This potentially negative, global outcome, however, has not hindered the presence of FTAs worldwide. The relative number of more complex forms of FTAs has dramatically increased since 1990, and most economists would still argue that free trade vis-à-vis protectionism is a superior form of economic policy. The question then on why Free Trade Agreements are still a praised process of globalization, and yet provoke such backlash from heterogeneous regions worldwide, remains unanswered. In this paper I explored the impact of FTAs on both human and economic development at the subnational level in order to assess whether views about FTAs can be associated with particular effects of such trade policies on development.

My empirical approach made use of global, high spatial-resolution land cover data which describes the predominant type of land on the surface of subnational areas between 1992 and 2015, and a time-series (1990-2015) national-level proxy of FTAs' depth for a maximum of 207 countries. I interacted the naturally-determined land data with the FTAs indicator to exploit exogenous subnational variation over time, via a difference-in-differences design. My identifying assumption is that other than via the impact of Free Trade Agreements, and conditional on the use of relevant covariates such as country-year and regional fixed effects, development trends in subnational regions with and without naturally (exogenously)-determined land cover should have not been different. In other words, I relied on an identification strategy that used a conditionally exogenous interaction (treatment) to identify the effect of Free Trade Agreements at the subnational level.

My main results show that FTAs' local effect on human development is *small* yet *positive*. I argue that this rather small yet positive impact on human development is best explained by an increase of economic activity that does not alter inequality levels. I test such a mechanism in various ways and show that FTAs have a *positive* impact on economic activity, measured by increases of night light emissions and GDP, and have no impact on inequality, measured by a night light GINI index. I also show that, while for more unequal countries the effects of FTAs on human development are negative vis-à-vis more equal countries, their positive effects on economic activity remain mostly undifferentiated from the ones seen in more equal nations.

FTAs are agreements that involve provisions that differ from dyad to dyad, from agreement to agreement, and from sector to sector. Only focusing on the analysis of average effects would render a limited overview of the phenomenon. Therefore, I looked into impact heterogeneities inspired in such provisions. When looking into a *north-south* partner distinction, I show that while the effect of FTAs signed with countries of the north do have a smaller size, the effect is not statistically different from the one generated by FTAs signed with countries of the south. Also, I looked into the role of added complexity or depth to FTAs when including provisions beyond the usual elimination of tariffs and quotas and show that, while the impact of more complex/deeper FTAs is negative, such impact is not statistically different from that produced by less complex/shallower FTAs. Finally, as part of my main results, I explored sectoral heterogeneities of FTAs' impact. These estimates reveal that urban-associated productive regions perform comparatively better than any other exploitable region.

This work sheds light on the effects of FTAs on different indicators related to development. By doing so, it reconciles the impact of FTAs on human development with economic development by assessing the effects on economic activity and inequality patterns of such FTAs. Moreover, it uses information on most countries of the globe and thus is more generalizable than previous studies that used a limited number of countries with incomparable identification strategies. Moreover, it investigates the subnational effects of FTAs on development, a task in which this work is a pioneer, and that allows for the better understanding of local heterogeneities and causal mechanisms of the FTAs' phenomenon. For policy makers, this piece offers key lessons regarding the conformation and negotiation of FTAs, as it identifies characteristics about the partners, depths, and sectors which FTAs should focus on. More importantly, it offers lessons about the goal indicators to be stressed as tackling existing levels of inequality has been shown in this study to be key to translating increased levels of economic activity into increased levels of human development.

Appendix

2.A Additional Tables

Table 2.A.1 Inverse Hyperbolic Sine Function

	(1)	(2)	(3)	(4)
VARIABLES	IHS GDP	IHS GDP	IHS Light	IHS Light
$Treat_i \times Post_{jt-5}$	0.069***	0.044*	0.083***	0.128***
	(0.016)	(0.026)	(0.013)	(0.023)
Observations	448,021	237,763	389,968	209,911
Adjusted R-squared	0.986	0.986	0.975	0.981
Controls	NO	YES	NO	YES
Country-Year FE	YES	YES	YES	YES
GRID FE	YES	YES	YES	YES
Countries	193	176	200	176
Regions	4,991	2,690	4,399	2,336

Notes: All columns include country-year and regional fixed effects. When specified, the set of controls includes temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.A.2 Water regions as exploitable land

	(1) **HDI**	(2) **Light**	(3) **GDP**	(4) **Inequality**
$Treat_i \times Post_{jt-5}$	0.110***	0.082***	0.039	0.013
	(0.041)	(0.029)	(0.036)	(0.010)
Observations	238,453	209,911	237,763	197,855
Adjusted R-squared	0.998	0.980	0.984	0.692
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	176	176	173
Regions	16,719	16,392	16,722	16,111

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.3 Land-changing regions excluded

	(1) **HDI**	(2) **Light**	(3) **GDP**	(4) **Inequality**
$Treat_i \times Post_{jt-5}$	0.086**	0.079***	0.046	0.010
	(0.040)	(0.028)	(0.033)	(0.009)
Observations	228,124	200,695	227,475	189,081
Adjusted R-squared	0.997	0.980	0.984	0.691
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	176	176	173
Regions	15,997	15,672	16,000	15,397

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.4 Local inequality and local development

	(1) **HDI**	(2) **Light**	(3) **HDI**	(4) **Light**	(5) **HDI**	(6) **Light**
$Treat_i \times Post_{jt-5}$	0.087***	0.068***	0.117***	0.069**	0.150***	0.101***
	(0.031)	(0.026)	(0.035)	(0.030)	(0.044)	(0.031)
$Treat_i \times Post_{jt-5} \times Inequality_{it-1}$	-0.070***	-0.026*				
	(0.016)	(0.015)				
$Treat_i \times Post_{jt-5} \times AvgIneq3y_{it}$			-0.092***	-0.023		
			(0.025)	(0.020)		
$Treat_i \times Post_{jt-5} \times AvgIneq5y_{it}$					-0.103***	-0.043**
					(0.034)	(0.021)
Observations	339,361	336,057	280,724	277,213	231,181	227,663
Adjusted R-squared	0.997	0.975	0.997	0.976	0.997	0.977
Controls	YES	YES	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Countries	184	185	184	185	184	185
Regions	17,157	17,169	17,157	17,167	17,099	17,025

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.5 Generalized DID

VARIABLES	(1) HDI	(2) Light	(3) GDP	(4) Inequality
$Treat_i \times Post_{jt-5}$	0.048**	0.034*	0.011	0.007
	(0.022)	(0.019)	(0.024)	(0.006)
Observations	449,786	389,968	448,021	358,031
Adjusted R-squared	0.997	0.972	0.985	0.706
Controls	NO	NO	NO	NO
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	190	200	193	185
Regions	18,375	18,046	18,400	17,169

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.6 Number of FTAs

	(1) **HDI**	(2) **Light**	(3) **GDP**	(4) **Inequality**
$Treat_i \times FTANumber_{it-5}$	0.0003	-0.0004**	-0.0008***	0.0000
	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Observations	238,453	209,911	237,763	197,855
Adjusted R-squared	0.9975	0.9804	0.9843	0.6925
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	176	176	173
Regions	16,719	16,392	16,722	16,111

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.7 Depth of FTAs

	(1) **HDI**	(2) **Light**	(3) **GDP**	(4) **Inequality**
$Treat_i \times FTADepth_{it-5}$	0.007	0.007	-0.006	0.003
	(0.007)	(0.005)	(0.005)	(0.002)
Observations	238,453	209,911	237,763	197,855
Adjusted R-squared	0.998	0.980	0.984	0.692
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	176	176	173
Regions	16,719	16,392	16,722	16,111

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.8 Big vs. Small countries

	(1) **HDI**	(2) **Light**	(3) **GDP**	(4) **Inequality**
$Treat_i \times Post_{jt-5}$	0.208*	0.055	0.102	0.001
	(0.113)	(0.048)	(0.091)	(0.031)
$Treat_i \times Post_{jt-5} \times Big_{jt}$	-0.115	0.027	-0.064	0.010
	(0.114)	(0.047)	(0.086)	(0.031)
Observations	238,453	209,911	237,763	197,855
Adjusted R-squared	0.998	0.980	0.984	0.692
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	176	176	173
Regions	16,719	16,392	16,722	16,111

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.9 Main results with comparable samples

	1992-2013				*Non-missing data*			
	(1) **HDI**	(2) **Light**	(3) **GDP**	(4) **Inequality**	(5) **HDI**	(6) **Light**	(7) **GDP**	(8) **Inequality**
$Treat_i \times Post_{jt-5}$	0.100***	0.081***	0.047	0.011	0.102**	0.085***	0.019	0.010
	(0.000)	(0.027)	(0.031)	(0.009)	(0.000)	(0.027)	(0.037)	(0.009)
Observations	206,047	209,911	205,702	197,855	178,159	178,159	178,159	178,159
Adjusted R-squared	0.998	0.980	0.985	0.692	0.997	0.980	0.980	0.693
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES
Countries	173	176	176	173	171	171	171	171
Regions	16,555	16,392	16,568	16,111	15,899	15,899	15,899	15,899

Notes: All HDI values are scaled (HDI×100). Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.10 Inequality: Winsorized ranges

	(1) **LumenGini: Population split**	(2) **LumenGini: 0.10-0.90**	(3) **LumenGini: 0.20-0.80**	(4) **LumenGini: 0.201-0.659**
$Treat_i \times Post_{jt-5}$	0.015	0.013	0.015	0.017*
	(0.011)	(0.012)	(0.010)	(0.010)
$Treat_i \times Post_{jt-5} \times BigGrid_{it}$	-0.007			
	(0.009)			
Observations	197,855	75,177	52,381	43,283
Adjusted R-squared	0.693	0.402	0.550	0.478
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	173	163	157	154
Regions	16,111	8,431	7,565	6,687

Notes: All columns include country-year and regional fixed effects. When specified, the set of controls includes temperature, World Bank aid, leaders' birth regions. Column 1 also controls for the $BigGrid_{it}$ dummy, defined as the regions that have a population above the regional median. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.11 Inequality and GDP

	Local Inequality	*Historic Inequality*	*Year-to-year Inequality*	*LAC vs.* The World	*Henderson's Early-Late* Adjusted	*Henderson's Early-Late:*
	(1)	(2)	(3)	(4)	(5)	(6)
	GDP	**GDP**	**GDP**	**GDP**	**GDP**	**GDP**
$Treat_i \times Post_{jt-5}$	0.019	0.086***	0.105***	0.082***	0.056**	0.111***
	(0.028)	(0.021)	(0.032)	(0.018)	(0.025)	(0.023)
$Treat_i \times Post_{jt-5} \times AvgIneq3y_{it}$	-0.041**					
	(0.018)					
$Treat_i \times Post_{jt-5} \times Unequal_j$		-0.038				
		(0.037)				
$Treat_i \times Post_{jt-5} \times Unequal_{jt}$			-0.058			
			(0.050)			
$Treat_i \times Post_{jt-5} \times LAC_j$				-0.257***		
				(0.081)		
$Treat_i \times Post_{jt-5} \times LateDev_j$					0.089**	-0.033
					(0.035)	(0.035)
Observations	278,790	408,193	192,765	448,021	302,883	302,883
Adjusted R-squared	0.977	0.983	0.982	0.985	0.983	0.983
Country-Year FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Countries	184	157	143	193	116	116
Regions	17,157	16,731	16,330	18,400	12,346	12,346

Notes: GDP values are logged. All columns include country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.12 FTAs' impact heterogeneity on HDI with all controls

	(1) **FTAs North-South**	(2) **FTAs Conditionality**	(3) **Conditionality: Number FTAs**	(4) **Sectoral Heterogeneity**
$Treat_i \times PostSouth_{jt-5}$	0.118***			
	(0.030)			
$Treat_i \times PostNorth_{jt-5}$	-0.009			
	(0.050)			
$Treat_i \times Post_{jt-5}$		0.268**	0.0002	0.077*
		(0.118)	(0.0003)	(0.040)
$Treat_i \times Post_{jt-5} \times ComplexFTA_{it-5}$		-0.207*	0.0015	
		(0.120)	(0.0028)	
$Treat_i \times Post_{jt-5} \times Agriculture_i$				0.137***
				(0.051)
$Treat_i \times Post_{jt-5} \times ManuServ_i$				0.106
				(0.069)
Observations	238,453	238,453	238,453	238,453
Adjusted R-squared	0.998	0.998	0.9975	0.998
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	175	175	175
Regions	16,719	16,719	16,719	16,719

Notes: All HDI values are scaled (HDI×100). All columns include country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.13 North vs. South

	(1) HDI	(2) Light	(3) GDP	(4) Inequality
$Treat_i \times PostSouth_{jt-5}$	0.108***	0.027	0.038	0.014
	(0.029)	(0.026)	(0.030)	(0.010)
$Treat_i \times PostNorth_{jt-5}$	-0.002	0.064**	0.010	-0.002
	(0.049)	(0.031)	(0.043)	(0.010)
Observations	238,453	209,911	237,763	197,855
Adjusted R-squared	0.998	0.980	0.984	0.692
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Countries	175	176	176	173
Regions	16,719	16,392	16,722	16,111
North vs. South (p-value)	0.06	0.46	0.66	0.431

Notes: All HDI values are scaled (HDI×100), Light and GDP are logged. All columns include country-year and regional fixed effects, and control for temperature, World Bank aid, Leaders' birth regions. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.14 Mechanism: Inequality with all controls

	Local Inequality		*Historic Inequality*		*Year-to-year Inequality*		*Henderson's Early-Late*		*Henderson's Early-Late: Adjusted*	
	(1) **HDI**	(2) **Light**	(3) **HDI**	(4) **Light**	(5) **HDI**	(6) **Light**	(7) **HDI**	(8) **Light**	(9) **HDI**	(10) **Light**
$Treat_i \times Post_{jt-5}$	0.167***	0.108***	0.206***	0.087**	0.118**	0.000	0.052	0.042	0.116***	0.008
	(0.051)	(0.031)	(0.064)	(0.039)	(0.047)	(0.036)	(0.054)	(0.034)	(0.035)	(0.026)
$Treat_i \times Post_{jt-5} \times AvgIneq3y_{it}$	-0.121***	-0.045**								
	(0.031)	(0.022)								
$Treat_i \times Post_{jt-5} \times Unequal_j$			-0.206***	-0.009						
			(0.077)	(0.054)						
$Treat_i \times Post_{jt-5} \times Unequal_{jt}$					-0.073*	0.054*				
					(0.041)	(0.031)				
$Treat_i \times Post_{jt-5} \times LateDev_j$							0.025	0.053	-0.067	0.092**
							(0.071)	(0.048)	(0.054)	(0.041)
Observations	181,224	174,491	228,251	200,866	127,001	112,523	168,947	147,822	168,947	147,822
Adjusted R-squared	0.997	0.998	0.998	0.981	0.997	0.979	0.997	0.984	0.997	0.984
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Countries	171	171	152	153	130	128	115	115	115	115
Regions	15,907	15,801	15,994	15,663	14,755	14,150	11,836	11,588	11,836	11,588

Notes: LAC computations are not included due lack of variation. All HDI values are scaled (HDI×100), Light values are logged. All columns include country-year and regional fixed effects. Standard errors are clustered at the country-year and regional level, and are shown in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.15 Trade and GDP: Country level

VARIABLES	(1) Total Trade	(2) Total Trade
GDP_WB	0.417***	
	(0.033)	
GDP_GIS		0.404***
		(0.060)
Observations	3,994	3,994
Adjusted R-squared	0.968	0.944
Countries	115	115
Controls	YES	YES
Country FE	YES	YES
Year FE	YES	YES

Notes: Column (1) use data of the World Bank indicators database. Column (2) uses GDP geo-referenced information of Kummu et al. (2019) that was aggregated at the national level. All columns include logged population, country and year fixed effects. Standard errors are clustered at the country and year level, and are detailed in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.16 Light and Light GINI

VARIABLES	Inequality	Inequality
Light (log)	-0.051***	
	(0.0002)	
Population (log)		-0.033***
		(0.0002)
Constant	0.546***	0.783***
	(0.0008)	(0.001)
Observations	358,432	316,289
Adjusted R-squared	0.108	0.086

Note: *** p<0.01, ** p<0.05, * p<0.1

2.B Variable Descriptives

Table 2.B.1 Variables and sources

Variable	Definition	Source
HDI	Average human development index in region i times 100.	Own construction based on Kummu et al. (2018)
Light	The logarithm of the yearly average of night-light emissions within region i.	NOAA (2015)
Light (IHS)	The inverse hyperbolic sine of the yearly average of night-light emissions within region i.	NOAA (2015)
GDP	The logarithm of the yearly average of GDP within region i.	Kummu et al. (2018)
Inequality	Gini-like coefficient that represents the yearly distribution of nightlight among the population within region i.	Own generation based on the work by Elvidge et al. (2012)
Treat	Dummy=1 indicating if region i has, predominantly, exploitable land.	Own construction based on ESA (2017)
Agriculture	Dummy=1 indicating if region i has, predominantly, agricultural land.	Own construction based on ESA (2017)
ManuServ	Dummy=1 indicating if region i has, predominantly, urban land.	Own construction based on ESA (2017)
Other	Dummy=1 indicating if region i has, predominantly, other exploitable land.	Own construction based on ESA (2017)
Post	Dummy=1 if country j in year t is in the post-FTA's treatment period.	Own construction based on Dür et al. (2014)
PostNorth	Dummy=1 if country j in year t is in the post-FTA's treatment period, considering only FTAs signed with early-developed countries (as defined by Henderson et al., 2018).	Own construction based on Dür et al. (2014)
PostSouth	Dummy=1 if country j in year t is in the post-FTA's treatment period, considering only FTAs signed with late-developed countries (as defined by Henderson et al., 2018).	Own construction based on Dür et al. (2014)
Post (number)	Total number of FTAs that country j has signed in year t.	Own construction based on Dür et al. (2014)
Complex	Dummy=1 if country j in year t has signed on average FTAs with provisions beyond tariffs and quotas.	Own construction based on Dür et al. (2014)
ComplexFTA (number)	Total number of FTAs with provisions beyond tariffs and quotas that country j has signed in year t.	Own construction based on Dür et al. (2014)
Unequal	Dummy=1 if country j is above the historic world median of income inequality, measured by the income GINI.	Own construction based on World Bank (2020)
Unequal (year)	Dummy=1 if country j is above the yearly world median of income inequality, measured by the income GINI. income GINI.	Own construction based on World Bank (2020)
LAC	Dummy=1 if country j is Latin-American.	Own construction
LateDeveloped	Dummy=1 if country j is considered late-developed in Henderson et al. (2018).	Henderson et al. (2018)
LateDeveloped (adj.)	Dummy=1 if country j is in Henderson et al. (2018) sample and is above the historic median of inequality.	Own construction based on Henderson et al. (2018) and World Bank (2020)
Big	Dummy=1 if country j is above the yearly world median of GDP.	Own construction based on World Bank (2020)
Leader	Yearly dummy indicating whether the presidential leader of country j was born within region i.	Hodler and Raschky (2014)
WB Aid	The yearly disbursed World Bank aid in region i.	AidData (2017)
Temperature	Yearly mean temperature in region i.	Tollefsen et al. (2012)
Distance to Capital	The average distance in kilometers to capital city from region i.	Tollefsen et al. (2012)
Population (log)	The logarithm of the total, yearly population in region i.	Goldewijk et al. (2010, 2011)

Table 2.B.2 Descriptive Statistics

	N	Mean	S.D.	Min	Max
HDI	449,786	70.57	16.00	20.80	100
Light	356,464	-2.181	2.507	-4.605	4.141
Light (IHS)	356,464	0.619	0.998	0	4.834
GDP	422,951	11.10	7.701	-4.605	24.22
GDP (IHS)	422,951	12.46	6.361	0	24.92
Inequality	339,121	0.654	0.389	0	1.000
Treat	449,786	0.813	0.390	0	1
Agriculture	449,786	0.0839	0.277	0	1
ManuServ	449,786	0.00133	0.0364	0	1
Other	449,786	0.728	0.445	0	1
Post	449,786	0.548	0.498	0	1
PostNorth	449,786	0.494	0.500	0	1
PostSouth	449,786	0.323	0.468	0	1
Post (number)	449,786	2.968	1.426	0	154
Complex	449,786	0.467	0.499	0	1
ComplexFTA (number)	449,786	1.621	7.053	0	100
Unequal	409,862	0.612	0.487	0	1
Unequal (year)	194,714	0.611	0.487	0	1
LAC	449,786	0.113	0.316	0	1
LateDeveloped	304,221	0.4791418	.4995656	0	1
LateDeveloped (adj.)	304,221	0.6731225	.4690728	0	1
Big	449,786	0.836	0.370	0	1
Leader	449,786	0.00535	0.0730	0	1
WB Aid	449,786	56,477.88	1,206,681	0	2.85e+08
Temperature	238,538	10.600	13.922	-24.619	55.993
Distance to capital	420,262	1,753	1,616	13.92	7,942
Population (log)	360,104	5.019	3.498	-10.54	14.25

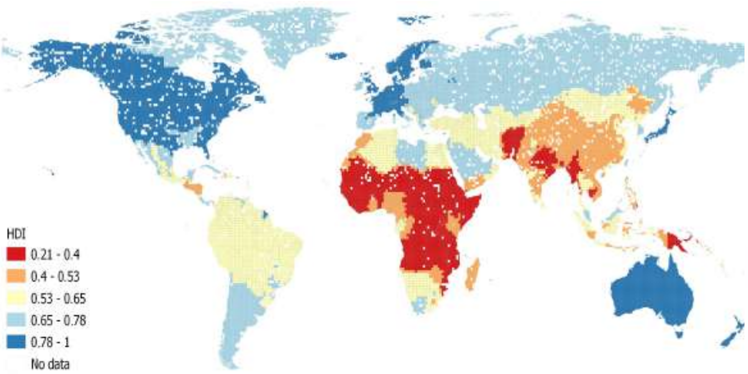
Table 2.B.3 Land Cover categories

NB_LAB	LC Label	NB_LAB	LC
0	No data	110	Mosaic herbaceous cover (>50%) / tree and shrub (<50%)
10	Cropland, rainfed	120	Shrubland
11	Herbaceous cover	121	Shrubland evergreen
12	Tree or shrub cover	122	Shrubland deciduous
20	Cropland, irrigated or post-flooding	130	Grassland
30	Mosaic cropland (>50%) / natural vegetation (tree, shrub, herbaceous cover) (<50%)	140	Lichens and mosses
40	Mosaic natural vegetation (tree, shrub, herbaceous cover) (>50%) / cropland (<50%)	150	Sparse vegetation (tree, shrub, herbaceous cover) (<15%)
50	Tree cover, broadleaved, evergreen, closed to open (>15%)	151	Sparse tree (<15%)
60	Tree cover, broadleaved, deciduous, closed to open (>15%)	152	Sparse shrub (<15%)
61	Tree cover, broadleaved, deciduous, closed (>40%)	153	Sparse herbaceous cover (<15%)
62	Tree cover, broadleaved, deciduous, open (15-40%)	160	Tree cover, flooded, fresh or brakish water
70	Tree cover, needleleaved, evergreen, closed to open (>15%)	170	Tree cover, flooded, saline water
71	Tree cover, needleleaved, evergreen, closed (>40%)	180	Shrub or herbaceous cover, flooded, fresh/saline/brakish water
72	Tree cover, needleleaved, evergreen, open (15-40%)	190	Urban areas
80	Tree cover, needleleaved, deciduous, closed to open (>15%)	200	Bare areas
81	Tree cover, needleleaved, deciduous, closed (>40%)	201	Consolidated bare areas
82	Tree cover, needleleaved, deciduous, open (15-40%)	202	Unconsolidated bare areas
90	Tree cover, mixed leaf type (broadleaved and needleleaved)	210	Water bodies
100	Mosaic tree and shrub (>50%) / herbaceous cover (<50%)	220	Permanent snow and ice

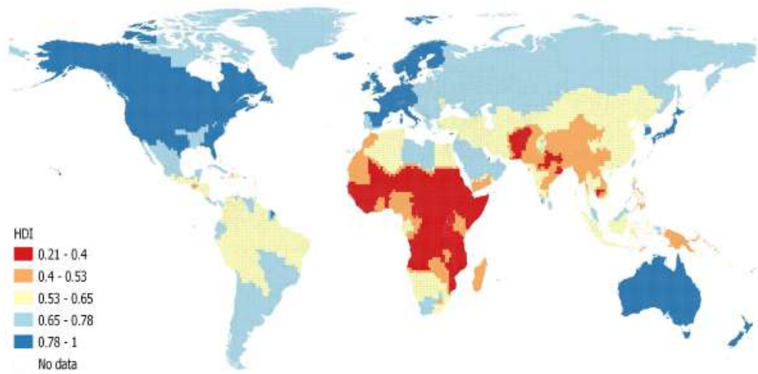
Note: Taken from Land Cover ESACCI-LC-Legend datafile (ESA CCI, 2017)

2.C Visual Descriptives

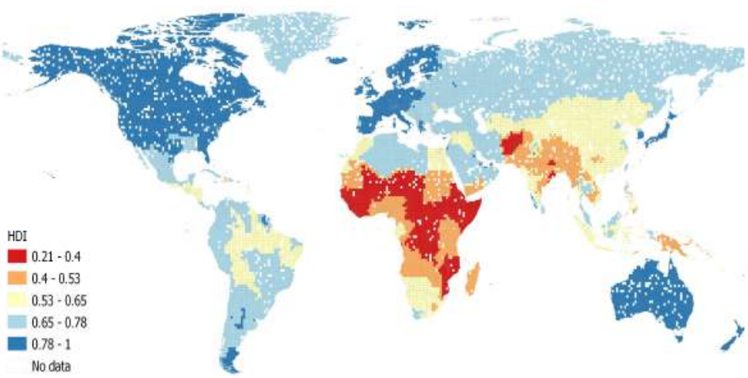
a) 1992



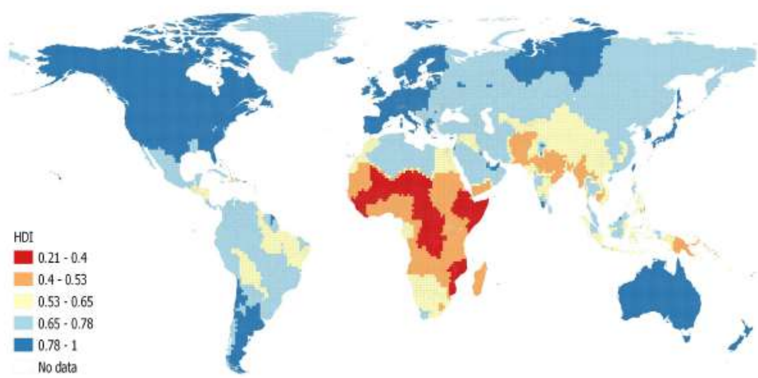
b) 1997



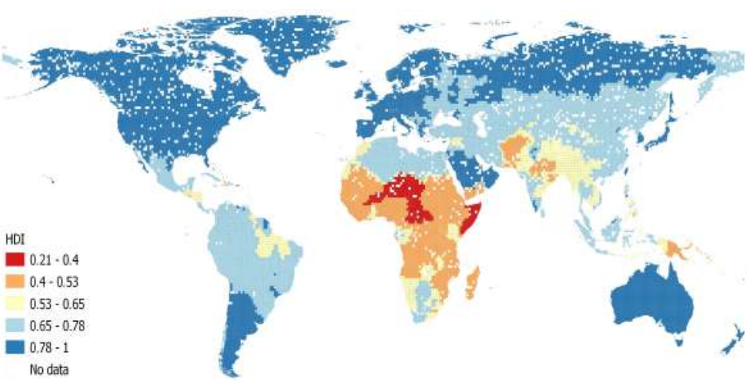
c) 2002



d) 2007



e) 2012



f) 2013

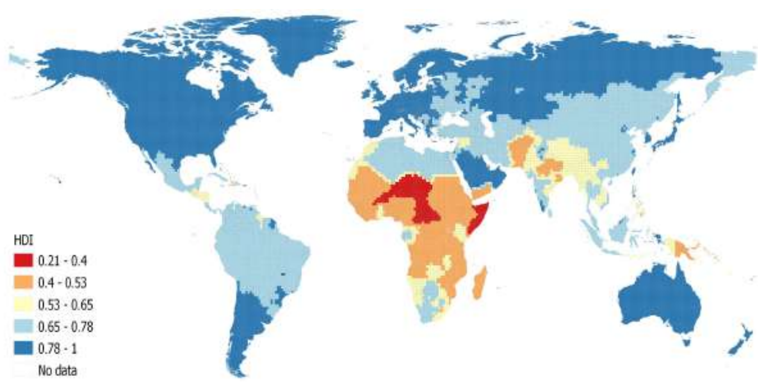


Figure 2.C.1 Gridded HDI over time

Notes: The dimensions of grids are of, roughly, 111 km by 111 km. The grids are clipped to land, at the ADM1 level.

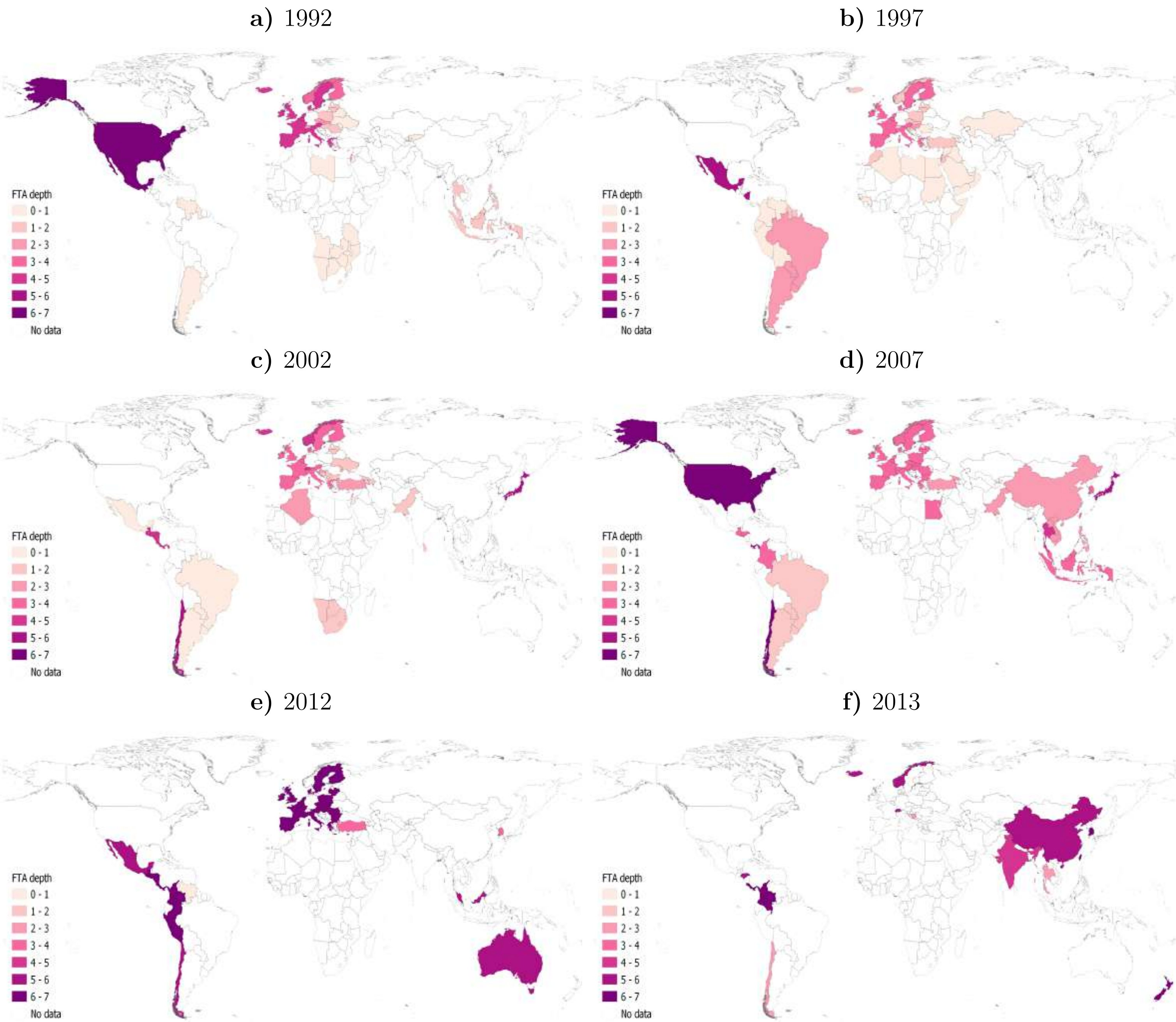


Figure 2.C.2 Gridded FTA depth over time

Notes: The dimensions of grids are of, roughly, 111 km by 111 km. The grids are clipped to land, at the ADM1 level.

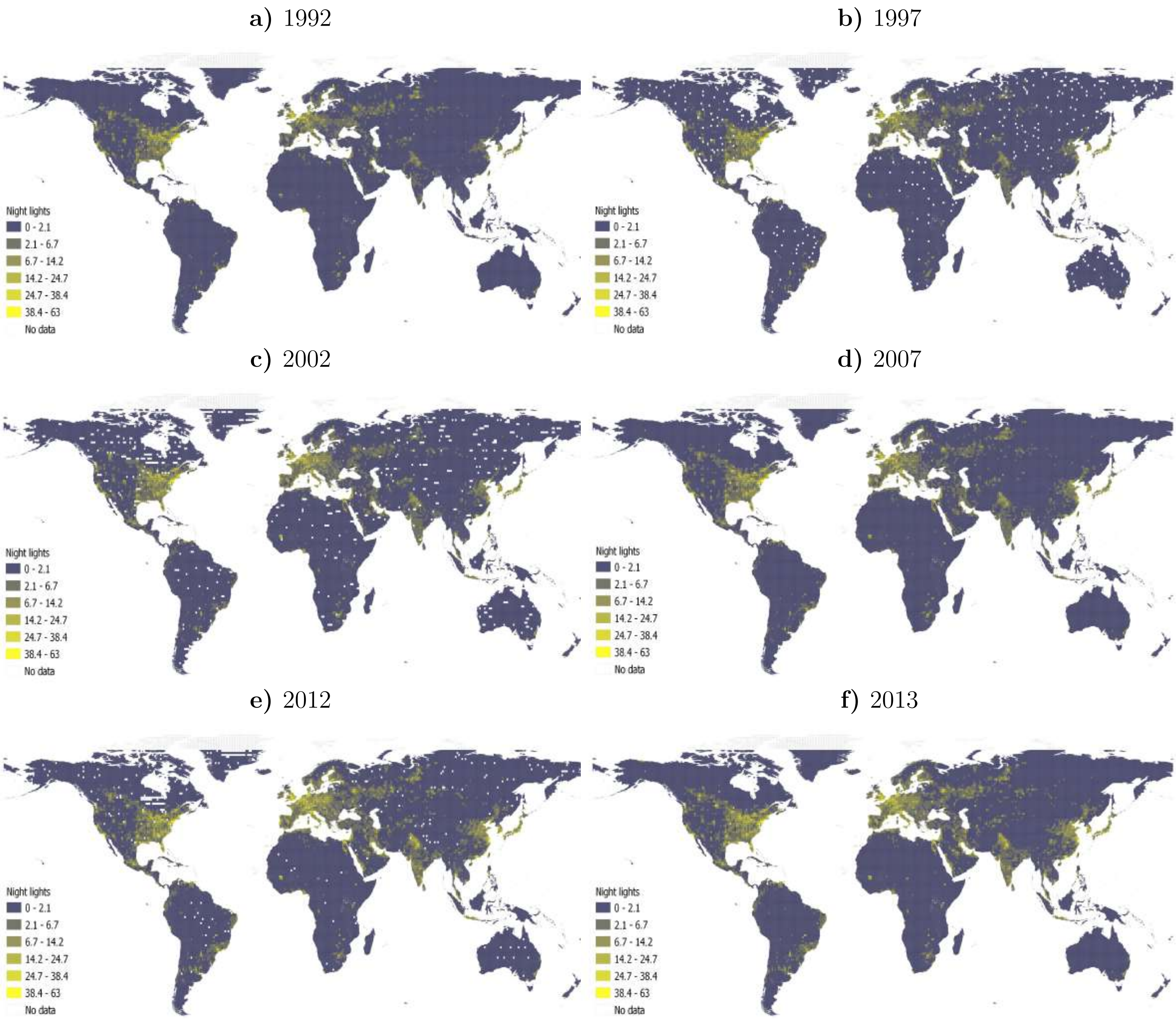
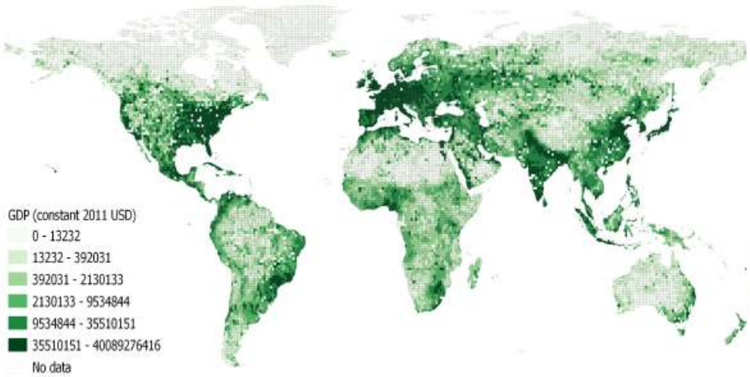


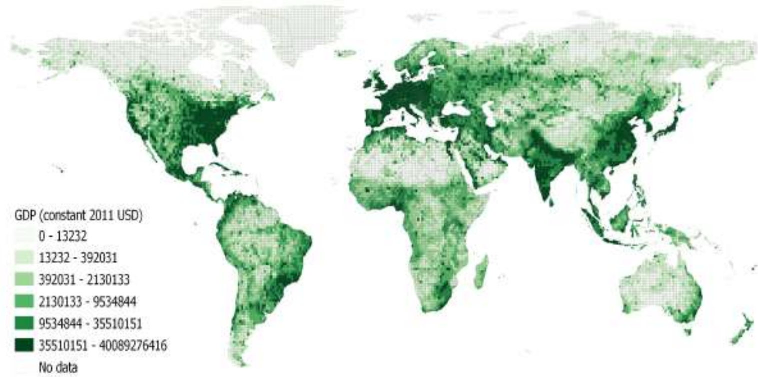
Figure 2.C.3 Gridded night lights over time

Notes: The dimensions of grids are of, roughly, 111 km by 111 km. The grids are clipped to land, at the ADM1 level.

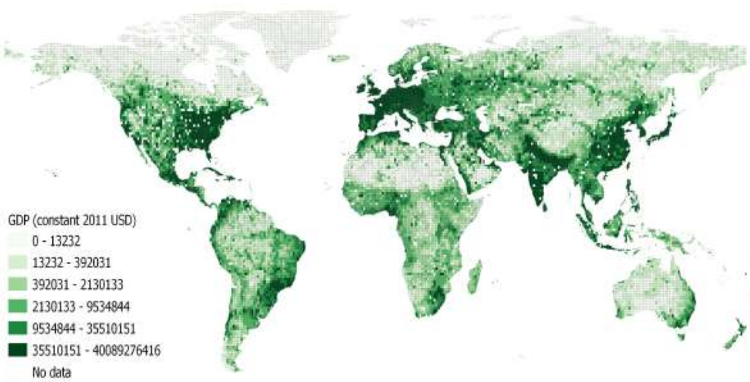
a) 1992



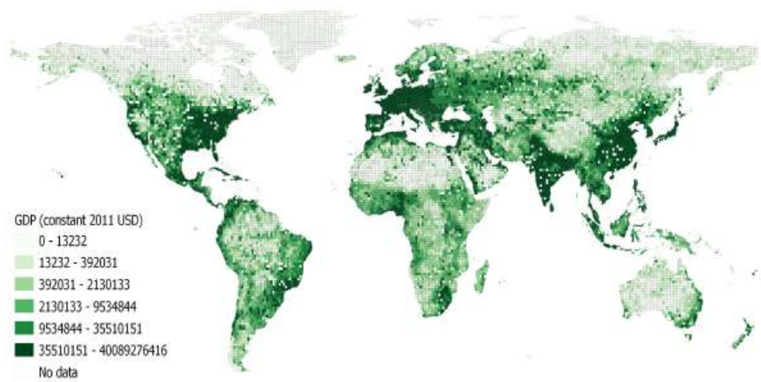
b) 1997



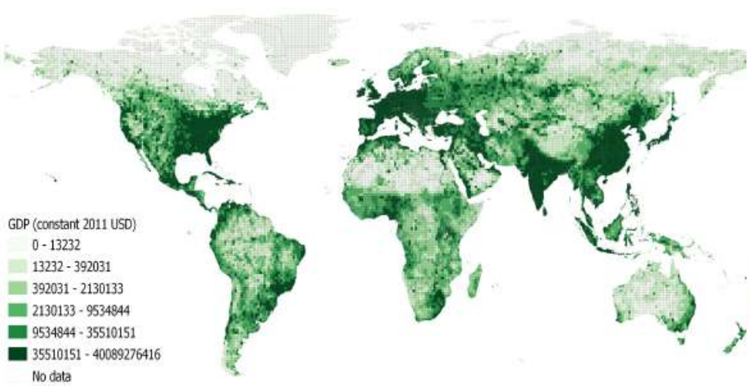
c) 2002



d) 2007



e) 2012



f) 2013

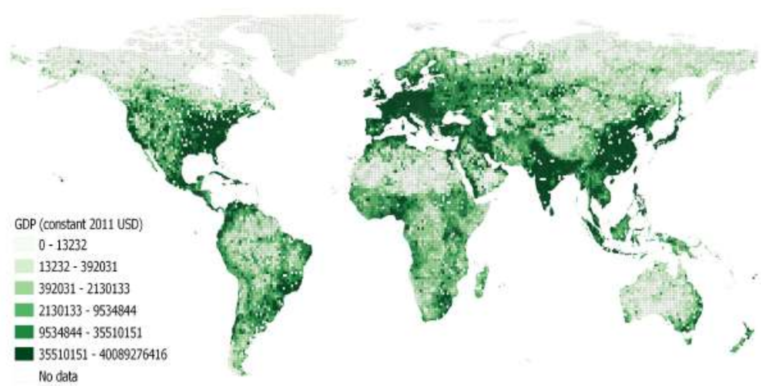


Figure 2.C.4 Gridded GDP over time

Notes: The dimensions of grids are of, roughly, 111 km by 111 km. The grids are clipped to land, at the ADM1 level.

a) 1992

b) 1997

c) 2002

d) 2007

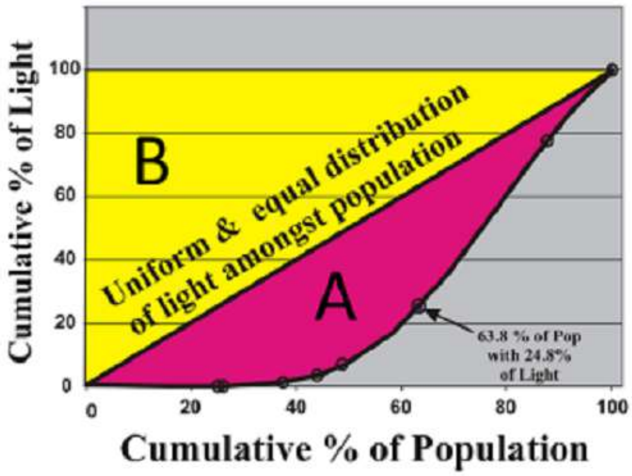
e) 2012

f) 2013

Figure 2.C.5 Gridded inequality over time

Notes: The dimensions of grids are of, roughly, 111 km by 111 km. The grids are clipped to land, at the ADM1 level.

2.D Additional Figures



Notes: Graph drawn from (Elvidge et al., 2012, p. 25). This local quasi-GINI coefficient ranges between 0 and 1 as a result of the ratio A/B, where A is the area between the line of perfect equality (diagonal) and a Lorenz curve that plots the cumulative distribution of night lights against the cumulative distribution of population, and B, which quantifies the area above the diagonal of perfect equality (being this area equal to 0.5).

Figure 2.D.1 Inequality of night light

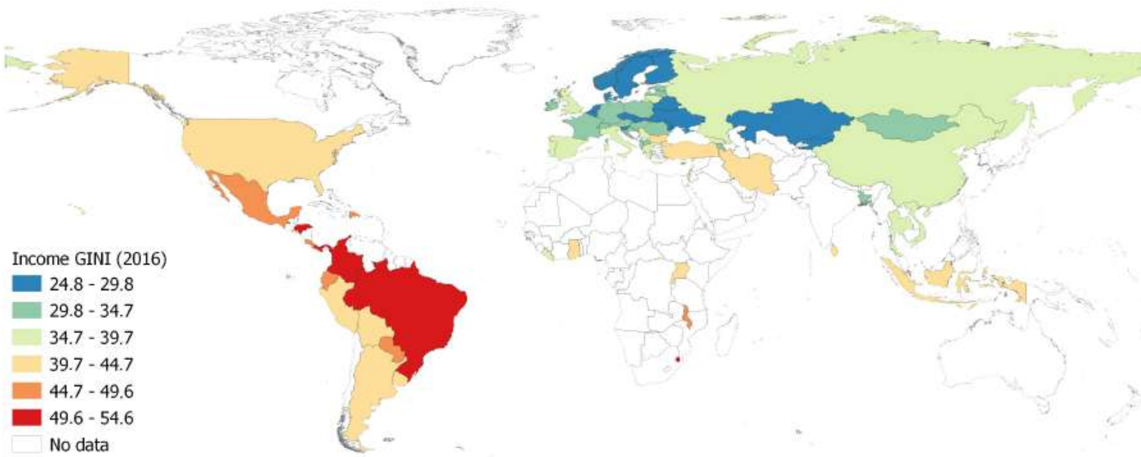


Figure 2.D.2 Inequality Map for 2016

Notes: The figure shows the scaled ($\times 100$) income GINI coefficients per country for the year 2016. The GINI coefficient is an indicator of inequality that ranges between 0 and 1, where the closest to 1 is more unequal and the closest to 0 the more equal.

Chapter 3

Geography, Power, and Development in LAC

Joint work with Christian Bjørnskov and Andrea Sáenz De Viteri

Abstract

While formal institutions are considered rather stable in Western countries, the same can not be said of those in Latin America and the Caribbean (LAC). In this context, although less obvious, patterns of favoritism and rent-seeking can be observed among political elites. This paper explores the degree to which the development of subnational regions is affected by their geographic proximity to parliament leaders' birthplaces, and how this might arise from the *de facto* influence given by the unstable *de jure* frameworks of LAC countries. We collected data on 366 political leaders and 238 distinct birth locations over the 1992–2016 period and constructed a panel of approximately 183,000 uniformly distributed subnational micro-regions across 45 countries and autonomous territories of the LAC region. Our results show that parliament leaders hold significant power to divert resources to regions in the vicinity of their birthplaces, as measured by increases in night light emissions and World Bank aid. We find that this favoritism is mostly informed by the de jure and de facto influence given to the parliament by novel constitutions.

3.1 Introduction

Political favoritism and pork-barrel politics are phenomena that have, arguably, existed as long as civil societies themselves. The Roman historian Tacitus mentioned widespread favoritism as one of the main problems of the early empire under Augustus, and pork-barrel politics have, for instance, been a consistent feature of US politics since at least the 19th century (Shepsle and Weingast, 1981). As national accounts of data are imprecise in most developing countries, and subnational accounts of development often do not exist, Hodler and Raschky (2014) instead use levels in light intensity at night in their seminal study of favoritism. Thus, apart from exposing the significantly higher levels of night light in leaders' birth regions, they find preliminary evidence that increased inflows of Official Development Assistance (ODA) in a country typically result in more economic activity in the home region of the country's president, suggesting aid is being used as a specific channel of favoritism. Dreher et al. (2019a) repeat the exercise using local level data of World Bank and Chinese aid instead. By focusing on inflows in African countries, they find substantial evidence that Chinese aid is diverted to leaders' home regions. Favoritism, however, is not a problem unique to developing countries. In modern political systems, favoritism is often associated with the (mis-)use of political power to benefit particular industries or particular regions. Aghion et al. (2010), for example, document that when a congressman joins the Appropriations Committee—responsible for allocating funds for research university expenditure—their state receives larger shares of federal university funds in subsequent years. Such mechanisms also operate at the supranational level in the UN Security Council (Vreeland and Dreher, 2014) as well as at local levels, as Carozzi and Repetto (2016) show for Italy. The latter work documents that municipal governments receive larger government transfers when legislators are born there, even when they are not elected in those municipalities.

Literature in this field typically focuses on heads of state or government—the former in the form of presidents in presidential systems and the latter as prime ministers in parliamentary ones. The bench-marking work by Hodler and Raschky (2014) looked at executive branch leaders of 126 countries, 21 of those countries being from the Americas. They did not find conclusive results for the Americas.[1] These results might not come as a surprise, however, given that political systems in Latin America and the Caribbean (LAC) have very influential leaders in alternative centers of power from those, for instance, in the parliament. Furthermore, while constitutions and basic institutions delimiting governance are very stable in Western countries, those in LAC countries change substantially over time. Ecuador, for instance, has had 20 constitutions since its formal independence from the Spanish empire in 1830, averaging a remarkable 9.5 years

[1] As the results in the work of Hodler and Raschky (2014) indicate, when categorizing by continent, leaders' birth regions have a non-significant coefficient of zero. More than doubling their sample size for the LAC region, our results, later detailed in Section 3.3, show that the effect for executive branch leaders is non-significant in general, yet negative for the regions that have been a "leader region" before.

per constitution. Some of the consequences of this institutional instability come in the form of, a priori, ephemeral *de jure* power residing in various political actors and, thus, rather precarious *de facto* influence. Indeed, this may imply that exercises of favoritism cannot become entrenched in particular political elites, yet the institutional instability of the region has brought about other consequences. One of the most important being the constant tension between the executive and the legislative.

Two of the many anecdotes of the region portray this tension well. On the one hand, the former Ecuadorian executive branch leader Rafael Correa has repeatedly argued that "... to win the presidency is not to win [discretionary] power [over national affairs]. There are several de facto powers that have informed, historically, our economic and public policy... " Fundamedios (2007). Correa was thereby referring to the de facto power over key economic and political decisions historically held by the Ecuadorian Parliament,[2] which he claimed needed to be rebalanced in order to improve the country's usually poor economic performance. On the other hand, in recent years parliament leaders in Venezuela have publicly challenged the power of President Nicolas Maduro. Maduro and his predecessor, among other things, have been accused of enriching their families and home regions (Baverstock and Foster, 2013). Most notably, however, and as recently as 2019, the leader of the national assembly Juan Guaidó reacted to an allegedly rigged election—by the Maduro regime—and declared himself interim president of Venezuela, arguing that the constitution in such situations grants him the power to do so. These examples, besides illustrating the very common tension between the executive and legislative branches in LAC countries, illustrate the significant influence the leaders of the legislature can have in the region. Thus, while the direct and quite visible favoritism and rent-seeking of heads of state may be pronounced elsewhere (Hodler and Raschky, 2014; Dreher et al., 2019a), the typical unstable allocation of de jure power in the region leaves substantial de facto power in the hands of party or faction leaders. A hitherto unexplored phenomenon is the regional favoritism enacted by parliament leaders of Latin America and the Caribbean.

While favoritism occurs at different levels and in different manifestations, it can take two basic forms. First, politicians can favor specific regions or groups of voters with subsidies or other forms of policy concessions in order to buy votes in upcoming elections (Cox and McCubbins, 1986; Dahlberg and Johansson, 2002; Dixit and Londregan, 1996), receive direct campaign or party support, or invite bribes or less direct forms of support (Cox and McCubbins, 2007; Bertelli and Grose, 2009; Berry et al., 2010). Second, politicians can also engage in *pure* favoritism in the form of policies or projects that directly benefit their family, friends, and immediate network (Bates, 1974; Kramon and Posner, 2013; Dahlberg et al., 2021; Harjunen et al., 2021). In the following, we argue that the implied relevant geographic area in which favoritism can be seen differs across these manifestations. On the one hand, in order for it to be effective, vote/support-buying favoritism must necessarily affect a relatively large area or a large demographic group, whereas the *pure* favoritism policies will, in most cases, have visible

[2]Correa was also referring to diverse other interest groups from the banking and media sector.

consequences in very sharply defined geographic areas. As such, we specifically ask if the particular institutional division of political power in Latin America implies that parliament leaders can channel resources to client regions in approximately the same dimension as is usually found for heads of government or prime ministers in other parts of the world.[3] We argue that a basic mechanism emerges from the uncertain normative framework underpinning governance in the region, and explore how and to what extent the influence provided by de jure and de facto mechanisms shape the favoritism of parliamentary leaders.

To do so, we collected data on Latin American and Caribbean leaders' birthplaces. Most of these data are from parliament leaders—from Upper and Lower Houses—but we also collected information on executive leaders that are not included in the data directly shared with us by Hodler and Raschky (2014). The panel data consist of 238 different leader birth regions over the 1992–2016 period, which we analyze in relation to 183,082 subnational micro-regions in models that control for ADM1-year and regional fixed effects, and that include relevant covariates such as lagged night lights and executive leader's birth region dummies. To shed light on our main mechanism of interest, we further develop an Index of Parliamentary Powers (IPP), which is then interacted with leaders' birthplaces to control for the quite different degrees of de jure powers allocated to the parliament. In parallel, we test other plausible proxies of informal, institutional resourcefulness. For example, we run a specification where we use the age of the current constitution as a measure of constitutional entrenchment or de facto institutional influence. By exploiting the cross-sectional and time-varying data of our preferred model we distinguish parliament leader's favoritism from a historic association between levels of economic development (night lights) and the birth region of the leader in office. That is, relying on subnational variation over time in tandem with our controls—and later, with the inclusion of pre- and post-trends dummies—we argue for a plausible causal relationship between elected parliament leaders and posterior development of, and aid allocated to, regions close to the leaders places of birth.

Our results show that parliament leaders are able to divert resources to regions in close proximity to their birthplaces (in a radius of 11 km from the leader's birthplace), represented by a 8.3% increase (significant at the 5% level) of the regions' night light emissions just one year after the leaders' taking office. The discretionary influence of parliament leaders is greater than that estimated for executive branch leaders, which is non-significantly different from zero in regions that have not been an executive leader region before, and rather negative (15.4% decrease, significant at the 10% level) for regions that have been near an executive leader birthplace in the past. The effects for parliamentary leaders are larger when comparatively high de jure power is allocated to the parliament as the regions of the countries in the third tercile of IPP (IPP>0.40) experience an increase of 24% (5% level) in its light indicator. Similarly, the effects

[3]We use the terms heads of state, heads of government, prime ministers, executive leaders, and presidents interchangeably to refer to leaders of the executive branch throughout the paper.

are larger in leader regions of countries with more de facto institutional instability or less entrenched constitutions, as measured by the age of the most recently introduced constitution in the country. In leader regions where the constitution has just been introduced, an increase of 12.7% (5% level) in night light emissions is expected. Every extra constitution year generates a 0.2% marginal decrease (10% level) of the leader's regional night lights figure, which implies that only after more than 63 years of a constitution remaining in place do the effects of favoritism completely dissipate.

Favoritism is also apparent in how World Bank (WB) aid is allocated. The effect, however, is mediated by the de jure influence given to the parliament—again, proxied by our measure of IPP. The leader regions in countries with an IPP over its second tercile (IPP>0.27) see an increase of around 23% (at 5% level) in the amount of WB aid they receive. Parallel to the light indicator, for every extra year the constitution is in place, the effect on aid decreases by 0.3%. The results on WB aid also suggest a competition-for-resources dynamic between parliament and executive leaders. When analyzing the effect across different levels of IPP, a significant increase (decrease) of aid is visible in parliament leaders' birth regions located in countries with higher (low) levels of IPP. In turn, the inverse is true for presidential leaders. A significant increase (decrease) of aid is visible in executive leaders' birth regions located in countries with low (higher) levels of IPP. Finally, favoritism from parliament leaders seems not to be present for Chinese aid.

We contribute to the literature that explores the importance of institutions on resource redistribution by documenting how different forms of institutions can strengthen or weaken subnational favoritism (Robinson et al., 2005; Acemoglu and Robinson, 2012; Prebisch, 2016). Furthermore, we add to the literature on channels of favoritism by assessing the effects of leaders' geographic characteristics on foreign aid (Hodler and Raschky, 2014; Dreher et al., 2019a). Whereas some previous studies focused on prime ministers in a smaller sample of the Americas, we exploit changes in night light intensity within subnational regions of almost all parliament leaders of LAC countries. Finally, our paper is related to literature that recognizes the interplay between geography, institutions, and regional development (Banerjee and Iyer, 2005; Henderson et al., 2001; Henderson et al., 2018). We complement these studies, however, by focusing on the phenomenon of favoritism in the LAC region, which has a particularly unstable context and thus is worth separating from other supra-regions.

We therefore conclude that parliament leaders' favoritism in LAC countries is more relevant than that of presidents or prime ministers, emerges already in their first year in office, and is as important as the degree of de jure and de facto influence provided by the institutional frameworks within which such distributional power operates. Note that the magnitude of this favoritism is comparable to that found in the work of Hodler and Raschky (2014) for presidential leaders in other parts of the world. In general, our findings are of political and economic relevance as they are consistent with the existence of *pure* favoritism targeting the politicians' immediate network, i.e., direct transfers to family, friends, or acquaintances, as parliament leaders are only able to divert resources to regions in a radius of 11 km from the leader's birthplace. This *pure* favoritism undermines a nation's distributional efficiency even more than general, vote-buying favoritism, as the benefits are concentrated in even fewer people. Overall, these effects and the key institutional mechanism on de jure and de facto influence that is given to the parliament via the constitution highlight the importance of a clear delimitation of control of the legislative branch and the intertemporal stability that the constitution should have.

As for robustness, we run several tests. For instance, we address the potential endogeneity of the leaders' birth region by running specifications with different proxies of development that might very well correlate with leaders' birth regions. We also test if the homelands of the future parliament leaders exhibit significantly more intense nighttime light in the years prior to or after a parliamentary transition, i.e., prior to or after their parliament's leadership. As a result of these tests, we find no evidence pointing towards post- or pre-trends potentially biasing our estimation of interest: night lights/aid with leaders' birth regions. Note, however, that while our work exploits data associated with economic activity, we leave room open for future research on other equally important proxies of development such as health, education, or security.

The rest of the paper is structured as follows. Section 3.2 outlines our data and the empirical strategy. Section 3.3 describes our findings, while Section 3.4 presents the main robustness checks conducted. Section 3.5 concludes.

3.2 Identification Strategy

3.2.1 Data Structure

We base our analysis on a panel dataset of 183,082 subnational micro-regions corresponding to 45 countries/autonomous territories, 613 states/provinces, and 10,753 cities/towns of the Latin American and Caribbean region between 1992 and 2016. We gathered information about 366 political leaders' 238 distinct birthplaces at either their official second (ADM2) or third administrative border division (ADM3) level, depending on the precision of such information. Depending on the country, these divisions could refer to a province, city, or town. We geocode those distinct birthplaces at their *centroid*, i.e., at their average geo-position, which is computed using all geo-coordinates of the ADM2 or ADM3 region. We use the cutoff date of January 1st to deal with half years or acting parliament leaders. In other words, if a leader was in office on January 1st, the year is "allocated" to them.[4] For countries with a bicameral system, we define the parliament leader as the one exercising the leadership of the Lower House, as they are institutionally—for instance, the Lower House can usually override Upper House's decisions—and historically more influential. Nevertheless, in the robustness tests that we show in the Appendix, we make a distinction between Upper and Lower House leaders.

[4]For countries where a number of individuals alternate the leading position during the same year, we allocated the legislative leadership to the individual who spent the most time as the leader.

To account for regional favoritism, we rely on a common subnational measure of development (Henderson et al., 2012; Hodler and Raschky, 2014; Donaldson and Storeygard, 2016; Weidmann and Schutte, 2017; Bruederle and Hodler, 2018). This literature has validated the use of night light emissions as a proxy for economic or human development, given its need for most forms of production and consumption nowadays. Therefore, our dependent variable $Light_{ict}$ accounts for the intensity of nighttime lights in region i in country c and year t. Produced by the National Oceanic and Atmospheric Administration (NOOA), nighttime light is an indicator that ranges between 0 and 63—with an added standard 0.0001 constant for emission when using logs—that allows us to account for a spatial resolution of 1 by 1 km, and a balanced panel between 1992 and 2013 for all the regions under study. We also replicate our main results using aid as the main dependent variable instead. We run regressions both on World Bank disbursed aid amounts $Aid_{i,c,t}$, and Chinese committed figures $China\ Aid_{i,c,t}$. Committed, as Chinese aid data does not include disbursement details.

Assigning latitude and longitude coordinates to birthplaces of parliament leaders allows us to create a binary variable, $LeaderBR_{i,c,t}$, that takes the value of 1 when region i is close to the leader's birth region of country c in year t, and 0 otherwise.[5] Similarly, we argue that a potential transmission channel is associated with the executive branch leaders' birth regions. We build on the data shared with us by Hodler and Raschky (2014), and code $PresidentialLeaderBR_{i,c,t}$ as a binary variable that is equal to 1 if the executive leader of country c in year t was born near region i, and 0 otherwise. As Hodler and Raschky's data do not cover all the countries that we look into, we collect information on the birthplace of executive leaders by searching official government and personal websites, and geo-code this information ourselves.

Institutions in Latin America and the Caribbean are known for their constant change and overall instability. Thus, changes in the amount of de jure power granted to the different political actors may affect their behavior directly as well as their de facto influence. As such, we expect heterogeneous favoritism effects across LAC countries and therefore include proxies that capture the redistribution of power among different factions of the political composite. While the specific Parliamentary Powers Index, developed by Fish and Kroenig (2009) exists, is available, their index is based on 32 criteria intended to capture different aspects of the power allocated to the legislature relative to the other branches of government. This index, however, is not a practical option for this study as several of its elements are not available for a large sample of countries, and the full index is only available as a cross-section. Given the substantial constitutional instability in most of Latin America, we cannot assume that the power allocation is stable over a 23-year period. We, therefore, develop our own Index of Parliamentary Power (IPP). Inspired by a similar exercise in Bjørnskov and Voigt (2018), we construct an indicator based on the constitutionally defined allocation of powers and separation of competences. We base our index on 15 variables available from the

[5]We exclude two parliament leaders who were born abroad from our sample: Victor Jeame Barrueto (born in Madrid, Spain), who was the leader of the Chilean parliament between 2000 and 2001, and Alfred T. Oughton (born in London, England), leader of the Bermuda Senate in the 1998-2008 period.

Comparative Constitutions Project (Elkins et al., 2009), which we update and expand to cover all sovereign countries in the region, as well as all colonies with effective home rule with available data on light intensity. Table 3.A.1 in the Appendix section details the 15 indicators included in our index. Our IPP measure first captures information on whether the constitution directly appoints a speaker or similar official leader of the legislature, i.e., if there indeed exists a de jure leader of the parliament. The IPP further includes elements that account for the degree of power discretion within which the parliament operates. That is, whether it legislates without the consent of any other political actor or faction, or, if cabinet members have immunity from prosecution. In sum, we use the IPP as a measure of the concentration of discretionary power in the parliament. For each element listed in Table 3.A.1, we code a score of 1 when the legislature has actual power, 0.5 if the provision is uncertain, and 0 if the legislature does not have an actual influence on the topic. The final *IPP* is a simple rate between 0 and 1, describing the average across the 15 components of Table 3.A.1. As illustrated in Figure 3.1, the power index is distributed between a minimum of 0.13 in a number of former British colonies in the Caribbean and a maximum of 0.67 in Nicaragua in recent years. We mainly use this index in interactions with variables at the local level, as they separate the potential effects of having greater parliamentary power allocated by the constitution. To the extent that more formal influence is allocated to the parliament, one should expect a greater room for favoritism by the parliament leaders.

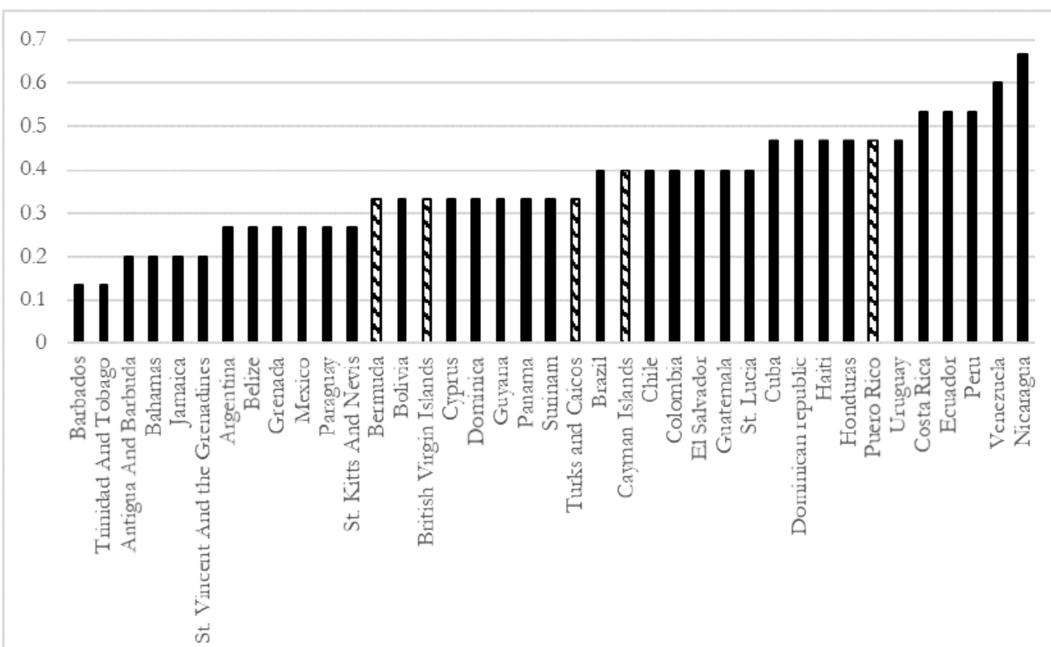


Figure 3.1 Index of Parliamentary Powers, all included countries in 2015

Furthermore, given the unstable jurisdictional framework within which our observation units are likely to operate, we exploit other, perhaps more direct proxies of de jure and de facto originated influence. *AgeConstitution* then refers to the number of years since the adoption of full new constitutions, not only reforms. For the number of years since the last reform or amendment was introduced to the constitution, we create a variable labeled *AgeAmend*. Both are arguably institutional sources of influence, yet politics do not operate in a social vacuum. Therefore, we use data on leaders from other branches or houses to generate interactions that would indicate, a priori, larger room for discretionary action for our leaders of interest. Namely, we use $PresidentialLeaderBR_{i,c,t}$, and a dummy representing the birth regions of leaders of the Upper House $LeaderUpperHouse_{i,c,t}$ to interact them with our main dummy $LeaderBR_{i,c,t}$. In robustness tests, we also construct an index portraying the degree of unclear delimitation of jurisdiction between the executive and the legislative in the constitution, $SharedPower_{c,t}$. We also use elements of our IPP directly and interact it with our Leader dummy. In particular, we use the dummy called LHLEAD in Table 3.A.1 and we rename it $Speaker_{c,t}$. The latter variable captures information on country-year pairs where the constitution defines a formal position of leadership within the parliament. All variables rely on information from the Comparative Constitutions Project (CCP) (Elkins et al., 2009) which we update and expand to cover all the constitutions within our sample. We also use a dummy variable *Independent* representing the independent status of the country under study, considering the colonial past of countries of LAC. Finally, we additionally account for time-in-office-related mechanisms that could inform varying degrees of power redistribution. Using our gathered data on legislative leaders, we build a variable *Experience*, which reports the number of years the parliament leader has been in power until year t, and a variable *Tenure*, which accounts for the total number of years in office between 1992 and 2015. Table 3.A.4 provides the sources and definitions for the variables used throughout this paper, while Table 3.5 provides summary statistics for all of them.

3.2.2 Empirical Strategy

In order to study the extent to which parliament leaders in LAC countries can channel resources to client localities, we employ a model based on the work by Hodler and Raschky (2014) on favoritism. Our preferred units of observation are circular-shaped micro-regions with a radius of 5 km uniformly dispersed throughout all Latin American and Caribbean countries. The regions are clipped to coastal and ADM1 borders. Thus, we compute the average night light emissions per micro-region and year as displayed in Figure 3.2.

To calculate the average impact of parliamentary favoritism then, we estimate:

$$Light_{i,c,t} = \alpha_i + \eta_{j,t} + \beta_1 LeaderBR_{i,c,t-1} + \beta_2 Light_{i,c,t-1} + \beta_3 PresidentialLeaderBR_{i,c,t-1} + \epsilon_{i,c,t} \tag{3.1}$$

where β_1 is our main coefficient of interest and $LeaderBR_{i,c,t}$ indicates whether the region under study is within a certain distance cutoff from the incumbent parliament leader's birthplace. Following Hodler and Raschky (2014), in our model we lag this variable, $LeaderBR_{i,c,t\text{-}1}$. $PresidentialLeaderBR_{i,c,t\text{-}1}$ is a dummy detailing whether the

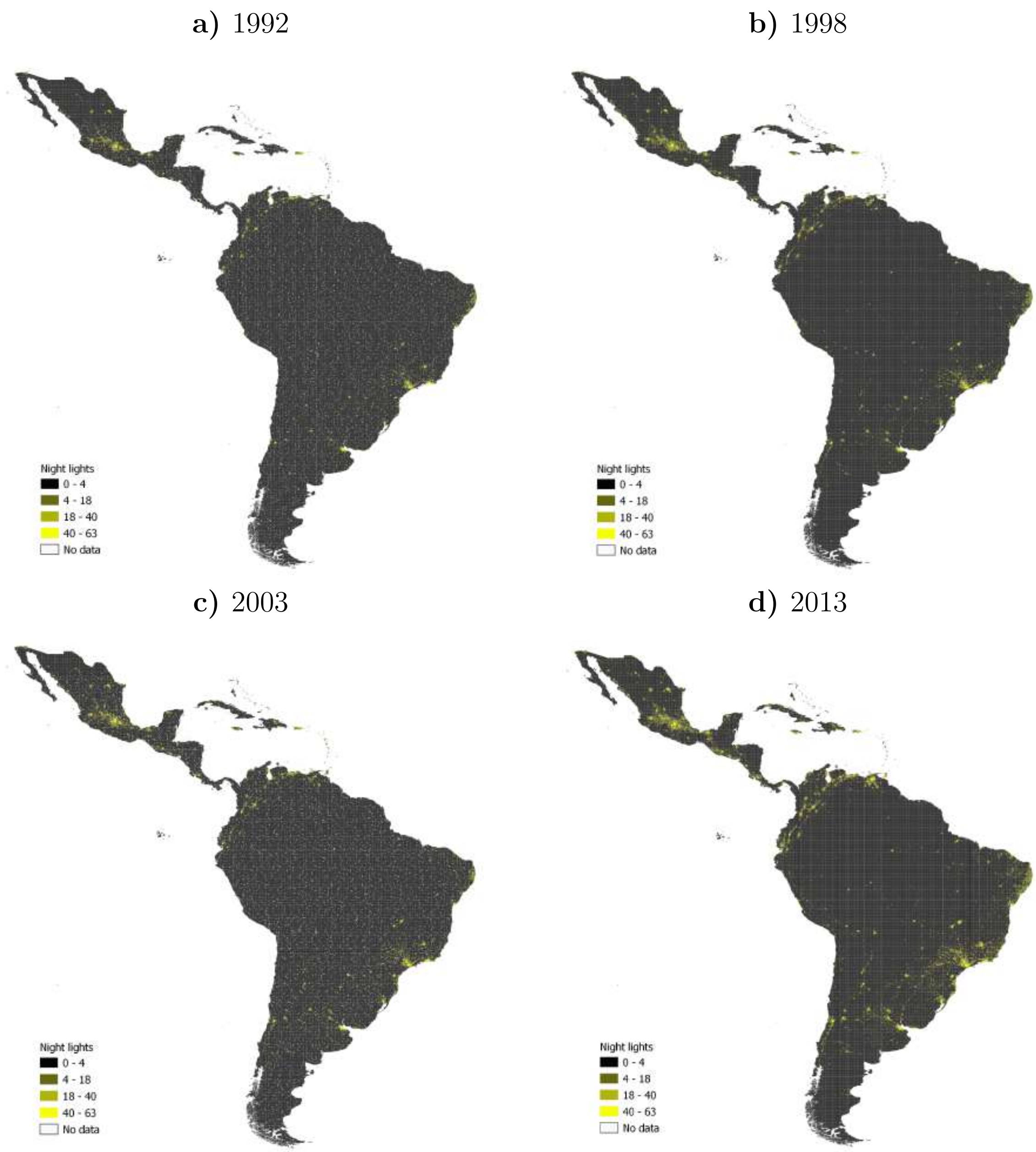


Figure 3.2 Micro-regional night lights over time

Notes: The micro-regions are buffers with a 5 km-radius. The micro-regions are clipped to land, at the ADM1 level.

micro-region is close to the executive branch leader's birthplace as several studies mentioned previously have shown that leaders of the executive can indeed channel resources to their birth regions. We also include $Light_{i,c,t\text{-}1}$ to capture previous levels of development or economic activity in order to address concerns about reverse causality, i.e., leaders being elected as a result of particular socioeconomic conditions (proxied by

$Light_{i,c,t}$) preceding them.[6] In all preferred specifications, to account for general shocks in all regions within a province/state in any given year we control for ADM1-year fixed effects ($\eta_{j,t}$). Similarly, to control for time-invariant traits of the regions under study—such as historical political influence, latitude, size, elevation, etc.—we include regional fixed effects (α_i).[7] Given that micro-regions close to the same parliament leader's birthplace might share relevant characteristics, which would imply a correlation between the error terms, we cluster standard errors at the level of parliament leaders to control for the likely correlation.[8] To account for potential geographically-related spill-overs, in our main Table 3.1 we use different cutoff distances from leaders' birth regions, i.e., 111 km, 55 km, 28 km, and 11 km—such distance cutoff distinction also allows us to understand better the type of favoritism enacted by parliamentary leaders, an aspect explained in detail later in the paper.

[6]In robustness specifications we use other plausible proxies of development that can be seen later in Table 3.5. Results do not vary qualitatively.

[7]ADM1 refers to the first official administrative division of a country. Depending on the country, this could either refer to a state or a province.

[8]For completeness, we lag the clusters by one period, even though results without this lag structure are qualitatively identical and can be requested directly from the authors. In parallel, we run a robustness test in Appendix 3.2 in which, instead of clustering at the leader's level, we use the country level in the fashion of De Luca et al. (2018) or Dreher et al. (2019a).

Figure 3.3 Leaders' Birth Regions

Notes: Gray points refer to the parliament leaders' birthplaces. Black points to prime ministers' (presidential) birth regions.

Figure 3.3 shows a map of the birth regions of political leaders across the LAC region at the ADM2 level. Regional variation between areas where the leaders of the parliament (in black) were born and the birthplaces of executive leaders (in gray) can be observed, particularly for the larger countries. Favoritism is likely to be present in more than one political faction, and more so, as discussed, in regions with volatile institutional incentives for discretionary action, such as in LAC countries. To the extent that leaders of the executive have been consistently shown to favor their birth regions in other continents, and these regions might coincide with the ones where the parliament leaders were born, $LeaderBR_{i,c,t\text{-}1}$ might capture the impact of presidential leaders instead. Thus, the role of the birth region of the leader of the executive branch might very well belong in the model as an independent covariate. For this reason, we include in our main specification a control $PresidentialLeaderBR_{i,c,t}$, which captures information similar to the $LeaderBR_{i,c,t}$ variable but now referring to the leader of the executive branch. We also lag this covariate, $PresidentialLeaderBR_{i,c,t\text{-}1}$.

As noted before, we expect systematically heterogeneous favoritism effects as the degree of power allocated (in-)formally to parliament leaders varies considerably in our sample (as suggested by, for instance, Figure 3.1). The baseline effects of constitutional features are captured by the ADM1-year fixed effects of equation 3.1, as they vary at the country-year level. Or in other words, as the effects of institutional differences on the entire country and ADM1 regions are captured fully by the fixed effects, the interactions capture any differential effects relevant at the local level. Thus, in equation 3.2, we include an interaction between our country-year level variables (e.g., Index of Parliamentary Powers) and our variable of interest $LeaderBR_{i,c,t\text{-}1}$. This interaction is meant to account for the local-level effect of institutionally, (in-)directly-originated, country-level influence given to the parliament. We thus estimate:

$$Light_{i,c,t} = \alpha_i + \eta_{j,t} + \beta_1 LeaderBR_{i,c,t-1} + \beta_2 (LeaderBR \times CYV)_{i,c,t-1} + \beta_3 Z_{i,c,t-1} + \epsilon_{i,c,t} \tag{3.2}$$

where $CYV_{i,c,t-1}$ would represent any country-year level institutional variable (IPP, AgeConstitution, etc.). Adding this interaction term implies—depending on β_1—that the coefficient of $(LeaderBR \times CYV)_{i,c,t-1}$ will now measure the effect of being near a parliament leader's birth region on night light intensity in countries with different degrees of de jure (e.g., IPP) or de facto (e.g., AgeConstitution) influence granted to the legislative branch. $Z_{i,c,t-1}$ is the vector of individual (micro-region) controls ($Light_{ict\text{-}1}$ and $PresidentialLeaderBR_{i,c,t\text{-}1}$) included in equation 3.1.

In the following section, we present baseline results and some variations using different proxies for formal and informal sources of leaders' influence in Latin America and the Caribbean.

3.3 Results

To get a first impression of how nighttime light data may capture changes in economic activity as a result of regional favoritism exercised by parliament leaders, we briefly explore the Dominican Republic as a pertinent case between 1996 and 2005. Figure 3.4 displays the average night light emissions between 1996 and 2005 in a radius of roughly 11 km from the center of the municipality "San José de Los Llanos" of the province "San Pedro de Macorís" in the Dominican Republic, which is the birthplace of the parliament leader Rafaela Alburquerque. Between the presidencies of Leonel Fernández of 1996-2000 and Hipólito Mejía of 2000-2004, Rafaela Alburquerque acted as president of the Lower House of the Dominican parliament between 1999 and 2002. The three individuals belonged to different political parties and did not share their region of birth. This particular dynamic exemplifies the phenomenon that we address in this paper, i.e., we look into a regions' growth over a given period time, for example 1999-2002 in the Dominican Republic, when it is geographically close to the birthplace of the parliament leader in office.

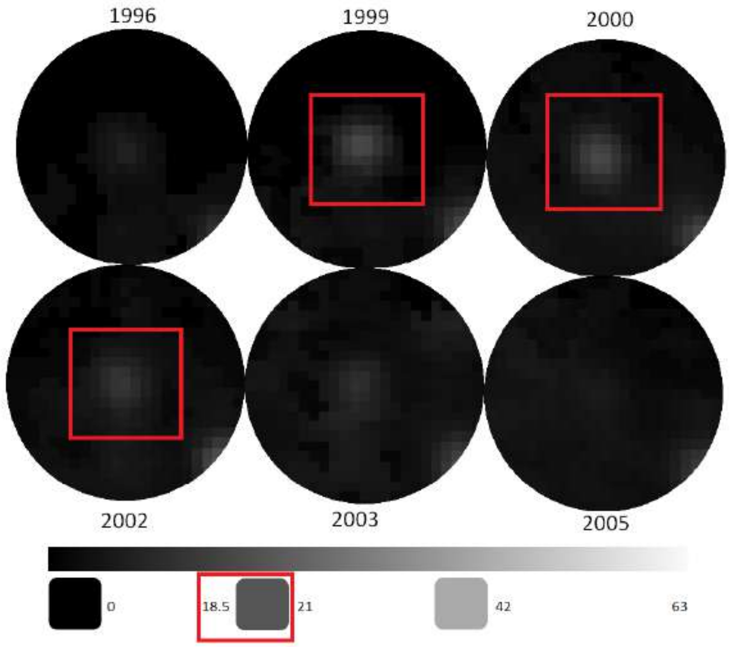


Figure 3.4 Night lights in Alburquerque's birth region

Notes: Images generated by authors that represent the change in night light emissions between 1996 and 2005 in regions within approx. 11 km Rafaela Alburquerque's birthplace. Rafaela Alburquerque acted as president of the Dominican Republic assembly between 1999 and 2002. The red squares are associated with Rafaela Alburquerque's time in office, the 18.5 night lights intensity level, and the regions closest to her birthplace.

Before Rafaela Alburquerque's arrival in office (1996-1998), nighttime light emissions in regions within roughly 11 km of her birthplace had a maximum output of 14. These emissions, however, as can be evidenced in Figure 3.4, increased dramatically upon her arrival in office (1999-2002), climbing up to 18.5—a 32.14% growth. Shortly after she left office these numbers returned to 14, as is also suggested in Figure 3.4 for the years 2003 and 2005. The fact that light intensity significantly grew during her term, and reversed shortly after the end of her leadership (post-2002), suggests that, when in office, Rafaela Albuquerque may have deliberately favored her birth region. While such an example is obviously not evidence of either causality or generality, this first example from our data is similar to the findings by Hodler and Raschky (2014). Although not conclusive for the Americas, they show that the birth regions of executive branch leaders tend to light up soon after the leaders come to power or gain access to additional funds. Furthermore, they show that immediately after leaving office it is common to notice a decrease in the region's light output, in line with our example in Figure 3.4.

3.3.1 Main Results: Parliament's favoritism

Our baseline results for equation 3.1 are reported in Table 3.1. We report three sets of results for each distance cutoff (111 km, 55 km, 28 km, 11 km): 1) results with only $PresidentialLeaderBR_{t\text{-}1}$ and $Light_{t\text{-}1}$ as covariates; 2) results including the just mentioned covariates and ADM1-year fixed effects; and 3) results including the full set of fixed effects: ADM1-year and micro-regional, and the $PresidentialLeaderBR_{t\text{-}1}$ and $Light_{t\text{-}1}$ controls. The latter is our preferred specification, as the estimates of 1) and 2) are likely to capture selection effects if leaders are more likely to be appointed when they are from, for instance, a politically relevant location or well-performing region.[9] Note that we prefer the reading on closer localities (11 km cutoff) to those farther away since defining treated localities as those beyond 11 km would remove treatment variation from a number of small Caribbean countries, and would exclude an actor of interest for us.

[9]We are aware of the potential Nickell (1981) bias produced by the use of a lagged dependent variable ($Light_{t\text{-}1}$) on the right-hand side of the equation. However, following Angrist and Pischke (2009), we run a robustness test without this variable in Table 3.2.1, which is included in Appendix 3.2. As can be seen, its inclusion does not qualitatively change our main results. Additionally, we ran a Fisher-ADF unit root test to rule out a potential unit root issue. All P, Z, L* and Pm tests reported a p-value smaller than 1%, rejecting the hypothesis that all panels contained unit roots and therefore, that at least one panel is stationary.

Table 3.1 Leader effects on Economic Activity

		111 km			*55 km*			*28 km*			*11 km*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Light	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**
$LeaderBR_{t-1}$	0.220***	0.119***	0.004	0.264***	0.146***	-0.029	0.360***	0.239***	-0.005	0.449***	0.330***	0.083**
	(0.042)	(0.029)	(0.025)	(0.065)	(0.045)	(0.036)	(0.070)	(0.058)	(0.045)	(0.055)	(0.054)	(0.042)
Observations	3,654,656	3,653,558	3,653,558	3,654,656	3,653,558	3,653,558	3,654,656	3,653,558	3,653,558	3,654,656	3,653,558	3,653,558
Adjusted R-squared	0.882	0.888	0.920	0.882	0.888	0.920	0.882	0.888	0.920	0.882	0.888	0.920
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
ADM1-Year FE	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES
Micro-Region FE	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES
Countries	45	45	45	45	45	45	45	45	45	45	45	45
Regions	183082	183030	183030	183082	183030	183030	183082	183030	183030	183082	183030	183030

Notes: The values for Light are in log form. All columns control for $Light_{t-1}$ and $PresidentialLeaderBR_{t-1}$. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

The main finding in Table 3.1 is that parliament leaders in LAC countries appear able to redistribute substantial resources to their birth regions, reflected in an average increase of 8.3% of night light emissions in those areas closest to their birth regions—note that the magnitude and direction of this effect is comparable to the one found by the concurrent work of Hodler and Raschky (2014) for presidential leaders. Across Table 3.1, when we do not include fixed effects (columns 1, 4, 7, 10), the estimates for $LeaderBR_{t\text{-}1}$ are always positive and statistically significant at the 1% level, providing evidence of regional favoritism for all distance cutoffs. When regional fixed effects are used, results are significant at the 5% level and only for the regions closest to the leader's birthplace (11 km cutoff, column 12). These results indicate that when one 'zooms in' on sufficiently specific localities, namely within 11 km from the leader's birthplace, favoritism becomes consistently apparent.[10] Despite these results, it is not clear whether administrative boundaries matter. Interestingly, the treated cities in our 11 km specification have a median size of 317 km^2, whereas the non-treated have a 519.5 km^2 median size. In combination with the general results, this difference could suggest that parliamentary favoritism concentrates especially in relatively smaller cities, namely smaller than the median city size in LAC (404 km^2). We test this in Table 3.2 in the Appendix 3.2 by reestimating our main specitications for the 111 km, 55 km, and 28 km cutoffs; there, we interact our main variable of interest with a dummy that distinguishes between micro-regions belonging to cities below the median size of LAC cities from those above. As can be seen, the overall results reflect how the identified favoritism effects are concentrated in parliament leaders' cities with a size smaller than the median of LAC cities. These findings are consistent with our hypothesis on the existence of *pure* favoritism as expressed by the limited geographic extension of the evidenced favoritism. With the aim of buying votes and, therefore, had the type of favoritism been broader, the effect of such favoritism would have been apparent regardless of the city size or beyond the city limits.

As its rather relevance has been shown often (Dreher et al., 2019a; De Luca et al., 2018; Hodler and Raschky, 2014), in Table 3.2 we expand the analysis to account for the effect of executive branch leaders $PresidentialLeaderBR_{t\text{-}1}$. For this, we generate five specifications that should allow us to understand such influence better and make sense of results of previous works. In column 1 we use the referential work of Hodler and Raschky (2014). They find that the favoritism, while generally significant and positive, disappears when isolating North and South America. Their identification model, however, is slightly different from ours, most noticeably because of the use of country-year fixed effects instead of the ADM1-year fixed effects utilized in our model. For this reason, to facilitate comparison column 1 uses the set of country-year fixed effects

[10] In Figure 3.1 in Appendix 3.A, we illustrate this idea. Considering an ADM2 region of median LAC size (404 km^2), an 11 km radius buffer would cover a considerable area of said region. In the case of a square-shaped region of approximately 400 km^2 (20 km $\times$ 20 km, diagonal = 28.28 km), the leader's birth location would be placed in the center (centroid). The 11-km-radius buffer (purple) would be generated from this centroid and, as depicted in the figure, would cover around 80% of the region's surface (11/14.14).

and find the same qualitative results, i.e., a rather statistically insignificant presidential favoritism. In column 2 we use our main model, as represented in equation 3.1. Once the set of ADM1-year fixed effects is employed, presidential favoritism becomes statistically apparent, yet the effect is negative (-10.8%, at the 5% level). This negative result is, a priori, counter-intuitive, given that the works cited at the beginning of the paragraph have only encountered positive effects of being a region near to where the president in office was born. All these works, however, use a less restrictive control for subnational temporal heterogeneities, i.e., country-year fixed effects. Our work then shows that there are still subnational determinants that vary over time and which might be driving the nature of presidential favoritism. For instance, recent studies have hinted that elected politicians might strategically move funds from region to region.[11] The most telling of these works is that by Seim et al. (2020), who show that once elected politicians get both old and new information on the places that have already received funds, they are less likely to channel funds to those places.[12] We thus generate a set of tests to analyze whether such mechanism might be taking place in LAC. Columns 3 to 5 use equation 3.1 as a baseline model, yet add an interaction between $PresidentialLeaderBR_{t\text{-}1}$ and a dummy categorizing regions that have already been birth locations of prior presidents/prime ministers ($PastPresidentBR_{t1}$), a parliament leaders ($PastLeaderBR_{t1}$), or either of the two ($PastAllLeadersBR_{t1}$). Following Seim et. al's rationale, we expect that regions that have already benefited from being near to a leader's birthplace concentrate the decrease seen in column 2 for our president variable. All three tests suggest that the regions that had already benefited from a leader in the past, disregarding if the leader was from the executive or the legislative branch, experience a decrease in their economic activity. These decreases range between -7% (column 3) and -22.4% (column 4) of the output of night light emissions. On the one hand, and in general, the results shed light on the relevance of accounting for subnational and time-sensitive heterogeneities, as their omission might—as seen in column 1—lead to misidentifying the phenomenon under study. On the other hand, and in particular, the results shed light on the relevance of funding signaling/information as it might very well drive the patterns of (funds) redistribution. In tandem, the tests in Table 3.2 suggest that LAC leaders of the executive do not enact patterns of favoritism, and if anything, they strategically allocate resources based on information of whether regions have received resources in the past or not.

[11] See, for example, Seim et al. (2020) and Cruzatti C et al. (2020).

[12] Seim et al. (2020) argue that the motivation behind such strategic redistribution is more associated with equity rather than electoral cycles, yet the scope of this study does not cover the analysis of such underlying mechanisms and therefore can say little to nothing about them.

Table 3.2 Economic Activity: Legislative and Executive leaders

	(1) **H&R's main model**	(2) **Our model**	(3) **Executive and Past Executive**	(4) **Executive and Past Legislative**	(5) **Executive and Past Any**
$LeaderBR_{t-1}$	0.133*	0.083**	0.084*	0.073	0.081*
	(0.068)	(0.042)	(0.050)	(0.045)	(0.048)
$PresidentialLeaderBR_{t-1}$	-0.075	-0.108**	-0.066	-0.062	-0.040
	(0.057)	(0.042)	(0.046)	(0.045)	(0.049)
$PastPresidentBR_{t-1}$			0.036		
			(0.079)		
$PresidentialLeaderBR_{t-1} \times PastPresidentBR_{t-1}$			-0.154*		
			(0.092)		
$PastLeaderBR_{t-1}$				-0.028	
				(0.075)	
$PresidentialLeaderBR_{t-1} \times PastLeaderBR_{t-1}$				-0.224***	
				(0.070)	
$PastAllLeadersBR_{t-1}$					0.015
					(0.066)
$PresidentialLeaderBR_{t-1} \times PastAllLeadersBR_{t-1}$					-0.174**
					(0.081)
Observations	3,742,213	3,653,558	3,653,558	3,653,558	3,653,558
Adjusted R-squared	0.905	0.920	0.920	0.920	0.920
Controls	YES	YES	YES	YES	YES
Country-Year FE	YES	NO	NO	NO	NO
ADM1-Year FE	NO	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES
Countries	45	45	45	45	45
Regions	183082	183030	183030	183030	183030

Notes: The dependent variable is night lights (log). All specifications use the 11 km distance cut-off. When specified, the controls include $Light_{t\text{-}1}$ (log). The p-value for $LeaderBR_{t-1}$ in column (4) is 0.104. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

3.3.2 Mechanism: De jure and de facto influence

We are interested in the sources of de jure and de facto influence for parliament leaders, as that influence may very well inform the patterns of their favoritism. A priori, the more prerogatives parliament leaders enjoy in national economic affairs, the bigger their capacity to redistribute resources would be, on average. Thus, in Table 3.3 we display the results for equation 3.2 using different, potentially relevant de jure and de facto variables as our Country-Year-Variable (CYV) of interest. The table divides results in three categories. First, a basic mechanism of favoritism arises from the characteristically uncertain regulatory framework that influences governance in the LAC region. Therefore, in columns 1 and 2 we proxy this unstable regulatory framework with the use of our Index of Parliamentary Powers and argue that such an index captures to a great degree the level of de jure influence that the parliament would have on national affairs of varied nature. Second, as discussed before, institutional frameworks of LAC not only vary across countries but also over time. For that reason, in columns 3 and 4, we explore proxies of temporal instability and analyze the age of their constitutions, as differing levels of constitutional entrenchment might represent a strong source of de facto influence. Third, in columns 5-7, apart from combining interactions of our strictly de jure and de facto variables used in previous columns, we include tests assessing the role of political networking and, specifically, how the fact that leaders of different instances of government share the same birth region molds the phenomenon of favoritism.

The first set of results detailed in columns 1 and 2—and in Figure 3.5—shows that parliamentary leader's favoritism is only evident in countries where IPP is greater than 0.4. In other words, once the parliament of a country is constitutionally capable of enacting almost half or more of the actions listed in 3.A.1, redistribution to their birthplaces takes place. In column 1 we directly use the IPP, whereas for column 2 we created different categories by dividing observations into balanced terciles.[13] As is visible in the fourth row of column 2, the variable representing the leader regions of the countries in the third tercile of IPP, $LeaderBR_{t-1} \times IPP3T_{t-1}$, is the only one with a positive and significant estimate at the 5% level. Namely, in countries with an IPP greater than 0.40, an average 24% increase of night light emissions is evidenced within one year in regions closest to the parliamentary leader's birthplace. Conversely, countries where relatively less discretionary power is assigned to the parliament, represented by the categories $IPP1T_{t-1}$—which in column 2 of Table 3.3 is represented by the baseline category $LeaderBR_{t-1}$—and $IPP2T_{t-1}$, favoritism does not take place.

[13]Namely IPP1T=0.0-0.27, IPP2T=0.271-0.40, IPP3T=0.401-0.733. The list of countries per category is described in Table 3.A.2 of the Appendix 3.A. In order to test for the non-linearity of IPP levels, we created several groupings for the IPP indicator. We created categories referring to all the IPP values in our sample: 0, 0.067, 0.133, 0.2, 0.267, 0.333, 0.4, 0.467, 0.533, 0.6, 0.667, 0.733. We also regrouped them in more cohesive categories: 0-0.14, 0.14-0.2, 0.2-0.3, 0.3-0.4, 0.4-0.5, 0.5-1. To be sure we were not picking up selection effects, the upper and lower bounds of the IPP categorizations were also randomized in placebo tests, and are available upon request. Overall, the results always pointed towards categories with lower IPP values behaving differently than categories with higher IPP values, as shown by the results of Table 3.3 and Figure 3.5.

Taken together, the results imply that parliament leaders' favoritism is a phenomenon particular to countries that give, by de jure means, comparatively higher influence to the parliament.

Table 3.3 Mechanisms: *De jure* and *de facto* influence

	De jure		*De facto*		*De jure and De facto*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Light	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**
$LeaderBR_{t-1}$	-0.238	-0.110	0.127**	0.170**	-0.180	0.092**	0.083**
	(0.191)	(0.081)	(0.051)	(0.070)	(0.124)	(0.043)	(0.042)
$LeaderBR_{t-1} \times IPP_{t-1}$	0.702						
	(0.444)						
$LeaderBR_{t-1} \times IPP2T_{t-1}$		0.146			0.249		
		(0.103)			(0.185)		
$LeaderBR_{t-1} \times IPP3T_{t-1}$		0.240**			0.473***		
		(0.100)			(0.150)		
$LeaderBR_{t-1} \times AgeConstitution_{t-1}$			-0.002*		0.001		
			(0.001)		(0.001)		
$LeaderBR_{t-1} \times AgeConstitution2Q_{t-1}$				-0.104			
				(0.091)			
$LeaderBR_{t-1} \times AgeConstitution3Q_{t-1}$				-0.231**			
				(0.101)			
$LeaderBR_{t-1} \times AgeConstitution4Q_{t-1}$				-0.221*			
				(0.127)			
$LeaderBR_{t-1} \times IPP2T_{t-1} \times AgeConstitution_{t-1}$					-0.003		
					(0.007)		
$LeaderBR_{t-1} \times IPP3T_{t-1} \times AgeConstitution_{t-1}$					-0.011***		
					(0.003)		
$LeaderBR_{t-1} \times PresidentialLeaderBR_{t-1}$						-0.155	
						(0.106)	
$LeaderBR_{t-1} \times LeaderUpperHouseBR_{t-1}$							-0.077
							(0.057)
Observations	3,637,000	3,637,000	3,637,334	3,637,334	3,637,000	3,653,558	3,653,558
Adjusted R-squared	0.920	0.920	0.920	0.920	0.920	0.920	0.920
Controls	YES	YES	YES	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES	YES	YES
Countries	38	38	39	39	38	45	45
Regions	182205	182205	182221	182221	182205	183030	183030

Notes: All specifications use the 11km distance cut-off. The values for Light are in log form. All columns control for $Light_{t-1}$ and $PresidentialLeaderBR_{t-1}$. Column 8 also includes a dummy for Upper House leadership. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

Constitutions are supposed to be stable and entrenched documents that are operationalized as literally established. As pointed out above, this is not the case for LAC. We thus test the effects of constitutional entrenchment by constructing an age variable for the constitution ($AgeConstitution_{t-1}$), which details the number of years since the most recent constitution has been in place.[14] Again, using equation 3.2 and replacing

[14]We also test the role of the number of years since the last amendment to the constitution ($AgeAmend_{t-1}$) in Table 3.3 in Appendix 3.2. The age of those amendments introduced and the adoption of a new constitution are not relevant to understanding how favoritism is operationalized by parliamentary leaders, as the results with such interaction are not significant.

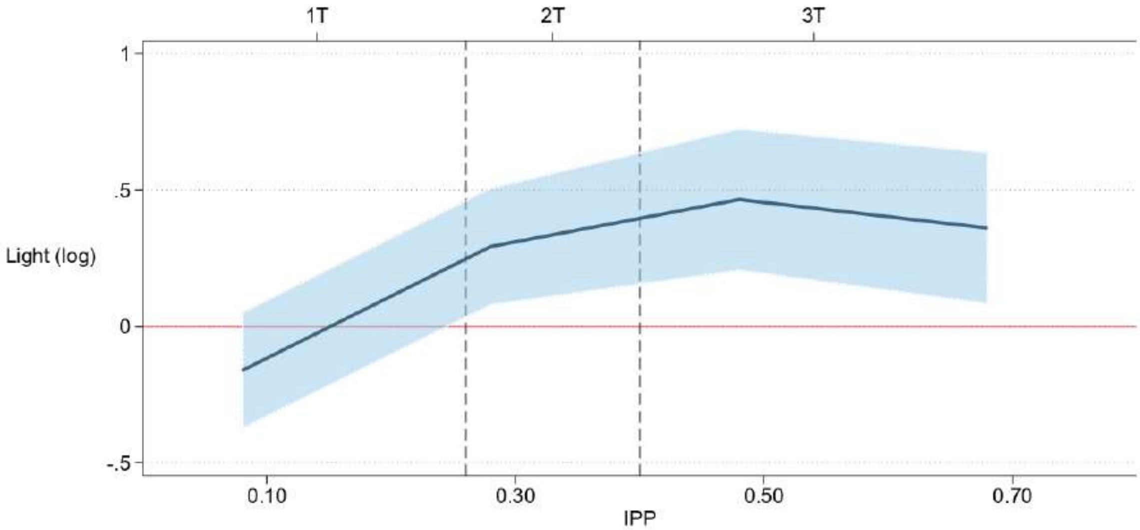


Figure 3.5 Average marginal effects of $LeaderBR_{t-1} \times IPP_{t-1}$

Notes: The figure shows the marginal effects of $LeaderBR_{t-1} \times IPP_{t-1}$ on night lights. 1T, 2T and 3T on the top x axis refer to each IPP tercile, as explained in footnote 13 and computed in Table 3.3, column 2.

CYV_{t-1} with $AgeConstitution_{t-1}$, in column 3 we directly use the age variable, whereas in column 4 we categorize different ages by separating them into balanced quartiles.[15] In tandem, the results of columns 3 and 4 suggest that leaders take advantage of the lack of entrenchment of formal rules, as favoritism only seems apparent when constitutions have just been changed, as one can also see in Figure 3.6. As seen in column 3, when a new constitution is adopted (*AgeConstitution*=0), night lights increase by about 12.7% (at the 5% level) in the regions in the vicinity of the leader's birthplace. With every year that the constitution has been in place (*AgeConstitution*>0), such favoritism decreases by 0.2% (at the 10% level). These estimates imply that the positive effects of a novel constitution are only overcome once the constitution has been in place for at least 63 years (0.127/0.002). Still, the results in column 4 give a clearer picture for the role of time of constitutions. When constitutions are younger than 23 years old, favoritism can be enacted by parliamentary leaders (17% night light increase, at a 5% significance level). Conversely, once constitutions pass this threshold, namely, once they are in the third and fourth quartiles of the age distribution (older than 22 years), the output of night lights decreases at a higher magnitude than the estimate for younger constitutions (around 22% vs. 17%, 23% vs. 17%). Qualitatively, these results underscore the argument that the overall institutional uncertainty in LAC reinforces redistributive

[15] AgeConstitution1Q=0-13 years, AgeConstitution2Q=14-22 years, AgeConstitution3Q=23-33 years, AgeConstitution4Q=34-163 years old. The list of countries per each quartile is shown in Table 3.A.3 of the Appendix 3.A.

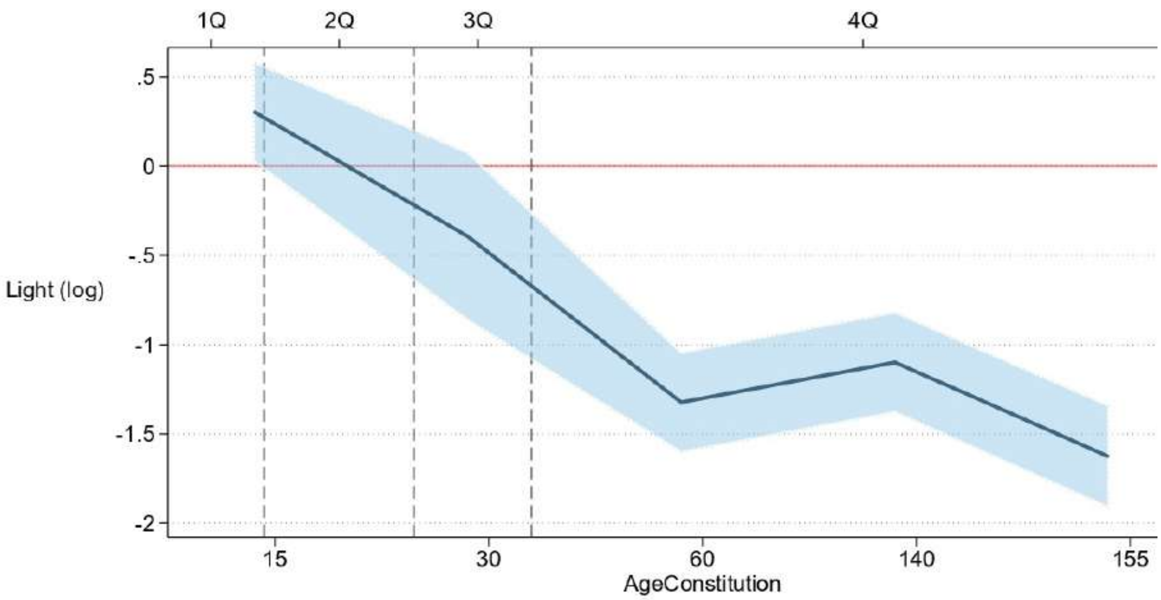


Figure 3.6 Average marginal effects of $LeaderBR_{t-1} \times AgeConstitution_{t-1}$

Notes: The figure shows the marginal effects of $LeaderBR_{t-1} \times AgeConstitution_{t-1}$ on night lights. 1Q, 2Q, 3Q and 4Q on the top x axis refer to each AgeConstitution quartile, as explained in footnote 15 and computed in Table 3.3, column 2.

patterns, expressed in our work in clear trends of parliament leader favoritism.

The results in columns 1 to 4 in Table 3.3 suggest that parliament leaders' home regions benefit when the constitutions give more influence to the parliamentary leader. Note, however, that this constitutionally-originating influence has two dimensions: one formal (de jure), reflected in attributions clearly given to the parliament in the constitution (i.e., IPP), and one informal (de facto), portrayed by the discretionary power allowed by the novelty of the ruling constitution (i.e., *AgeConstitution*). As such, the results might imply that the de jure constraints may only become de facto binding once the constitution is sufficiently entrenched. With this in mind, column 5 includes an interaction term combining these formal and informal roles of the constitution. To the extent that constitutions constrain leaders' favoritism when the constitution is not new, and when it explicitly limits the attributions of the parliament, one would expect that patterns of favoritism become evident only in regions where IPP is high and where the constitution has just been changed. In line with this expectation, column 5 shows that regions in countries with high IPP and new constitutions experience a 47.3% increase of night light emissions—statistically significant at the 1% level. Moreover, in regions with high IPP and established constitutions (i.e., *AgeConstitution*>0) the effects on night lights are reduced as the constitution grows older—1.1% yearly, at the 1% level. For the rest of the regions, i.e., with comparatively low IPP, the effects are not significant at standard levels. Altogether, the results of column 5 suggest that parliament leaders enact favoritism when they are explicitly given higher influence on matters of the state, yet such favoritism is constrained by the novelty (or rather, lack thereof) of the ruling constitution.[16]

Leaders' incentives to take arbitrary action are, nevertheless, not only shaped by formal institutions such as the constitution. Politics do not operate in a social vacuum. One particular strand of research on distributional politics, for instance, highlights the role of informal devices such as partisanship or political networks as the source of redistribution (Arulampalam et al., 2009; Baskaran and Hessami, 2017; Brollo and Nannicini, 2012; Curto-Grau et al., 2018). In a nutshell, these authors show how more social interaction (institutionalized or not) between political figures at different levels of government can render benefits for both in the form of greater allocation of votes, government funds, infrastructural projects, or privileged information. Column 6 in Table 3.3 shows the results of interacting the executive leader's region of birth $PresidentialLeaderBR_{t\text{-}1}$ with our main variable of interest $LeaderBR_{t\text{-}1}$ as in equation 3.2, with $PresidentialLeaderBR_{t\text{-}1}$ being featured as the relevant CYV. If systematic cooperation between the executive and legislative leaders existed, we would expect to see larger and significant effects of such an interaction $LeaderBR_{t-1} \times PresidentialLeaderBR_{t-1}$. As they stand, however, the results do not indicate that parliamentary leaders' favoritism is affected by sharing birth locations with presidential leaders. Similarly, column 7 in Table 3.3 reports the estimates after interacting an Upper House leader's dummy $LeaderUpperHouse_{t\text{-}1}$ with our main variable of interest $LeaderBR_{t\text{-}1}$—as mentioned in the main analysis, $LeaderBR_{t\text{-}1}$ refer to the leaders of the Lower House only. Again, if significant cooperation between the legislative leaders of the Upper and Lower Houses existed, we would expect to see a larger point estimate as a result of the interaction $LeaderBR_{t-1} \times LeaderUpperHouse_{t-1}$. Nevertheless, as with the executive leaders, we do not find evidence pointing in this direction.

Overall, the evidence presented in Table 3.3 suggests at least three things. First, institutionalized sources of discretionary power, i.e., de jure influence, are relevant mediators of parliament leaders' favoritism. Second, we argue that abrupt institutional changes can also inform patterns of favoritism by constituting a source of de facto influence. Third, mixed sources of power related to formal and informal political networks do not seem to be relevant for redistributive practices of parliament leaders in LAC.

[16]We test the role of several other proxies of institutional influence in Table 3.4 of Appendix 3.2. The table shows the country variables of Table 3.A.1—with enough variation—that may also proxy *de jure* influence for parliament leaders. Most of these variables do not play a role. There are however, two exceptions: when the constitution allows the parliament to approve changes to the same constitution, and when the constitution gives the parliament the power to dismiss the cabinet. If these two attributions are granted, parliament leaders favor their home regions, strengthening further our main argument. As more constitutional attributions are assigned to the parliament, parliament leaders' discretionary redistributive power increases. Similarly, in the same section check more traditional sources of potential heterogeneity in Table 3.6. The table shows interactions with variables on the quality of budget management, quality of public sector management, corruption, the share of women in parliament, and GDPpc (World Bank, 2020). All variables have variation at the country-year level. However, none of these variables seem to explain heterogeneous effects.

3.3.3 Channel: Aid

When analyzing African countries, Dreher et al. (2019a) find that Chinese aid is one of the transmission channels of executive leader's favoritism. As very precise georeferenced data are available from 1995 for the World Bank (WB) (AidData, 2017) and from 2000 for Chinese projects (Bluhm et al., 2020), we test the relevance of this channel for parliament leaders in Table 3.4. We use similar setups to those of equations 3.1 and 3.2; however, while the right-hand side of the equation remains the same, we now use the logarithm of World Bank disbursed and committed Chinese aid as outcome variables—instead of (log) night lights.[17] We only include aid projects where geo-coordinates (i) correspond to the exact location, or (ii) are within 25 km of the exact location—AidData precision codes 1-2. Namely, we rely on data for 3,245 World Bank aid project locations between the years 1995-2014, and 137 China aid project locations between 2000-2014.[18]

On the one hand, as can be seen in column 1 of Table 3.4, our coefficient of interest for WB aid is non-significant, suggesting that regions do not receive more WB aid when located near the current parliament leader's birthplace. On the other hand, these results become significant when particular de jure traits are taken into account. Column 2 details the results for the interaction of different levels of IPP—the same three IPP terciles used for column 2 in Table 3.3—with our usual dummy on leader regions. As evidenced for the interactions $LeaderBR_{t-1} \times IPP2T_{t-1}$ and $LeaderBR_{t-1} \times IPP3T_{t-1}$, only when IPP is relatively high (IPP>0.27) do leader regions experience a statistically significant increase of aid (between a 4% and 4.4%). These findings suggest that parliamentary leaders can indeed channel aid to their birth regions under particular institutional circumstances. However, a priori, results of column 2 also pose a puzzle. Namely, when IPP is lowest (i.e., IPP<0.27 or IPP1T=1), why do leader regions receive less aid than regions that are not in the vicinity of leader birthplaces? In principle, given the results of our comparable tests in column 2 of Table 3.3 for night lights, one would expect no significant impact for leader regions with low IPP. We, however, following Seim et al. (2020), have already shown that political leaders make strategic choices when directing resources within their countries (Table 3.2). One mechanism behind these decisions can relate to information on resources given to particular regions in the past (Table 3.2, columns 3-5); however, in column 3 we open up the discussion to another form of the information mechanism underlying political leaders' calculated choices. We argue that political leaders not only react to information on previous funding, but also to information on the degree of power that other instances of government have. Thus, we do not only assess the role of IPP for parliament leaders' favoritism, but, also for executive leaders' favoritism. In column 3, apart from the interactions of column 2, we also include the interactions $PresidentialLeaderBR_{t-1} \times IPP2T_{t-1}$ and $PresidentialLeaderBR_{t-1} \times IPP3T_{t-1}$. With such inclusions we expect to find

[17] Similar to the night lights variable, we added a constant value of 0.0001 on both log aid variables.

[18] We prefer the reading on WB aid as the Chinese aid data for precise projects for LAC have much less variation.

contrasting dynamics between parliament and presidential leaders' favoritism. Given a parliament leader with relatively few formal attributions, presidential leaders can enact more discretionary power, as the system of check and balances would be biased in their favor, leaving regions overall more vulnerable or, more specifically, prone to favoritism. In line with our expectations, when IPP is low (IPP1T) presidential regions receive more aid (19.3%) and—as already hinted by Table 3.3—parliament regions receive less. Similarly, when IPP is higher (IPP2T and IPP3T) president regions receive less aid (-27.6% and -26.3%, respectively), whereas parliament leader regions receive more (4% and 3.9%, respectively).[19] All results are, at a minimum, statistically significant at the 5% level. The results on Chinese aid, detailed in columns 4 to 6, suggest that parliament leaders cannot direct Chinese aid projects to their home regions when they are in power. Such stark differences between WB and Chinese aid, however, are in line with the main arguments on recipient's accountability and donor's conditionality of the aid literature. To the extent that China's aid policy involves fewer controls—than World Bank policy—on the use of such largesse, one would expect questionable practices (such as favoritism) to be present in a larger number of individuals (Isaksson and Kotsadam, 2018) and thus less apparent in the elites we analyse in this study.

The results of Table 3.4, similar to our main results on night lights, suggest that de jure and de facto sources of influence are important for parliamentary leaders to channel resources to their home regions. However, those institutional sources of influence are also mediated by the actions of other type of leaders, suggesting that political leaders not only react to information on previous and current funding to assign resources to specific regions, but also depend on the degree of power to channel resources from other political leaders. In sum, all evidence points towards World Bank aid as, indeed, a channel through which leaders can improve economic performance of their birth regions.

[19]We also ran similar tests with night lights as the dependent variable. The results in such tests are in line with the results on aid. That is, when IPP is highest (IPP3T), parliament leaders favor their regions (13%) while in parallel, regions near presidential leaders' birthplace experience a decline of their night lights (-13.2%). More detailed results can be requested from the authors.

Table 3.4 Favoritism and Aid: World Bank vs. China

	(1)	(2)	(3)	(4)	(5)	(6)
	WB Aid	**WB Aid**	**WB Aid: Executive vs. Legislative**	**China Aid**	**China Aid**	**China Aid: Executive vs. Legislative**
$LeaderBR_{t-1}$	0.027	-0.189***	-0.210***	0.015	-0.116	-0.111
	(0.068)	(0.024)	(0.032)	(0.032)	(0.090)	(0.089)
$PresidentialLeaderBR_{t-1}$	-0.129	-0.129	0.193**	-0.047	-0.047	-0.130
	(0.091)	(0.091)	(0.086)	(0.046)	(0.046)	(0.109)
$LeaderBR_{t-1} \times IPP2T_{t-1}$		0.233**	0.250**		0.107	0.104
		(0.108)	(0.110)		(0.090)	(0.090)
$LeaderBR_{t-1} \times IPP3T_{t-1}$		0.229**	0.249**		0.177	0.173
		(0.102)	(0.103)		(0.114)	(0.114)
$PresidentialLeaderBR_{t-1} \times IPP2T_{t-1}$			-0.469***			0.160
			(0.161)			(0.124)
$PresidentialLeaderBR_{t-1} \times IPP3T_{t-1}$			-0.456**			0.067
			(0.196)			(0.129)
Observations	3,293,595	3,293,301	3,293,301	2,429,569	2,429,349	2,429,349
Adjusted R-squared	0.125	0.125	0.125	0.199	0.199	0.199
Controls	YES	YES	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES	YES
Regions	182221	182205	182205	182221	182205	182205
Countries	39	38	38	39	38	38

Notes: All specifications use the 11km distance cut-off. The values for Light are in log form. All columns control for $Light_{t-1}$ and $PresidentialLeaderBR_{t-1}$. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

3.4 Robustness Tests and Time Mechanics of Parliamentary Favoritism

One could argue that, even being conditional on ADM1-year and regional fixed effects, the identification of favoritism could be threatened by omitted variable bias. That is indeed is a valid concern, especially when considering that our lagged light variable might be capturing something different than what is intended—previous economic development. With this in mind, in Table 3.5 we run several tests for a handful of potentially relevant controls. Bluhm et al. (2021) show, for instance, that night lights are a valid proxy for agglomeration, yet whether they are equivalent to economic development is put into question when referring to units with high spatial resolution such as ours. For this reason, in column 2 of Table 3.5 we test whether our main control of previous development ($Light_{i,c,t\text{-}1}$) does capture previous development—and not just agglomeration—and add a variable of population ($Population(log)_{t-1}$) to equation 3.1. In column 3, we also included a variable for GDP per capita (Kummu et al., 2018) to separate development, as a more holistic indicator of welfare, from just economic output. In different words, GDP per capita would then control for relative levels of production/output, whereas $Light_{i,c,t\text{-}1}$ would uniquely control for other forms of human development, e.g., degree of development of public services, local wealth measured in infrastructure, etc. As can be seen in columns 1-3, the (non-)inclusion of other plausible controls does not qualitatively modify our main results. The estimates and statistical significance are almost identical to those of the main model (column 1).[20] Finally, one might also worry about the potential confounder effect of other types of leadership on regions' economic development. While this concern is mostly proxied by the use of a dummy on executive branch leaders' birth regions, we also wanted to test the influence of other leaders of the legislative branch. Thus, in column 4 we ran the same specification as in equation 3.1, including a dummy for Upper House leader birth regions. Its inclusion does not modify our main results; the point estimates remain unchanged. Column 5 reports the estimates considering all three additional controls. Importantly, parliamentary leaders' favoritism is still evidenced.

Conditional on the use of our controls (lagged night lights, presidential dummy, and ADM1-year and regional fixed effects), time trends affecting the association between our main output of interest (night light) and the parliament leaders' birth regions could still remain unobservable. In Table 3.6 we test the robustness of our main results to timing. Following Hodler and Raschky (2014), we construct a series of dummy variables (*Past1*, *Past3*, *Future1*, and *Future3*) detailing whether a certain location is soon to become a leader region, i.e., in one year (*Future1*), or ceased to be one in the previous year (*Past1*). Similarly, to further strengthen identification, we control for pre-trends (*Pretrend*) and post-trends (*Posttrend*). *Pretrend* is a time trend included in all regions that will become a leader region three years in the future, whereas *Posttrend* is a time

[20] As it stands, the control on GDP per capita seems to be already captured by the lagged variable on light, as its point estimate is not statistically significant.

Table 3.5 Robustness: Other plausible controls

	(1) Light	(2) Light	(3) Light	(4) Light	(5) Light
$LeaderBR_{t-1}$	0.083**	0.078*	0.085*	0.083**	0.076*
	(0.042)	(0.042)	(0.045)	(0.042)	(0.044)
$PresidentialLeaderBR_{t-1}$	-0.108**	-0.107**	-0.112***	-0.108**	-0.108**
	(0.042)	(0.042)	(0.043)	(0.042)	(0.043)
$Light_{t-1}$	0.346***	0.346***	0.346***	0.346***	0.345***
	(0.013)	(0.014)	(0.014)	(0.013)	(0.014)
$Population(log)_{t-1}$		0.031**			0.031**
		(0.013)			(0.014)
$GDPpc(log)_{t-1}$			-0.028		-0.029
			(0.030)		(0.029)
$LeaderUpperHouseBR_{t-1}$				-0.055	-0.064
				(0.057)	(0.062)
Observations	3,653,558	3,622,813	3,524,325	3,653,558	3,507,866
Adjusted R-squared	0.920	0.920	0.920	0.920	0.920
Controls	YES	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES
Countries	45	42	43	45	41
Regions	183030	181535	182211	183030	181396

Notes: All specifications use the 11km distance cut-off. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1

trend included in regions that stopped being a leader region three years ago.

The key finding of Table 3.6 is that our main results do not qualitatively change, even after accounting for prior and posterior trends. More specifically, regional increases in economic activity due to favoritism are not mediated by past ($Past1_{t-1}$, $Pretrend_{t-1}$) or future trends ($Future1_{t-1}$, $Posttrend_{t-1}$). Thus, the favoritism effects that we identify coincide quite precisely with the incumbency of parliament leaders from specific regions. Moreover, based on the non-statistical significance of the trends' coefficients, a potential trend bias does not seem to be present, strengthening the claim of exogenous variation in $LeaderBR_{t-1}$. In other words, changes in the intensity of night light emissions in a leader region are unlikely to be explained by the presence of unobservable time trends. To further capture the role of time in these redistributive dynamics separately from the inclusion of the trends, in columns 4 and 5 we account for effects of the leader's experience ($Experience_{t-1}$), as captured by the number of years the leader has been in

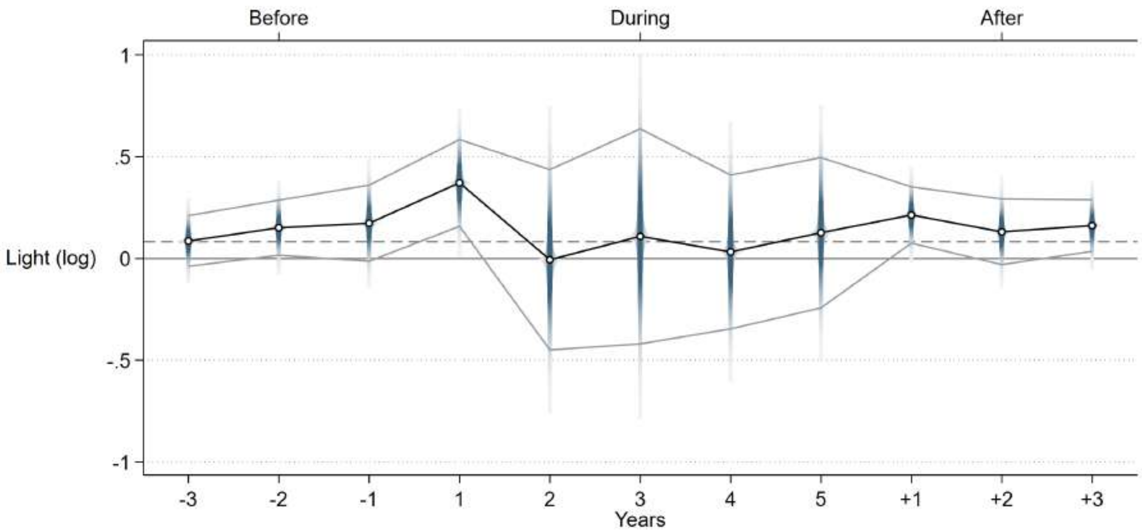
Table 3.6 Time robustness tests

	(1)	(2)	(3)	(4)	(5)
	Light	**Light**	**Light**	**Light**	**Light**
$LeaderBR_{t-1}$	0.080*	0.069	0.070	0.107**	0.096*
	(0.044)	(0.049)	(0.050)	(0.051)	(0.057)
	[0.069]	[0.165]	[0.163]	[0.038]	[0.090]
$Future1_{t-1}$	-0.087		0.008		0.011
	(0.081)		(0.062)		(0.063)
	[0.281]		[0.901]		[0.859]
$Past1_{t-1}$	0.038		0.038		0.037
	(0.087)		(0.079)		(0.080)
	[0.661]		[0.633]		[0.643]
$Pretrend_{t-1}$		-0.033	-0.034		-0.036
		(0.028)	(0.031)		(0.032)
		[0.249]	[0.279]		[0.260]
$Posttrend_{t-1}$		-0.001	-0.003		-0.004
		(0.027)	(0.025)		(0.025)
		[0.969]	[0.894]		[0.867]
$LeaderBR_{t-1} \times Experience_{t-1}$				-0.029	-0.032
				(0.022)	(0.022)
				[0.196]	[0.146]
Observations	3,653,558	3,653,558	3,653,558	3,653,558	3,653,558
Adjusted R-squared	0.920	0.920	0.920	0.920	0.920
Controls	YES	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES
Countries	45	45	45	45	45
Regions	183030	183030	183030	183030	183030

Notes: All specifications use the 11km distance cut-off. The values for Light are in log form. All columns control for $Light_{t-1}$ and $PresidentialLeaderBR_{t-1}$. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

power until t.[21] The inclusion of this leader time-related trait does not affect our results, suggesting that characteristics such as experience are not relevant for parliament leaders to favor their home regions.

[21] We also run tests with trends covering larger periods of time and other potentially relevant covariates—e.g., leader's tenure, the total number of years that the leader has been in power—yet those results do not qualitatively change from those presented in Table 3.6. The tests are available upon request.



Notes: The connected dots plot the coefficient estimates for each time variable, the dyed blue bars above and below the dots represent the confidence intervals, and the light gray lines indicate the upper and lower limits of the 90% confidence interval. We label the x axis as -τ if the the leader will come to office in τ years. Similarly, we code as as +τ if the leader's term ended τ years ago. We represent the number of τ years of leader's current incumbency by labeling the axis as τ (without signs). Finally, we code the axis as 5 if the leaders has been in office for 5 years or more. The horizontal dashed line indicates the coefficient estimate in our main specification (Table 3.1, column 12).

Figure 3.7 Time dynamics of Parliament leaders' favoritism

To illustrate leaders' redistributive choices in LAC countries, we plot their redistributive impacts over time. Figure 3.7 displays the effects on night light emissions over time of parliament leaders' births regions. The computations are comparable to our estimates in Table 3.1. We construct dummies representing 3 years before (-3,-2,-1 in the x axis) and 3 years after (+1, +2, +3) the parliament leader enters/leaves office, their 4 first years in power (1, 2, 3, 4), and 5 or more years (5).[22] As depicted in the figure, there is no clear effect in the three-year periods before and after the region starts and ends being a leader region, strengthening the results portrayed in Table 3.6. Interestingly, night light emissions seem to experience a significant increase in the first year (t=1) of the leader in office. Similar to our first look at the data of the Dominican Republic in Figure 3.4, we can notice a more intense effect on the region's night light once the leader has been in office for one year. Furthermore, as soon as the leader leaves office, emissions start going back to pre-leadership levels. Considering that in LAC countries most parliamentary leaders' stay in power for two years or less, and as regional favoritism abruptly stops after the first year, one can argue these dynamics follow political cycles. Thus, parliament leaders of LAC take advantage of their limited

[22] 99% of the leaders in our sample have a tenure lasting between one and five years, with only a few observations having a maximum of 7 years.

time in power to benefit their immediate family, friends, and networks, which moreover is only consistent with a short-term activity impact and not longer-lasting growth effects.

Finally, we show an overview of the multiple robustness tests (with/without controls, fixed effects, heterogeneity tests, trends tests, etc.) conducted in this study, and which is depicted in Figure 3.8. The estimate marked with red corresponds to the main result from column 12, Table 3.1. As can be seen, for the majority of specifications, the effect of interest (parliament leaders' favoritism) is positive and moderately significant.

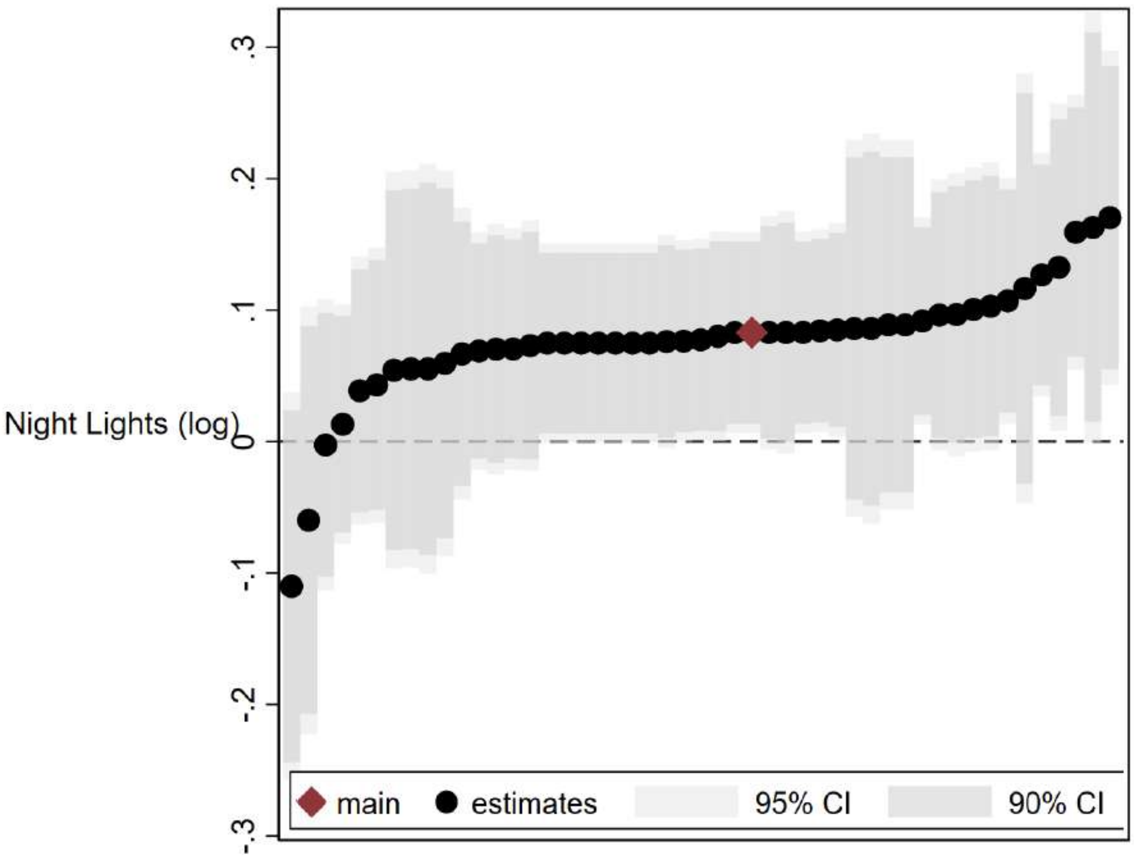


Figure 3.8 Summary of coefficients

Notes: The figure shows the point estimates and their 90 and 95% confidence intervals, for the different specifications explored in this study.

3.5 Conclusions

Recent studies have documented the phenomenon of presidents and prime ministers favoring their home regions by channeling resources to them. This phenomenon, which is known in developed democracies as a specific type of favoritist pork-barrel politics, is likely to cause overall economic losses due to their politically determined reallocation of resources. However, while this literature has found conclusive evidence portraying this type of favoritism elsewhere, for the case of the Americas it has not.

Constitutions and basic institutions delimiting governance are very stable in Western countries, yet those in LAC countries change substantially over time. One of the consequences of this institutional instability comes in the form of ephemeral de jure power residing in various political actors, which in principle makes de facto power rather volatile. This institutional instability of the region has created particular consequences. One of the most important is the constant tension between the executive and the legislative. Other than heads of state and government, parliament leaders in Latin America and the Caribbean also hold significant redistributive power. In this piece, we have therefore explored whether parliament leaders in the region are able to exert similar kinds of favoritism as previous studies documented for presidents and prime ministers. We have done so by exploring levels of light intensity at night, as our measure of economic activity, and aid, as a specific channel of such favoritism. As both indicators share a high spatial resolution, we thus sidestep the problem of either missing or misleading regional and local economic data common in our sample countries.

Overall, we report evidence of favoritism by parliamentary leaders, which mainly occurs when *de jure* and *de facto* frameworks related to the country's constitution give them more influence over their nation's matters. Namely, via more formal attributions and the adoption of new constitutions. Moreover, when regions are close enough to the birthplaces of parliament leaders, favoritism exists in the first year of their time in office, especially in cities that better match the median size of LAC cities. This influence can also be seen in terms of World Bank aid, again, when explicit formal influence is given to the legislative. Together, the results are consistent with the existence of *pure* favoritism targeted at politicians' immediate networks, i.e., direct transfers to family, friends, or acquaintances, given the geographic extent of the effect and the short-term impact of such favoritism. Thus, political favoritism in Latin America and the Caribbean is a real phenomenon that arises from political opportunities seized by parliamentary leaders, especially when new constitutions explicitly grant them high discretionary power.

Appendix

3.A Descriptives

Table 3.A.1 Elements in the Index of Parliamentary Powers

Variable	Variable in CCP
Who presides over the legislature? Coded as 1 if the constitution defines a 'Speaker' or similar official leader of the legislature	LHLEAD
Is the first (or only) chamber of the legislature given the power to legislate?	LHLEGIS
Do members of the legislature have immunity?	IMMUNITY
Does the legislature have the power to interpellate members of the executive branch	INTEXEC
Does the legislature have the power to investigate the activities of the executive branch?	INVEXE
Can members of the legislature initiate legislation?	LEG_IN_5
Can the legislature approve / reject legislation?	LEGAPP
Can the legislature override executive vetos?	OVERWHO
Can the legislature propose amendments to the constitution?	AMNDPROP_4
Can the legislature approve amendments to the constitution?	AMNDAPPR_4
Can the legislature dismiss the head of state?	HOSPDISS_2
Can the legislature approve a dismissal of the head of state?	HOSADISS_2
Does the legislature appoint the cabinet?	CABAPPT_3
Does the legislature need to approve the cabinet?	CABAPPR_3
Can the legislature dismiss the cabinet?	CABDISS_3

Table 3.A.2 Countries per IPP tercile

IPP1T (0-0.27)	*IPP2T (0.271-0.40)*		*IPP3T (0.401-0.733)*
Antigua and Barbuda	Bermuda	Suriname	Costa Rica
Argentina	Bolivia	Turks and Caicos Islands	Cuba
Bahamas	Brazil		Dominican Republic
Barbados	British Virgin Islands		Ecuador
Belize	Cayman Islands		Haiti
Cayman Islands	Chile		Honduras
Dominican Republic	Colombia		Nicaragua
Grenada	Dominica		Peru
Guyana	Dominican Republic		Puerto Rico
Jamaica	Ecuador		Uruguay
Mexico	El Salvador		Venezuela
Paraguay	Guatemala		
Saint Kitts and Nevis	Guyana		
Saint Vincent and the Grenadines	Nicaragua		
Trinidad and Tobago	Panama		
Turks and Caicos Islands	Saint Lucia		

Table 3.A.3 Countries per *AgeConstitution* quartile

AgeConstitution1Q	*AgeConstitution2Q*		*AgeConstitution3Q*		*AgeConstitution4Q*
Antigua and Barbuda	Antigua and Barbuda	Trinidad and Tobago	Antigua and Barbuda	Suriname	Argentina
Belize	Bahamas	Turks and Caicos Islands	Bahamas	Trinidad and Tobago	Bahamas
Bolivia	Belize	Uruguay	Barbados	Uruguay	Barbados
Brazil	Brazil	Venezuela	Belize	Venezuela	Bermuda
British Virgin Islands	British Virgin Islands		Bermuda		Bolivia
Cayman Islands	Cayman Islands		Bolivia		Cayman Islands
Chile	Chile		Brazil		Costa Rica
Colombia	Colombia		British Virgin Islands		Cuba
Dominican Republic	Cuba		Cayman Islands		Dominica
Ecuador	Dominica		Chile		Jamaica
El Salvador	El Salvador		Cuba		Mexico
Grenada	Grenada		Dominica		Panama
Guatemala	Guatemala		Dominican Republic		Puerto Rico
Guyana	Guyana		El Salvador		Saint Lucia
Haiti	Haiti		Guatemala		Saint Vincent and the Grenadines
Honduras	Honduras		Guyana		Trinidad and Tobago
Nicaragua	Nicaragua		Haiti		Venezuela
Paraguay	Panama		Honduras		Virgin Islands, U.S.
Peru	Paraguay		Jamaica		
Saint Kitts and Nevis	Peru		Nicaragua		
Suriname	Saint Kitts and Nevis		Panama		
Turks and Caicos Islands	Saint Lucia		Saint Kitts and Nevis		
Uruguay	Saint Vincent and the Grenadines		Saint Lucia		
Venezuela	Suriname		Saint Vincent and the Grenadines		

Table 3.A.4 Sources and Definitions

Variable	Definition	Source
Light	The logarithm yearly average of nighttime luminosity within micro-region i.	NOAA (2015)
Light (IHS)	The inverse hyperbolic sine of the yearly average of nighttime luminosity within micro-region i.	NOAA (2015)
LeaderBR	Dummy=1 if micro-region i is within 11, 28, 55 or 111 km from the parliament leader's birthplace.	Own construction
PresidentialLeaderBR	Dummy=1 if micro-region i is within 11, 28, 55 or 111 km from the presidential leader's birthplace.	Own construction based on Hodler and Raschky (2014)
PastLeaderBR	Dummy=1 if micro-region i has been a LeaderBR before t.	Own construction based on Hodler and Raschky (2014)
PastPresidentBR	Dummy=1 if micro-region i has been a PresidentialLeaderBR before t.	Own construction based on Hodler and Raschky (2014)
PastAllLeadersBR	Dummy=1 if micro-region i has been a LeaderBR or PresidentialLeaderBR before t.	Own construction based on Hodler and Raschky (2014)
LeaderUpperHouseBR	Dummy=1 if micro-region i is within 11, 28, 55 or 111 km from the parliament, upper house leader's birthplace.	own construction
Experience	Number of years the incumbent Parliament leader near micro-region i has been in power until year t.	Own construction based on Hodler and Raschky (2014)
Tenure	Total number of years the incumbent Parliament leader near micro-region i has been in power between 1992 and 2015.	Own construction based on Hodler and Raschky (2014)
Future1	Dummy=1 if micro-region i becomes a parliament-leader region in $t+1$	Own construction based on Hodler and Raschky (2014)
Past1	Dummy=1 if micro-region i became a parliament-leader region in $t-1$	Own construction based on Hodler and Raschky (2014)
Posttrend	Time trend between t and $t+3$ in micro-regions that stopped being a leader region in t.	Own construction based on Hodler and Raschky (2014)
Pretrend	Time trend between $t-3$ and t in micro-regions that will become a leader region in t.	Own construction based on Hodler and Raschky (2014)
WB Aid	The logarithm of the total, yearly amount of World Bank aid disbursed within micro-region i.	AidData (2017)
China Aid	The logarithm of the total, yearly amount of Chinese aid committed within micro-region i.	Bluhm et al. (2020)
IPP	Yearly average across the 15 components of Table 3.A.1 in country c.	Own construction based on Bjørnskov and Voigt (2018)
IPP1T	IPP between 0 and 0.27.	Own construction based on Bjørnskov and Voigt (2018)
IPP2T	IPP between 0.271 and 0.40.	Own construction based on Bjørnskov and Voigt (2018)
IPP3T	IPP between 0.401 and 0.733.	Own construction based on Bjørnskov and Voigt (2018)
AgeConstitution	Number of years since the adoption of a new Constitution in country c.	Own construction based on Elkins et al. (2009)
AgeConstitution1Q	AgeConstitution between 0 and 13 years.	Own construction based on Elkins et al. (2009)
AgeConstitution2Q	AgeConstitution between 14 and 22 years.	Own construction based on Elkins et al. (2009)
AgeConstitution3Q	AgeConstitution between 23 and 33 years.	Own construction based on Elkins et al. (2009)
AgeConstitution4Q	AgeConstitution between 34 and 163 years.	Own construction based on Elkins et al. (2009)
AgeAmend	Number of years since the last amend was introduced to the Constitution in country c.	Own construction based on Elkins et al. (2009)
SharedPower	Yearly average across components of Table 3.A.1 that portray shared/ambiguous attributions between the executive and legislative in country c.	Own construction based on Bjørnskov and Voigt (2018)
Independent	Dummy=1 if country c is fully autonomous.	Own construction based on Elkins et al. (2009)
Speaker	Dummy=1 if LHLEAD of Table 3.A.1 is coded as 1.	Own construction based on Elkins et al. (2009)
GDPpc (log)	The logarithm of the average gross domestic product per capita within micro-region i. surrounding region i in year t. .	Kummu et al. (2018)
Population (log)	The logarithm of the number of people within micro-region i in year t.	Goldewijk et al. (2010, 2011)

Table 3.5 Descriptive Statistics

	N	Mean	SD	Min	Max
Light	3.654e+06	-7.004	4.066	-9.210	4.143
Light (IHS)	3.654e+06	0.322	0.808	0	4.836
LeaderBR	3.654e+06	0.000287	0.0169	0	1
PresidentialLeaderBR	3.654e+06	0.000920	0.0303	0	1
PastLeaderBR	3.654e+06	0.00186	0.0431	0	1
PastPresidentBR	3.654e+06	0.00108	0.0329	0	1
PastAllLeadersBR	3.654e+06	0.00270	0.0519	0	1
LeaderUpperHouseBR	3.654e+06	0.000162	0.0127	0	1
Experience	3.654e+06	0.000264	0.0323	0	12
Tenure	3.654e+06	0.000728	0.0513	0	7
Future1	3.654e+06	0.000178	0.0133	0	1
Past1	3.654e+06	0.000178	0.0133	0	1
Posttrend	3.654e+06	0.00113	0.0575	0	8
Pretrend	3.654e+06	0.00115	0.0570	0	8
WB Aid	3.737e+06	-9.195	0.586	-9.210	20.17
Aid China	3.737e+06	-9.210	0.103	-9.210	21.52
IPP	3.637e+06	0.376	0.0851	0	0.733
IPP1T	3.637e+06	0.282	0.450	0	1
IPP2T	3.637e+06	0.570	0.495	0	1
IPP3T	3.637e+06	0.147	0.354	0	1
AgeConstitution	3.637e+06	43.90	50.76	0	160
AgeConstitution1Q	3.637e+06	0.317	0.465	0	1
AgeConstitution2Q	3.637e+06	0.261	0.439	0	1
AgeConstitution3Q	3.637e+06	0.131	0.337	0	1
AgeConstitution4Q	3.637e+06	0.291	0.454	0	1
Ageamended	3.637e+06	2.220	4.312	0	58
SharedPower	3.637e+06	0.539	0.0727	0.182	0.909
Independent	3.637e+06	0.999	0.0310	0	1
Speaker	3.637e+06	0.611	0.487	0	1

Table 3.5 – Descriptive Statistics (continued)

	N	**mean**	**sd**	**min**	**max**
lhlegis	3.637e+06	0.999	0.0264	0	1
immunity	3.637e+06	0.0228	0.149	0	1
intexec	3.637e+06	0.981	0.137	0	1
invexe	3.637e+06	0.924	0.266	0	1
leg_in_5	3.637e+06	0.999	0.0274	0	1
legapp	3.637e+06	0.00338	0.0580	0	1
overwho	3.637e+06	0.0923	0.289	0	1
amndprop_4	3.637e+06	0.714	0.452	0	1
amndappr_4	3.637e+06	0.145	0.352	0	1
hospdiss_2	3.637e+06	0	0	0	0
hosadiss_2	3.637e+06	0	0	0	0
cabappt_3	3.637e+06	0.000241	0.0155	0	1
cabappr_3	3.637e+06	0.0193	0.138	0	1
cabdiss_3	3.637e+06	0.124	0.329	0	1
GDPpc (log)	3.533e+06	9.165	0.643	6.367	11.90
Population (log)	3.623e+06	4.514	2.707	0	14.23

a) Regions within 11 km in LAC

b) Regions within 11 km in grid

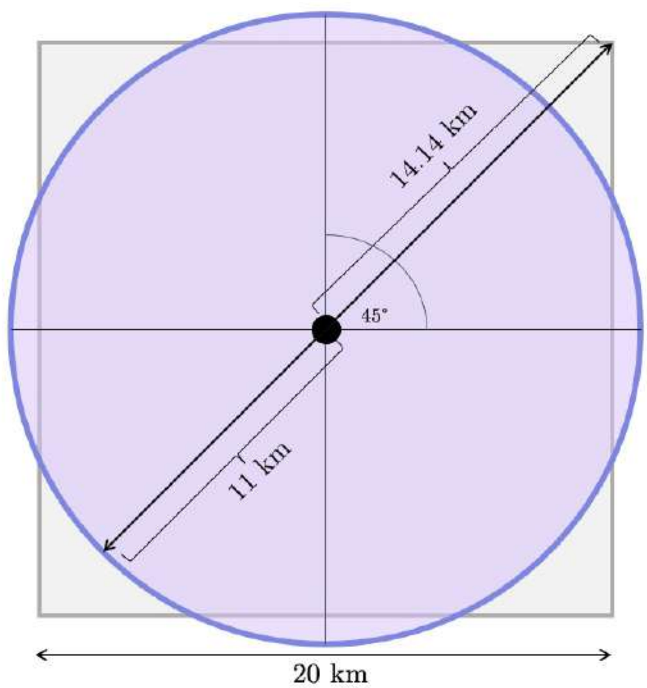


Figure 3.1 Regions within 11 km from leaders' birth regions

Notes: Both maps display regions within 11 km from leaders' birthplaces. The figure above shows an overview of the regions near leaders' birth regions in LAC. The figure below zooms in into one of these regions and illustrates the extension of the leaders' potential impact.

3.2 Additional Tables

Table 3.2.1 Leader effects on Economic Activity: without lagged dependent variable $Light_{t-1}$

		111 km			*55 km*			*28 km*			*11 km*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Light	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**	**Light**
$LeaderBR_{t-1}$	3.170***	1.377***	0.008	4.160***	1.929***	-0.031	5.563***	2.823***	-0.016	7.169***	3.738***	0.101*
	(0.443)	(0.228)	(0.029)	(0.423)	(0.229)	(0.035)	(0.429)	(0.247)	(0.048)	(0.407)	(0.345)	(0.059)
Observations	3,742,213	3,741,120	3,741,120	3,742,213	3,741,120	3,741,120	3,742,213	3,741,120	3,741,120	3,742,213	3,741,120	3,741,120
Adjusted R-squared	0.033	0.358	0.908	0.018	0.358	0.908	0.010	0.358	0.908	0.005	0.357	0.908
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
ADM1-Year FE	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES
Micro-Region FE	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES
Countries	45	45	45	45	45	45	45	45	45	45	45	45
Regions	183082	183030	183030	183082	183030	183030	183082	183030	183030	183082	183030	183030

Notes: The values on Light are on log form. All columns control for $PresidentialLeaderBR_{t-1}$. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

Table 3.2 Robustness: Median size of LAC cities

	(1) **Light 111km**	(2) **Light 55km**	(3) **Light 28km**
$LeaderBR_{t-1}$	0.003	-0.036	-0.032
	(0.025)	(0.036)	(0.048)
$LeaderBR_{t-1} \times SmallCities_i$	0.084*	0.107	0.113*
	(0.048)	(0.066)	(0.067)
Observations	3,653,558	3,653,558	3,653,558
Adjusted R-squared	0.920	0.920	0.920
Controls	YES	YES	YES
ADM1-Year FE	YES	YES	YES
Micro-Region FE	YES	YES	YES
Countries	45	45	45
Regions	183030	183030	183030

Notes: The values for Light are in log form. All columns control for $Light_{t-1}$ and $PresidentialLeaderBR_{t-1}$. Leader clustered standard errors in parentheses. P-value for $LeaderBR_{t-1} \times SmallCities_i$ in column 2, is 0.105. Significance levels denoted *** p<0.01, ** p<0.05, * p<0.1

Table 3.3 Other sources of influence I

	(1) **Light**	(2) **Light**	(3) **Light**	(4) **Light**	(5) **Light**
$LeaderBR_{t-1}$	0.043	2.756	-0.238	0.067	0.089
	(0.057)	(1.797)	(0.269)	(0.061)	(0.077)
$LeaderBR_{t-1} \times Speaker_{t-1}$	0.042				
	(0.079)				
$LeaderBR_{t-1} \times Independent_{t-1}$		-2.681			
		(1.798)			
$LeaderBR_{t-1} \times SharedPower_{t-1}$			0.597		
			(0.509)		
$LeaderBR_{t-1} \times AgeAmended_{t-1}$				0.005	
				(0.015)	
$LeaderBR_{t-1} \times Tenure_{t-1}$					-0.002
					(0.021)
Observations	3,637,000	3,637,334	3,637,000	3,637,334	3,653,558
Adjusted R-squared	0.920	0.920	0.920	0.920	0.920
Controls	YES	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES
Countries	38	39	38	39	45
Regions	182205	182221	182205	182221	183030

Notes: All specifications include a lagged night-light (log), and a lagged Presidential leader dummy as controls. The values for Light are in log form. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

Table 3.4 Other sources of influence II

	(1) **Light**	(2) **Light**	(3) **Light**	(4) **Light**	(5) **Light**	(6) **Light**	(7) **Light**	(8) **Light**	(9) **Light**
$LeaderBR_{t-1}$	0.117	0.159***	0.075*	0.039	-0.060	-0.002	0.075*	0.077*	0.013
	(0.090)	(0.057)	(0.042)	(0.056)	(0.089)	(0.061)	(0.041)	(0.042)	(0.050)
$LeaderBR_{t-1} \times intexec_{t-1}$	-0.043								
	(0.099)								
$LeaderBR_{t-1} \times invexe_{t-1}$		-0.129							
		(0.079)							
$LeaderBR_{t-1} \times legapp_{t-1}$			0.001						
			(0.044)						
$LeaderBR_{t-1} \times overwho_{t-1}$				0.084					
				(0.086)					
$LeaderBR_{t-1} \times amndprop\_4_{t-1}$					0.144				
					(0.100)				
$LeaderBR_{t-1} \times amndappr\_4_{t-1}$						0.149*			
						(0.085)			
$LeaderBR_{t-1} \times cabappt\_3_{t-1}$							0.025		
							(0.043)		
$LeaderBR_{t-1} \times cabappr\_3_{t-1}$								-0.035	
								(0.239)	
$LeaderBR_{t-1} \times cabdiss\_3_{t-1}$									0.172**
									(0.084)
Observations	3,637,000	3,637,000	3,637,000	3,637,000	3,637,000	3,637,000	3,637,000	3,637,000	3,637,000
Adjusted R-squared	0.920	0.920	0.920	0.920	0.920	0.920	0.920	0.920	0.920
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Countries	38	38	38	38	38	38	38	38	38
Regions	182205	182205	182205	182205	182205	182205	182205	182205	182205

Notes: When specified all specifications include a lagged night-light (log), and a lagged Presidential leader dummy as controls. The values for Light are in log form. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

Table 3.5 Alternative specifications

	(1) **No controls**	(3) **No President dummy**	(4) **Country SE**	(5) **ADM1-Year and Region SE**	(6) **Light IHS**
$LeaderBR_{t-1}$	0.103*	0.084**	0.083	0.083*	0.021*
	(0.060)	(0.042)	(0.050)	(0.049)	(0.012)
Observations	3,741,120	3,653,558	3,653,558	3,653,558	3,653,558
Adjusted R-squared	0.908	0.920	0.920	0.920	0.962
Controls	NO	YES	YES	YES	YES
ADM1-Year FE	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES
Countries	45	45	45	45	45
Regions	183030	183030	183030	183030	183030

Notes: When specified all specifications include a lagged night-light (log), and a lagged Presidential leader dummy as controls—with the exception of column 2. The values for Light are in log form. When not specified otherwise, computations show Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6 Heterogeneity tests

	(1) **Budget Management**	(2) **Public Sector Management**	(3) **Transparency and Corruption**	(4) **Women in parliament**	(5) **GDPpc**
$LeaderBR_{t-1}$	0.937	0.438	-0.377	0.096	0.163*
	(1.235)	(0.997)	(0.478)	(0.083)	(0.090)
$LeaderBR_{t-1} \times Quality$	-0.121	0.014			
	(0.323)	(0.337)			
$LeaderBR_{t-1} \times Corruption$			0.307		
			(0.234)		
$LeaderBR_{t-1} \times GDPpc$					-0.000
					(0.000)
$LeaderBR_{t-1} \times cL.ShareWomen$				-0.001	
				(0.004)	
Observations	102,852	102,852	102,852	2,552,846	3,524,325
Adjusted R-squared	0.896	0.896	0.896	0.927	0.920
Controls	YES	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES	YES
Micro-Region FE	YES	YES	YES	YES	YES
Countries	7	7	7	29	43
Regions	13462	13462	13462	174195	182211

Notes: All specifications look into the effects on night lights. All columns include a lagged night-light, and a lagged Presidential leader dummy as controls. The values for Light are in log form. Leader clustered standard errors in parentheses; significance levels denoted *** p<0.01, ** p<0.05, * p<0.1.

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